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PATRICIA

LEAVY



≡ The Oxford Handbook *of*
QUALITATIVE RESEARCH

The Oxford Handbook of
Qualitative Research

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This handbook is dedicated to the curious at heart.

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The *Oxford Library of Psychology*, a landmark series of handbooks, is published by Oxford University Press, one of the world's oldest and most highly respected publishers, with a tradition of publishing significant books in psychology. The ambitious goal of the *Oxford Library of Psychology* is nothing less than to span a vibrant, wide-ranging field and, in so doing, to fill a clear market need.

Encompassing a comprehensive set of handbooks, organized hierarchically, the *Library* incorporates volumes at different levels, each designed to meet a distinct need. At one level are a set of handbooks designed broadly to survey the major subfields of psychology; at another are numerous handbooks that cover important current focal research and scholarly areas of psychology in depth and detail. Planned as a reflection of the dynamism of psychology, the *Library* will grow and expand as psychology itself develops, thereby highlighting significant new research that will impact on the field. Adding to its accessibility and ease of use, the *Library* will be published in print and, later on, electronically.

The *Library* surveys psychology's principal subfields with a set of handbooks that capture the current status and future prospects of those major subdisciplines. This initial set includes handbooks of social and personality psychology, clinical psychology, counseling psychology, school psychology, educational psychology, industrial and organizational psychology, cognitive psychology, cognitive neuroscience, methods and measurements, history, neuropsychology, personality assessment, developmental psychology, and more. Each handbook undertakes to review one of psychology's major subdisciplines with breadth, comprehensiveness, and exemplary scholarship. In addition to these broadly conceived volumes, the *Library* also includes a large number of handbooks designed to explore in depth more specialized areas of scholarship and research, such as stress, health and coping, anxiety and related disorders, cognitive development, or child and adolescent assessment. In contrast to the broad coverage of the subfield handbooks, each of these latter volumes focuses on an especially productive, more highly focused line of scholarship and research. Whether at the broadest or most specific level, however, all of the *Library* handbooks offer synthetic coverage that reviews and evaluates the relevant past and present research and anticipates research in the future. Each handbook in the *Library* includes introductory and concluding chapters written by its editor to provide a roadmap to the handbook's table of contents and to offer informed anticipations of significant future developments in that field.

An undertaking of this scope calls for handbook editors and chapter authors who are established scholars in the areas about which they write. Many of the

nation's and world's most productive and best-respected psychologists have agreed to edit *Library* handbooks or write authoritative chapters in their areas of expertise.

For whom has the *Oxford Library of Psychology* been written? Because of its breadth, depth, and accessibility, the *Library* serves a diverse audience, including graduate students in psychology and their faculty mentors, scholars, researchers, and practitioners in psychology and related fields. All will find in the *Library* the information they seek on the subfield or focal area of psychology in which they work or are interested.

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In summary, the *Oxford Library of Psychology* will grow organically to provide a thoroughly informed perspective on the field of psychology, one that reflects both psychology's dynamism and its increasing interdisciplinarity. Once published electronically, the *Library* is also destined to become a uniquely valuable interactive tool, with extended search and browsing capabilities. As you begin to consult this handbook, we sincerely hope you will share our enthusiasm for the more than 500-year tradition of Oxford University Press for excellence, innovation, and quality, as exemplified by the *Oxford Library of Psychology*.

Peter E. Nathan
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ABOUT THE EDITOR

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Patricia Leavy is an independent scholar, novelist, and public speaker (formerly associate professor of sociology, founding director of gender studies, and chairperson of sociology and criminology at Stonehill College). She earned her PhD in sociology at Boston College. She has published thirteen nonfiction books, including the best-seller *Method Meets Art: Arts-Based Research Practice, Essentials of Transdisciplinary Research: Using Problem-Centered Methodologies, Fiction as Research Practice*, and *Oral History: Understanding Qualitative Research*. She has also published two novels, *American Circumstance* and Sense Publisher's top-selling title, *Low-Fat Love*. She is the editor for four book series with Oxford University Press and Sense Publishers. Frequently called on by the media, she has appeared on national television and radio, is regularly quoted by the news media, publishes op-eds, and is a blogger for *The Huffington Post*. She frequently makes presentations and keynote addresses at universities, as well as at national and international conferences. The New England Sociological Association named her the "2010 New England Sociologist of the Year," and she has been nominated for a lifetime achievement award by the International Congress of Qualitative Inquiry and a Special Achievement award by the American Creativity Association. For more information, please visit www.patricialeavy.com.

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PREFACE

The *Oxford Handbook of Qualitative Research* presents a comprehensive retrospective and prospective review of the field. Filled with robust examples from real-world research, ample discussion of the historical, theoretical, and methodological foundations of the field, as well as coverage of key issues like data collection, interpretation, writing, and assessment, *The Oxford Handbook of Qualitative Research* aims to be a valuable text for students, professors, and researchers.

In the interest of disclosure, it is necessary to note that the field of qualitative inquiry is so broad that no book, even a handbook, can cover everything, certainly not to all readers' satisfaction. Even common phrases like "the qualitative research community" are problematic as some may argue that there are multiple communities distinguished by location and/or the approaches they privilege. Nevertheless, I use the term frequently as an umbrella, meant to signify the work of all qualitative researchers who, even if not always connected to each other, are connected via the use of qualitative techniques. There is also the issue of content. Because a handbook cannot be all things to all people, I have approached it with practicality in mind. Sometimes those who write about research practice are so enmeshed within the debates in the community that the focus can become quite theoretical and esoteric. This handbook offers the "meat and potatoes" of the field, sprinkled with different condiments. If this were a cookbook, it would be comfort food with a few twists, not exotic meals you need to go to specialty food stores to shop for. The hope is that the handbook will be useful in the teaching of qualitative research to those with little to no background in the subject, while still providing substantive contributions to the field that will be of interest to even the most experienced researchers.

Introduction

Patricia Leavy¹

Abstract

This chapter serves as the introduction to the *Oxford Handbook of Qualitative Research*. The first half of the chapter responds to two questions. First, the chapter addresses the question: What is qualitative research? In answering the question, the chapter reviews the major elements of research: paradigm, ontology, epistemology (which together form the philosophical basis of research), genre, methods, theory, methodology (which operate at the level of praxis), ethics, values, and reflexivity (which merge the philosophical and praxis dimensions of research). Second, the chapter addresses the question: Who are qualitative researchers? Leavy explains qualitative research as a form of bricolage and qualitative researchers as bricoleurs. The remainder of the chapter reviews the contents of the handbook, providing a chapter by chapter summary.

Key Words: Qualitative research, paradigm, ontology, epistemology, genre, methods, theory, methodology, ethics, values, reflexivity

We shall not cease from exploration
and the end of all our exploring
will be to arrive where we started
and know the place for the first time.

– T. S. Eliot

I open the introduction to the *Oxford Handbook of Qualitative Research* with the preceding quote for two reasons. First, it captures the essence of qualitative inquiry as a way of understanding, describing, explaining, unraveling, illuminating, chronicling, and documenting social life—which includes attention to the everyday, to the mundane and ordinary, as much as the extraordinary. Qualitative research can involve the study of others, but also the self and the complex relationships between, within, and among people and groups, including our own entanglements. The second reason I have begun with this quote is because it opens Laurel Richardson’s book *Fields of Play: Constructing an Academic Life* (1997). This is one of my favorite

books, and, in it, Richardson expands the way we think of ourselves as researchers, writers, and knowers. What I intend to do by way of sharing this is to locate myself within the field and within this text—this is something that many qualitative researchers aim to do, in various ways. In qualitative research, we are not outside of our projects, but located and shifting within them. Qualitative research is an engaged way of building knowledge about the social world and human experience, and qualitative researchers are enmeshed in their projects.

What Is Qualitative Research?

Science is a conversation between rigor and imagination. (Abbott, 2004, p. 3)

Qualitative research is a way of learning about social reality. Qualitative approaches to research can be used across the disciplines to study a wide array of topics. In the social and behavioral sciences, these approaches to research are often used to explore, describe, or explain social phenomenon; unpack the meanings people ascribe to activities, situations, events, or artefacts; build a depth of understanding about some aspect of social life; build “thick descriptions” (see Clifford Geertz, 1973) of people in naturalistic settings; explore new or under-researched areas; or make micro–macro links (illuminate connections between individuals–groups and institutional and/or cultural contexts).

Qualitative research itself is an umbrella term for a rich array of research practices and products. Qualitative research is an expansive and continually evolving methodological field that encompasses a wide range of approaches to research, as well as multiple perspectives on the nature of research itself. It has been argued that qualitative research developed in an interdisciplinary, transdisciplinary, or counterdisciplinary field (Denzin & Lincoln, 1998; Jovanic, 2011; Lorenz, 2010). This approach to inquiry is unique, in part because of its philosophical and methodological diversity, as well as because of the value system guiding research practice.

The diversity of the qualitative landscape, as well as the gestalt of qualitative practice, is also partly attributable to the context in which qualitative research developed. Chapter 2 in this handbook looks much more fully at the historical development of qualitative research, but I would like to briefly note the period of growth in the 1960s and 1970s because it bears directly on the richness of contemporary qualitative practice.²

Although there were many pivotal works published prior to the 1960s, the social justice movements of the 1960s and 1970s—the civil rights, women’s, gay rights, and peace movements—culminated in major changes in the academic landscape, including the asking of new research questions and the reframing of many previously asked research questions and corresponding approaches to research. These movements in essence became sites for new ways of thinking and led to the critique of dominant methods of scientific practice, many of which relied on positivism (Jovanic, 2011). There was a drive to include people historically excluded from social research or included in ways that reinforced stereotypes and justified relations of oppression, and researchers became more cognizant of power within the research process (Hesse-Biber &

Leavy, 2011). A couple of decades later, interdisciplinary area studies that developed in the context of critique—women’s studies, African-American studies, Chicana/Chicano studies—began emerging across academic institutions.

Because of the sociohistorical conditions in which it developed, the qualitative tradition can be characterized by its multiplicity of approaches to research as well as by its focus on *the uses to which that research might be put*. In this vein, there is a social justice undercurrent to qualitative practice, one that may be implicit or explicit depending on the positioning and goals of the practitioner and the project at hand.

Many qualitative researchers define qualitative research by comparing it to quantitative research. I myself have done this. However, instead of describing what something is by explaining what it isn’t, I focus on a discussion of the qualitative tradition as understood on its own merits.³ One way of understanding qualitative research is by considering the key dimensions of any research practice and discussing them in terms of qualitative practice.

The Elements of Research

The main dimensions of research can be categorized under three general categories: philosophical, praxis, and ethics. The philosophical substructure of research consists of three elements: paradigm, ontology, and epistemology. At the level of praxis there are four key elements of research: genre, methods, theory, and methodology. The ethical compass (which combines philosophical and praxis dimensions) includes three main elements: ethics, values, and reflexivity (see Table 1.1).

Table 1.1 The elements of research

Philosophical	Paradigm
	Ontology
	Epistemology
Praxis	Genre
	Methods
	Theory
	Methodology
Ethics (Philosophical and Praxis)	Ethics
	Values
	Reflexivity

THE PHILOSOPHICAL SUBSTRUCTURE OF QUALITATIVE RESEARCH

A range of beliefs guide research practice—beliefs about how research should proceed, what can be known, who can be a knower, and how we come to know. Together, these beliefs form the philosophical substructure of research and inform all aspects of the research from topic selection to research design to the final representation and dissemination of the research findings and all phases in between.

A paradigm is a worldview through which knowledge is filtered (Kuhn, 1962). In other words, it is an overarching perspective that guides the research process. I think of paradigms as sunglasses, with different color lenses. When you put a pair on, it influences everything you see. Qualitative research is multiparadigmatic, with researchers working from different worldviews (such as post-positivism, interpretivism, and critical orientations), which makes it a highly diverse field of inquiry.

An ontology is a philosophical belief system about the nature of social reality, including what we can learn about this reality and how we can do so. In their classic definition, Egon Guba and Yvonna Lincoln explained the ontological question as: “What is the form and nature of reality and, therefore, what is there that can be known about it?” (1998, p. 201). Qualitative researchers adopt a perspective that suggests knowledge *building* is viewed as generative and process-oriented. The truth is not absolute and ready to be “discovered” by “objective” researchers, but rather it is contingent, contextual, and multiple (Saldaña, 2011). Subjectivity is acknowledged and valued. Objectivity may be redefined and achieved through the owning and disclosing of one’s values system, not disavowing it (Hesse-Biber & Leavy, 2011).

If the ontological question is “What can be known?” then the epistemological question is “Who can be a knower?” An epistemology is a philosophical belief system about how research proceeds as an embodied activity, how one embodies the role of researcher, and the relationship between the researcher and research participants (Guba & Lincoln, 1998; Harding, 1987; Hesse-Biber & Leavy, 2004; 2011). Qualitative researchers work from many different epistemological positions. Researchers may work individually or as a part of a team with their participants in the co-creation of knowledge. From this perspective, researchers are not considered neutral or objective in the traditional sense. Rather, researchers acknowledge how their personal, professional, and

political commitments influence all aspects of their research. Researchers are considered instruments in qualitative research (Bresler, 2005; Saldaña, 2011). Research participants are valued and positioned as knowledge bearers and co-creators. This position rejects a hierarchical structure between the researcher and research participants or the idea that the researcher is the sole authority.

Together, the ontological and epistemological belief systems guiding the research practice serves as the philosophical basis or substructure of any research practice (Hesse-Biber & Leavy, 2011). Although a researcher’s ontological and epistemological positions can vary across qualitative projects and may be influenced by a range of other factors, including theoretical and personal commitments, generally, qualitative researchers seek to build partial and contextualized truths in collaboration with their research participants or through reflexive engagement with their research texts.

PRAXIS: APPROACHES, METHODS, AND THEORIES IN ACTION

Praxis is the doing of research—the *practice* of research. Approaches, methods, and theories come into being during praxis, as researchers build projects and execute on them, often making adjustments along the way.

Genres of research are overarching categories for different ways of approaching research (Saldaña, 2011). Each genre lends itself to studying particular kinds of topics and includes a range of commonly used methods of data collection, analysis, and representation. Frequently used research genres include but are not limited to field research, interview, grounded theory, unobtrusive approaches, participatory research, community-based research, arts-based research, internet research, and multimethod and mixed-method approaches. This is not an exhaustive list. The genre within which a researcher works is motivated by a combination of factors, including the research topic, the research question(s), his or her methodological preferences and experiences, and the intended audience(s) for the research, as well as by a range of pragmatic considerations such as funding, time, and the researcher’s previous experience, skills, and personal preferences.

Research methods are tools for data collection. Research methods commonly used in qualitative practice include but are not limited to ethnography, autoethnography, duoethnography, narrative inquiry, in-depth interview, semistructured interview, focus group interview, oral history, document

analysis, content analysis, historical-comparative methods, poetic inquiry, audiovisual methods, visual methods, photo-voice, case study, multiple case study, discourse analysis, conversation analysis, daily diary research, program evaluation, ethnodrama, ethnotheatre, ethnocinema, play building, and fiction-based research. As you can see, qualitative researchers use a range of tools for data collection. Research methods are selected because they are the best tools to gather the data sought for a particular study. The selection of research methods should be made in conjunction with the research question(s) and purpose or objective. In other words, depending on the research topic and how the research questions are framed, as well as more pragmatic issues such as access to participants or textual/preexisting data sources, time, and practical skills, researchers are guided to particular methods.

Each genre discussed earlier lends itself to the use of particular methods. For example, the genre of arts-based research lends itself to the use of ethnodrama, ethnotheatre, ethnocinema, play building, fiction-based research, poetic inquiry, audiovisual methods, photo-voice, or visual methods. The genre of interview research lends itself to the use of in-depth interview, semistructured interview, focus group interview, or oral history. Of course, these genres are all more complicated in practice. For example, discourse analysis is a method that may be employed in an interview study, document analysis, or narrative inquiry. Furthermore, depending on the context in which one employs a method, such as narrative inquiry, one might view it as an arts-based approach, interview approach, way of doing autoethnography, or a method of analysis. The intent is not to confuse matters but, given how large and diffuse the field of qualitative research is and the variety of ways that methods can be creatively employed, it is important to understand that you may come across these terms conceptualized in various ways in the literature. One of the reasons that methods can be conceptualized and employed in many different ways is because qualitative researchers also draw on multiple theories.

A theory is an account of social reality that is grounded in empirical data but extends beyond that data. Numerous theoretical perspectives may guide the research process, including but not limited to post-positivism, interpretive, symbolic interactionism, dramaturgy, phenomenology, ethnomethodology, social constructionism, post-structuralism, post-modernism, feminism, intersectionality theory, queer theory, and critical race theory. This is

also not an exhaustive list and in most instances each of these theoretical perspectives are general categories for a range of more specific theories. A qualitative research study may also yield the development of a new theory. In these instances, theory develops inductively out of the research process. In other words, the study generates data out of which a theory is built—that theory is grounded in the empirical data from that study but extends beyond that data and can be applied to other situations.

A methodology is plan for how research will proceed—combining methods and theory. The methodology is what the researcher actually does once he or she has combined the different elements of research. The methodology is informed by the philosophical beliefs guiding the research, the selection of research methods, and the use of theory. One's attention to ethics and their corresponding values system also influences how a study is designed and how methods are employed. Although two studies may use the same research method—for instance, a focus group interview—the researchers' methodologies may be completely different. In other words, how they proceed with the research, based not only on their data collection tool but also on how they conceive of the use of that tool and thus structure the study, determines their methodology. The level of moderation and/or control a researcher exhibits during focus group interviews can vary greatly. Methodologies are not standardized nor are they typically etched in stone. Not only will methodological approaches to research vary across projects, but, even within a particular project, methodologies are often viewed as flexible and malleable. A qualitative researcher might adjust his or her methodology over the course of a project to facilitate new learning or new insights or to adapt to unanticipated challenges, obstacles, or opportunities. The malleability of qualitative methodologies is a strength of this approach to knowledge generation.

It is important to note that although I have reviewed methods for data collection as a part of methodology, there are also methods or strategies for qualitative data analysis, interpretation, representation, and dissemination of research findings. Similar to data collection tools and theories, these too are diverse, making the methodological possibilities rich.

ETHICS: BELIEFS AND PRACTICES

Ethics is an area that bridges the philosophical and praxis aspects of research. Ethics play a central role in any research practice. Typically, when we

think about ethics in social research, particularly when working with human subjects, we are referring to issues such as preventing harm to the people or settings involved in the study, avoiding exploitation of research participants (with added attention in the case of vulnerable populations), disclosure of the nature of the study and how the findings will be used, the voluntary nature of participation, and confidentiality. Additionally, qualitative researchers have an ethical obligation to carefully consider how research participants are portrayed and to act sensitively.

Additional ethical issues are linked to a researcher's ontological, epistemological, and practical imperatives, which together form a researcher's values system. For instance, the real-world value or public usefulness of the research, the inclusion of underrepresented populations, the treatment of anomalous or contradictory data, and the way that the research findings are distributed to relevant stakeholders—these issues are also connected to ethical practice.

Reflexivity is also a core concept in the qualitative community and refers to one's attention to how power and bias come to bear during all phases of the research. As D. Soyini Madison suggests, reflexivity is about "the politics of positionality" and acknowledging our power, privileges, and biases throughout the research process (2005, p. 6). The social justice imperative of many qualitative projects is a driver of reflexivity, as are critical and power-sensitive

theoretical traditions. I suggest reflexivity is both a philosophical perspective and a way of doing or acting within the context of research, from start to finish (see Table 1.2).

Given the wide range of approaches, tools, and values that guide qualitative research, it is a rich and evolving tradition with innumerable possibilities for knowledge building and knowledge sharing. Researchers can build, craft, or construct many different kinds of projects to study a nearly limitless range of topics. For these reasons, many consider qualitative research a craft or form of bricolage.

Who Are Qualitative Researchers?

We are all interpretive bricoleurs stuck in the present working against the past as we move into a politically charged and challenging future. (Norman K. Denzin, 2010, p. 15)

The qualitative researcher can be thought of as a bricoleur—someone who comfortably draws on multiple bodies of scholarship, methods, and theories to do her or his work. The term *bricoleur* is attributed to Levi-Strauss (1966); however, Denzin and Lincoln popularized applying the term to the work of qualitative researchers. Thomas A. Schwandt (2001) writes:

As a *bricoleur*, the qualitative inquirer is capable of donning multiple identities—researcher, scientist, artist, critic, and performer—and engaging in different kinds of *bricolage* that consist of particular

Table 1.2 Summary of key elements of research

Element	Philosophical or Praxis	Definition
Paradigm	Philosophical	Guiding worldview
Ontology	Philosophical	The nature of social reality and what can be known about it
Epistemology	Philosophical	The role of the researcher and researcher/participant relationship
Genres	Praxis	Categories of ways of approaching research
Methods	Praxis	Tools for data collection
Theory	Praxis	Account of social reality that extends beyond data
Methodology	Praxis	A plan for how research will proceed (combining methods, theory, and ethics)
Ethics	Philosophical and Praxis	How one engages with, informs, and protects participants
Values System	Philosophical and Praxis	Usefulness and distribution to the public, inclusion of underrepresented groups
Reflexivity	Philosophical and Praxis	Attention to power, bias, and researcher positionality

configurations of (or ways of relating) various fragments of inherited methodologies, methods, empirical materials, perspectives, understandings, ways of presentation, situated responsiveness, and so on into a coherent, reasoned approach to a research situation and problem. The *bricolage* appears to vary depending on one's allegiance to different notions of interpretation, understanding, representation, and so on drawn from various intellectual and practice traditions. (p. 20)

Qualitative researchers may draw on scientific, humanistic, artistic, and other disciplinary forms. In this regard, qualitative research can be viewed as a scholarly, practical, and creative pursuit. Researchers need to be able to think analytically, symbolically, imaginatively, and metaphorically (Saldaña, 2011). Moreover, projects often demand innovation, creativity, intuition, flexibility, and responsiveness (adapting to new learning or practical problems). This is a rigorous and often labor-intensive process. Qualitative research commonly requires working with others over an expanse of time and producing large amounts of data for analysis while also demanding sustained attention to ethics and values. It is also a creative process—allowing researchers to experiment, play, adapt, learn, and grow along the way.

Of course, pragmatic considerations come into play when designing a project: funding, time, access to needed participants or textual/preexisting data sources, and the researcher's previous experience, skills, and personal preferences. Unfortunately, qualitative researchers are more often limited by practical issues than by their imaginative capabilities.

Despite these challenges, qualitative research is also a deeply rewarding process that may result in new learning about topics of import, increased self-awareness, the forging of meaningful relationships between co-creators of knowledge, the production of public scholarship, and the impetus for social change.

The Contents of This Handbook

As noted in the preface, no handbook can be all things to all people. It's impossible to cover the entire field, and so I have approached the content with practicality in mind: what one learning about and/or embarking on qualitative research most needs to know, peppered with advanced material and prospective reviews intended to be of value to even the most experienced researchers.

Part 1 of this handbook, "The Qualitative Tradition," offers a historical review of the field. Specifically, Part 1 presents an overview of the

history of qualitative research in the social sciences and the ethical substructure of qualitative research practice.

In Chapter 2, "Historical Overview of Qualitative Research in the Social Sciences," Svend Brinkmann, Michael Hviid Jacobsen, and Søren Kristiansen provide a detailed history of qualitative research in the social sciences. As they note, this history is a complicated task because there is no agreed-upon version but rather a variety of perspectives. Accordingly, these authors present six histories of qualitative research: the conceptual history, the internal history, the marginalizing history, the repressed history, the social history, and the technological history. They also suggest that writing about history is necessarily tied up with writing about the future and thus conclude their contribution with a vision of the field. In Chapter 3, "The History of Historical-Comparative Methods in Sociology," Chares Demetriou and Victor Roudometof present an overview of the historical trajectory of comparative-historical sociology while considering the development of specific methodological approaches. Next is Anna Traianou's chapter, "The Centrality of Ethics in Qualitative Research." Attention to the ethical substructure of research is central to any qualitative practice and thus is given priority as the closing chapter in Part 1. Traianou details the main ethical issues in qualitative practice, bearing in mind the changing sociohistorical climate in which research is carried out.

Part 2 of this handbook, "Approaches to Qualitative Research," presents an array of philosophical approaches to qualitative research (all of which have implications for research praxis). Because qualitative research is a diverse tradition, it is impossible to adequately cover all of the approaches researchers may adopt. Nevertheless, Part 2 provides both an overview of the key approaches to qualitative research and detailed reviews of several commonly used approaches.

Part 2 opens with Renée Spencer, Julia M. Pryce, and Jill Walsh's chapter, "Philosophical Approaches to Qualitative Research," which provides a general view of the philosophical approaches that typically guide qualitative practice. They review post-positivism, constructivism, critical theory, feminism, and queer theory and offer a brief history of these approaches, considering the ontological, epistemological, and axiological assumptions on which they rest, and they detail some of their distinguishing features. They also identify three overarching, interrelated, and contested issues with which

the field is being confronted: retaining the rich diversity that has defined the field, the articulation of recognizable standards for qualitative research, and the commensurability of differing approaches.

After the overview in Chapter 5, we turn to in-depth treatments of specific approaches to qualitative research. In Chapter 6, “Applied Interpretive Approaches,” Sally E. Thorne turns to the applied world of qualitative practice. Thorne considers how many applied scholars have been departing from established method to articulate approaches better suited to the questions of the applied world. This chapter considers the evolving relationship between the methods and their disciplinary origins and current trends in the direction of the applied interpretive qualitative project. Interpretive description is used as a methodological case in point to illustrate the kinds of departures that applied approaches are taking from their theoretical roots as they begin to advance knowledge development within applied contexts.

Chapter 7, “The Grounded Theory Method” by Antony Bryant, reviews grounded theory, which, as Bryant notes, is itself a somewhat misleading term because it actually refers to a method that facilitates the development of new theoretical insights. Bryant’s suggestion about the complexity of the term itself is duly noted because this chapter could easily have been placed in Part 3 of this handbook. However, because grounded theory can be used in conjunction with more than one method of data collection, I have placed it in Part 2 as an approach to research. This chapter provides background information about the development of grounded theory as well as its main features, procedures outputs, and evaluation criteria.

The final three chapters in Part 2 tackle power-sensitive or social justice approaches to qualitative research that have emerged in the context of activist and scholarly work. In Chapter 8, “Feminist Qualitative Research: Toward Transformation of Science and Society,” Maureen C. McHugh offers an in-depth treatment of feminist qualitative research, described in terms of its purposes of addressing women’s lives, advocacy for women, analysis of gender oppression, and transformation of society. The chapter covers topics including the feminist critiques of social science research, the transformation of science from empiricism to post-modernism (including intersectionality and double consciousness), reflexivity, collaboration, power analysis, advocacy, validity, and voice. Several qualitative approaches to research are described in

relation to feminist research goals, with illustrations of feminist research. In Chapter 9, “Critical Approaches to Qualitative Research,” Kum-Kum Bhavnani, Peter Chua, and Dana Collins reflect on critical strategies in qualitative research and examine the meanings and debates associated with the term “critical.” The authors contrast liberal and dialectical notions and practices in relations to social analysis and qualitative research. The chapter also explores how critical social research may be synonymous with critical ethnography in relation to issues of power, positionality, representation, and the production of situated knowledges. It uses Bhavnani’s (1993) framework to draw on Dana Collins’s research as a specific case to suggest how the notion of the “critical” relates to ethnographic research practices: ensuring feminist and queer accountability, resisting reinscription, and integrating lived experience. In Chapter 10, “Decolonizing Research Practice: Indigenous Methodologies, Aboriginal Methods, and Knowledge/Knowing,” Mike Evans, Adrian Miller, Peter Hutchinson, and Carlene Dingwall review Indigenous approaches to research that are fundamentally rooted in the traditions and knowledge systems of Indigenous peoples themselves. The authors suggest Indigenous methodologies and methods have become both systems for generating knowledge and ways of responding to the processes of colonization. They describe two approaches drawn from the work of two Indigenous scholars with their communities in Australia and Canada. They hope this work leads not only to better, more pertinent research that is well disseminated but also to improvement in the situations of Indigenous communities and peoples.

The third section of this handbook, “Narrative Inquiry, Field Research, and Interview Methods,” provides chapters on a range of methods for collecting data directly from people (groups or individuals) or by systematically observing people engaged in activities in natural settings.

Part 3 begins with Chapter 11, “Practicing Narrative Inquiry,” by Arthur P. Bochner and Nicholas A. Riggs. Arguably, this is a chapter that could have appeared just as easily in Part 2 because narrative is as much an approach to research as a method, or in Part 4 because narrative inquiry can be employed in the context of text- or arts-based research, or even in Part 6 as an approach to analysis. This chapter focuses on the development of the turn toward narrative in the human sciences. The authors trace the rise of narrative inquiry as it evolved in the aftermath of the crisis of representation in the social

sciences, locating the explosion of interest in stories and storytelling in changing population demographics and the debunking of venerable notions about scientific knowledge. They show how narrative inquiry offered an opportunity to humanize the human sciences, placing people, meaning, and personal identity at the center of inquiry; inviting the development of reflexive, relational, dialogic, and interpretive methodologies; and drawing attention to the need to focus not only on the actual but also on the possible and the good. The chapter attempts to synthesize the changing methodological orientations of qualitative researchers associated with narrative inquiry, as well as their ethical commitments. In the second half of the chapter, the focus shifts to the divergent standpoints of small-story and big-story researchers; the differences between narrative analysis and narratives-under-analysis; and various narrative practices that seek to help people form better relationships, overcome oppressive canonical identities, amplify or reclaim moral agency, and cope better with contingencies and difficulties experienced over the course of life.

Chapter 12, “Ethnography,” by Anthony Kwame Harrison, presents a new take on a classic method of qualitative research. Embracing the trope of ethnography-as-narrative, this chapter uses the mythic story of Bronislaw Malinowski’s—the reputed “founding father” of the ethnographic approach—early career and fieldwork as a vehicle through which to explore key aspects of ethnography’s history and development into a distinct form of qualitative research. Through a series of intervallic steps—in and out of Malinowski’s path from Poland to the “Cambridge School” and eventually to the western Pacific—Harrison traces the legacy of ethnography to its current position as a critical, historically informed, and unfailingly evolving research endeavor. Harrison suggests that, as a method continually reflected on and revised, ethnography is boundless.

In Chapter 13, “The Purposes, Practices, and Principles of Autoethnographic Research,” Carolyn Ellis and Tony E. Adams define autoethnography according to their practice of the method, and they describe its history and emergence within qualitative social research and within psychology. They propose general guiding principles for those seeking to do autoethnography, such as using personal experience, acknowledging existing research, understanding and critiquing cultural experience, using insider knowledge, breaking silence, and maneuvering through pain, confusion, anger, and uncertainty.

They present autoethnography as a process and as a product, one that can take a variety of representational forms. After offering ways to evaluate and critique autoethnography, they conclude with a discussion of autoethnography as an orientation to the living of life and an approach that has the potential of making life better—for the writer, reader, participant, and larger culture.

Switching gears from generating data from one’s own experiences to interviewing others, the next three chapters detail different methods of interview. Chapter 14, “Unstructured and Semistructured Interviewing,” by Svend Brinkmann, provides an introduction to qualitative interviewing as a social practice with a cultural history. Issues addressed include different levels of structure, numbers of participants, media of interviewing, and also interviewer styles. A more detailed exposition of semistructured life world interviewing is offered, as Brinkmann suggests this is arguably the standard form of qualitative interviewing today. The next chapter is “Oral History Interviewing: Issues and Possibilities” by Valerie J. Janesick. As she explains, oral history resides in storytelling and involves the collection of stories, statements, and reminiscences of a person or persons who have firsthand knowledge of any number of experiences. Oral history offers qualitative researchers a way to capture the lived experiences of participants. The techniques of oral history may include interviews, document analysis, photographs, and video. Three major issues that emerge are those of social justice, arts-based approaches to oral history, and transdisciplinarity. Janesick notes that, in the current climate, there are endless possibilities in terms of using digital techniques for data presentation, data analysis, and dissemination. In Chapter 16, “Focus Group Research: Retrospect and Prospect,” by George Kamberelis and Greg Dimitriadis, we turn to a method of group interviewing. First, the authors highlight the historical origins, tensions, and continuities/discontinuities of focus group research. Second, they suggest that focus group research embodies three primary, related functions: an inquiry function, a pedagogical function, and a political function. Third, they explore issues including mitigating the researcher’s authority; disclosing the constitutive power of discourse; approximating the natural; filling in knowledge gaps and saturating understanding; drawing out complexity, nuance, and contradiction; disclosing eclipsed connections; and creating opportunities

for political activism. Fourth, they discuss contemporary threats to focus group work, and they conclude with what they see as new research frontiers for focus group research, especially in relation to new information technologies.

Part 3 concludes with Erica Tucker's chapter "Museum Studies" which, as an entire area of study, arguably could have been placed in other sections of the handbook (such as the next section on multimethod research). However, given that museum studies often involve ethnographic observations in natural settings, I conclude Part 3 with this chapter. Tucker reviews the major research methods used to study museums, including gallery analyses and interviews with museum visitors, professionals, and stakeholders, as well as ethnographic fieldwork. Drawing from a range of case studies conducted by museum practitioners, anthropologists, historians, and other museum studies scholars, the author explores how these qualitative methods can be adapted to the study of exhibits, programs, and museums as knowledge-generating institutions. Approaches to research design, data analyses, and representation are also examined.

The next section of the handbook, "Text, Arts-Based, and Internet Methods," considers how qualitative researchers work with nonliving data or through mediated forms. Although these methods are at times considered unobtrusive (because the data exist independent of the research; e.g., in the case of content analyzing newspapers), there are also many participatory approaches that are considered (such as participatory arts-based research).

Chapter 18, "Content Analysis," by Lindsay Prior, focuses on the ways in which content analysis can be used to investigate and describe interview and textual data. The author considers the method in both qualitative and quantitative social research. Examples of four different kinds of data are subjected to content analysis. Using a distinctive style of content analysis that calls on the notion of semantic networks, Prior shows how the method can be used either independently or in conjunction with other forms of inquiry (including various styles of discourse analysis) to analyze data and also how it can be used to verify and underpin claims that arise out of analysis. The chapter ends with an overview of the different ways in which the study of "content"—especially the study of document content—can be positioned in social scientific research projects.

Chapter 19, "Photography as a Research Method," by Gunilla Holm, reviews the development of

photography as a research method in social sciences. Holm describes the different types of photographs used, such as archival photographs, photographs taken by the researcher, and photographs taken by participants. The uses of different approaches to obtain photographs and issues of interest concerning each approach are presented. The most common approaches to analyze photographs, such as content analysis, discourse analysis, and ethnographic analysis, are described. Questions surrounding interpretation and ethical practice are also considered.

Chapter 20, "Arts-Based Research Practice: Merging Social Research and the Creative Arts," by Gioia Chilton and Patricia Leavy, offers an overview of the emerging genre of arts-based research (ABR). ABR adapts the tenets of the creative arts in social research in order to approach research questions in new ways, ask new questions, and make research findings publicly accessible, evocative, and engaged. The authors provide a retrospective and prospective overview of the field, including a review of some of the pioneers of ABR, methodological principles, robust examples of ABR within different artistic genres, assessment criteria, and the future of the field.

The final chapter in this section of the handbook is "Qualitative Approaches in Internet-Mediated Research: Opportunities, Issues, Possibilities" by Claire Hewson. Internet-mediated research (IMR) has grown expansively over the past decade, in both its scope and range of methodological possibilities and in its breadth of penetration across disciplines and research domains. However, the use of IMR approaches to support qualitative research has lagged behind its application in supporting quantitative methods. This chapter discusses the possibilities and scope for using IMR methods in qualitative research and considers some of the issues and debates that have led some qualitative researchers to be reluctant to consider this approach as a viable alternative to traditional offline methods. Hewson adopts an optimistic stance on the potential for qualitative IMR and outlines a range of possible methods and strategies, punctuated with examples of successful (as well as less successful) studies. The chapter also covers practical issues and offers a commentary on the possible future of IMR.

Part 5 of the handbook, "Multimethod, Mixed Method, and Participatory Designs," focuses on approaches to research that typically rely on the use of more than one method of data collection and/or the participation of nonacademic stakeholders. Several of the chapters in this section could easily

have been placed in Part 2 of the handbook because they can be viewed as “approaches” to research. Again, this illustrates how fluid the field of qualitative research is, with its overlaps in definitions and practice. Notwithstanding the suggestion that some of these chapters cover broad approaches to research, I have placed them in this section of the handbook because they generally involve the use of more than one method.

Chapter 22, “Case Study Research: In-Depth Understanding in Context,” by Helen R. Simons, explores case study as a major approach to research and evaluation. After first noting various contexts in which case studies are commonly used, the chapter focuses on case study research directly. Strengths and potential problematic issues are outlined, as are key phases of the process. The chapter emphasizes how important it is to design the case, to collect and interpret data in ways that highlight the qualitative, to have an ethical practice that values multiple perspectives and political interests, and to report creatively to facilitate use in policy making and practice. Finally, the chapter explores how to generalize from the singular case. Concluding questions center on the need to think more imaginatively about design and the range of methods and forms of reporting available to persuade audiences to value qualitative ways of knowing in case study research.

In Chapter 23, “Program Evaluation,” Paul R. Brandon and Anna L. Ah Sam offer a detailed overview of program evaluation situated in the historical context in which this practice has developed. The chapter includes discussion regarding the choice of methods, some of which are used primarily within evaluation approaches to conducting evaluation; the aspects of programs that evaluators typically address; the concept of value; the differences between evaluation and social science research; research on evaluation topics; and the major evaluation issues and concerns that have dominated discussion in the literature.

The following two chapters cover approaches to research that involve community participation. Chapter 24 “Community-Based Research: Understanding the Principles, Practices, Challenges, and Rationale,” by Margaret R. Boyd, reviews the inclusion of community members in research practice. This chapter is an introduction to the historical roots and subdivisions within community-based research (CBR) and discusses the core principles and skills useful when designing and working with community members in a collaborative, innovative, and transformative research partnership. The

rationale for working within this research paradigm is discussed as are the challenges researchers and practitioners face when conducting CBR. Boyd suggests CBR challenges the traditional research paradigm by recognizing that complex social problems must involve multiple stakeholders in the research process—not as subjects but as co-investigators and co-authors. It is an “orientation to inquiry” rather than a methodology and reflects a transdisciplinary paradigm by including academics from many different disciplines, community members, activists, and often students in all stages of the research process. As the scholarship and practice of this form of research has increased dramatically over the past twenty years, this chapter looks at both new and emerging issues, as well as at founding questions that continue to draw debate in the contemporary discourse. In Chapter 25, “Lineages: A Past, Present, and Future of Participatory Action Research,” Sarah Zeller-Berkman provides a historical overview of participatory action research (PAR). Like CBR, this is a social justice-oriented approach to research that transcends method but relies on a variety of qualitative methods. Zeller-Berkman writes that PAR in the twenty-first century asserts a democratization of who has the right to create knowledge, research social conditions, engage in participatory processes, and take action. People using PAR generally believe that knowledge has and will continue to be a source of power. Participatory research is an attempt to shift the balance of power back in favor of people who have historically been denied representational power.

The next chapter in the handbook covers the methodological work being done in the content area of disaster research.⁴ In “Qualitative Disaster Research,” Brenda D. Phillips provides an overview of the history of qualitative disaster research since the 1920s. Challenges associated with conducting disaster research, particularly field-based studies, are presented. The chapter also discusses ethical challenges related to homeland security and the emotional impacts of disaster research on humans. Sections then lay out issues specific to the life cycle of disasters (preparedness, response, mitigation, and recovery), data gathering techniques commonly used (interviews, documents, observations, visual data), and strategies for data analysis. A final section links efforts to strengthen the trustworthiness and credibility of qualitative research to disaster studies.

The final chapter in this section of the handbook covers mixed-methods research. In Chapter 27, “Conducting Mixed-Methods Research: Using

Dialectical Pluralism and Social Psychological Strategies,” R. Burke Johnson, Tony Onwuegbuzie, Susan Tucker, and Marjorie L. Icenogle first summarize the philosophy of dialectical pluralism (DP). Ontologically, DP views reality as plural and changing. Epistemologically, DP follows a dialectical, dialogical, hermeneutical approach to listening, interacting, and learning from “the other.” Theoretically, DP integrates concepts especially from Rawls (e.g., procedural justice, reasonable pluralism, overlapping consensus, realistic utopia), Dewey (e.g., deliberative democracy, community, inquiry, growth), and Habermas (e.g., communicative rationality, deliberative democracy, discourse ethics, knowledge, public sphere). From empirical research, the authors draw on concepts and findings from social psychological literatures such as conflict management, negotiation, small-group psychology, group counseling, group dynamics, political diplomacy, deliberative democracy, and workplace justice. Dialectical pluralism requires purposeful construction of teams that include multiple/different values and perspectives and stakeholders from the most disadvantaged affected groups. The group process operates from the position of equal power, the use of social psychological strategies, and the working toward win-win solutions.

Part 6 of the handbook, “Analysis, Interpretation, Representation, and Evaluation,” covers a range of topics, including the analysis and interpretation of qualitative data, writing up qualitative research, and issues pertaining to evaluation.

The first two chapters in this section review qualitative data analysis. Chapter 28, “Coding and Analysis Strategies,” by Johnny Saldaña, provides an overview of selected qualitative data analytic strategies, with a particular focus on codes and coding. Preparatory strategies for a qualitative research study and data management are first outlined. Six coding methods are then profiled using comparable interview data: process coding, in vivo coding, descriptive coding, values coding, dramaturgical coding, and versus coding. Strategies for constructing themes and assertions from the data then follow. Analytic memo writing is woven throughout the preceding as a method for generating additional analytic insight. Next, display- and arts-based strategies are provided, followed by recommended qualitative data analytic software programs and a discussion on verifying the researcher’s analytic findings. Chapter 29, “Computer-Assisted Analysis of Qualitative Research,” by Christina Silver and Ann F. Lewins, picks up on the discussion of qualitative

data analytic software programs (although it should be noted that this chapter also considers how technology can be used in data collection). Silver and Lewins focus on the current state of technological support for qualitative research practice. The chapter focuses on technology and how it assists three main aspects of qualitative research: data collection, preparation, and/or transcription; bibliographic management and systematic literature reviews; and data management and analysis. The main body of the chapter discusses the functionality, role, and implications of Computer Assisted Qualitative Data Analysis (CAQDAS) tools. Three recent trends in computer assistance are emphasized: support for visual analysis, support for mixed-methods approaches, and online solutions.

Moving from data analysis to interpretation, Chapter 30, “Interpretation Strategies: Appropriate Concepts,” by Allen Trent and Jeasik Cho, presents a wide range of concepts related to interpretation in qualitative research. The chapter examines the meaning and importance of interpretation in qualitative inquiry and explores the ways methodology, data, and the self/researcher as instrument interact and impact interpretive processes. Additionally, the chapter presents a series of strategies for qualitative researchers engaged in the process of interpretation. The chapter closes by presenting a framework for qualitative researchers designed to inform their interpretations. The framework includes attention to the key qualitative research concepts transparency, reflexivity, analysis, validity, evidence, and literature. Four questions frame the chapter: What is interpretation and why are interpretive strategies important in qualitative research? How do methodology, data, and the researcher/self impact interpretation in qualitative research? How do qualitative researchers engage in the process of interpretation? And, in what ways can a framework for interpretation strategies support qualitative researchers across multiple methodologies and paradigms?

Chapter 31, “Writing Up Qualitative Research,” by Jane Gilgun, provides guidelines for writing journal articles based on qualitative approaches. The guidelines are a part of the tradition of the Chicago School of Sociology and the author’s experience as an author and reviewer. The guidelines include understanding experiences in context, immersion, interpretations grounded in accounts of informants’ lived experiences, and conceiving of research as action-oriented. Gilgun suggests excellent write-ups have “grab”; that is, accounts that jump off the page and convey a sense of lived experiences. Although

most of the chapter addresses the writing of conventional research reports, there is some coverage of writing articles that report findings resulting from ethnographies, autoethnographies, performances, poetry, and photography and other media.

The final chapter in this section of the handbook, "Evaluating Qualitative Research," by Jeasik Cho and Allen Trent, addresses a wide range of theories and practices related to the evaluation of qualitative research (EQR). The authors present six categories of EQR: (1) a positivist category; (2) Lincoln and Guba's alternative category; (3) a "subtle-realist" category developed by Hammersley, Atkinson, and Seale; (4) a general EQR category; (5) a category of post-criteriology; and (6) a post-validity category. The authors offer several evaluation strategies for EQR by providing a variety of examples. They also discuss a path forward for EQR. They conclude with a holistic view of EQR needed to collectively construct/confront inner and outer challenges to qualitative paradigms in the twenty-first century.

The final section of the handbook, "Conclusion: Politics and The Public," offers some final thoughts about the politics of qualitative research, the importance of public scholarship, and the future of qualitative research in a transdisciplinary context.

In Chapter 33, "The Politics of Research," Michael D. Giardina and Joshua I. Newman critically interrogate the politics of research currently dominating US higher education, a politics that is shaped as much by theoretical and methodological questions and debates as it is by prevailing social, cultural, political, and economic forces. The authors' arguments are guided by four primary questions: How and to what do the cultural and political priorities of the free-marketized, corporate university impact, direct, or confound the conduct of research? How and to what extent does politics situate methodologies? How and to what extent is the research act impinged on by such particularities as institutional review boards, national funding councils, scholarly journals, and the promotion and tenure process? And, how and where do we as academics fit into this new research climate? Giardina and Newman also provide a series of practical recommendations for professors and students alike who seek to actively confront and challenge the academic-industrial complex.

The closing chapter, "A Brief Statement on the Public and the Future of Qualitative Research," offers some final comments about the future of qualitative research. I suggest that there is a widespread move from a disciplinary to a transdisciplinary research

structure in which problems of importance are at the center of research practices (see Leavy, 2011). Within this context, qualitative researchers are well positioned to advance because of their ability to develop responsive and flexible research designs and present their work in multiple formats. Furthermore, I note how the broader move toward public scholarship is propelling both the practice of qualitative research and the teaching of qualitative methods.

Notes

1. Thank you to Dr. Tony Adams for providing his thoughtful and most helpful feedback on an earlier draft of this chapter.
2. There is qualitative work that can be pointed to in the late 1800s and early 1900s. However, it was the use of ethnography and related methods in the 1920s by researchers at the University of Chicago who were primarily studying urbanization (popularly deemed "The Chicago School of Sociology") that prompted the use of qualitative methods in sociology departments around the United States. In the 1960s, the qualitative tradition fully emerged.
3. Chapter 2 of this handbook, "Historical Overview of Qualitative Research in the Social Sciences," by Brinkmann, Jacobsen, and Kristiansen, provides a rich discussion of the history of qualitative research in relation to quantitative research.
4. There has been little documentation of the methodological work done in this field and therefore this chapter represents a significant contribution to the literature on both qualitative research and disaster studies.

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PART

1

The Qualitative Tradition

Historical Overview of Qualitative Research in the Social Sciences

Svend Brinkmann, Michael Hviid Jacobsen, and Søren Kristiansen

Abstract

Qualitative research does not represent a monolithic, agreed-upon approach to research but is a vibrant and contested field with many contradictions and different perspectives. In order to respect the multivoicedness of qualitative research, we will approach its history in the plural—as a variety of *histories*. We will work polyvocally and focus on six histories of qualitative research, which are sometimes overlapping, sometimes in conflict, and sometimes even incommensurable. They can be considered as articulations of different discourses about the history of the field, which compete for researchers' attention. The six histories are: (1) the conceptual history of qualitative research, (2) the internal history of qualitative research, (3) the marginalizing history of qualitative research, (4) the repressed history of qualitative research, (5) the social history of qualitative research, and (6) the technological history of qualitative research.

Key Words: qualitative research, history of science, social history, phenomenology, hermeneutics, sociology, technology

History writing is not just about charting the past but also about prospects for the future. There is no doubt that one's way of depicting the past is greatly important for how the future will unfold. This holds for human history in general but is perhaps particularly true for a contested field such as qualitative research. For decades, especially in the years following the rise of positivist social science in the mid-twentieth century, qualitative research methods were considered of little value, and some even deemed them unscientific. Fortunately, this situation has been changing in recent years, and while disciplines such as social anthropology and communication studies have always been open to qualitative inquiry (and have even been built around them in the case of ethnography), disciplines in the health sciences and psychology are now rediscovering their roots in qualitative studies of human lives and social phenomena. Most social sciences such

as sociology and political science lie somewhere between an unproblematic acceptance of and mild hostility toward qualitative inquiry, with huge local differences concerning openness toward qualitative research.

In this chapter, we do not seek to articulate *the* history of qualitative research in the social sciences, as this could easily monopolize one interpretation of the past with unfortunate consequences for the future. Qualitative research does not represent a monolithic, once-and-for-all, agreed-upon approach to research but is a vibrant and contested field with many contradictions and different perspectives. In order to respect the multivoicedness of qualitative research and inquire into its past in a way that is more congenial to a qualitative stance, we will present a variety of *histories* (in the plural) of qualitative research in the social sciences. Some of these histories are quite well known to insiders of the field,

while others may be more surprising and perhaps even provocative. One thing to be avoided is writing the historical narratives as *Whig history*, presenting the development of qualitative research as necessarily progressing towards enlightenment and liberation. There is still a tendency among some qualitative researchers to present their methods of inquiry as inherently more humane and liberating than the “objectifying” measures of quantitative researchers. This, we find, is a myth—which sometimes goes by the name “qualitative ethicism” (Brinkmann & Kvale, 2005)—but it is a myth that may be understandable as qualitative researchers here and there feel marginalized and have been looking for solid arguments to justify their practices. The marginalization of qualitative research, however, is possibly itself another myth that we will challenge in the multiperspectival histories to be unfolded in this chapter.

History writing in any field presupposes that it is possible to delineate and delimit the field whose history one is interested in recounting. This is a significant problem in qualitative research, so this gives us one further reason to approach the matter in terms of histories in the plural. We are aware that interesting accounts of the historical development of qualitative research exist, such as Denzin and Lincoln’s useful depiction of the so-called “eight historical moments” in the development of qualitative research (Denzin & Lincoln, 2011). We believe, however, that there are too many separate qualitative histories in the different social science disciplines and too little overall cumulative development for us to dare attempt a grand narrative of the history of qualitative research.

To repeat our basic point, history writing is not just about the past but also about the present and the future. When one knows how something came to be, one will often know what it presently is, and one will have a powerful voice in determining how it will develop in the future. In what follows, we will work polyvocally and focus on six histories of qualitative research, which are sometimes overlapping, sometimes in conflict, and sometimes even incommensurable. They can be considered as articulations of different discourses about the history of the field, which compete for researchers’ attention. The six histories are: (1) the conceptual history of qualitative research, (2) the internal history of qualitative research, (3) the marginalizing history of qualitative research, (4) the repressed history of qualitative research, (5) the social history of qualitative research, and (6) the technological history of qualitative research.

Obviously, these histories represent *our* selection. They are not representative or exhaustive of

all possible histories about qualitative research, and others would undoubtedly have cut the historical cake differently. Therefore, ironically, this chapter with its preselected histories might itself become a subject of qualitative scrutiny. As in all qualitative research, it remains a fundamental premise that different aspects of reality are salient for different researchers, but as always, this should be considered a virtue rather than a vice. It enables us to celebrate the richness of a past that allows us to reflect upon it from so many different angles, giving us so many different interpretations. Not all histories, however, are given equal space in our account. With some of them, we tell a short story, perhaps offering a novel perspective, while with others, we recount a longer and more elaborated story. This goes in particular for the second internal history of qualitative research, concentrating in some detail on giants such as Husserl, Heidegger, Gadamer, Blumer, Goffman, and Garfinkel. We have been guided in our selection by an ambition to understand the development of qualitative research as more than a pure history of ideas. We will argue against this form of idealism, which looks at theories and paradigms in abstraction from broader social, cultural, political, and technological forces; and we will try to show that it has often been exactly such forces that have been pushing qualitative research forward (or, in some cases perhaps, backward). This, of course, should not be thought of as rendering qualitative research invalid, for no forms of research exist in a historical vacuum, but it should instead enable qualitative researchers now and in the future to understand the complexities of their practices better.

The Conceptual History of Qualitative Research

Our first history is a basic conceptual history of the term “qualitative research.” While the term itself is much younger than one should think, the adjective “qualitative” has a longer history. Medieval philosophers of scholasticism distinguished *qualia* (the qualities of things) from *quanta* (the quantities) hundreds of years ago, and, with modern philosophy from the seventeenth century onwards, empiricist philosophers like John Locke argued that there are different kinds of qualities: primary qualities were thought to be independent of observers and are for example extension, number, and solidity. Secondary qualities, on the other hand, were thought to be produced as effects in observers such as colors, tastes, and smells. Modern philosophers—those who worked in the post-medieval

world (Descartes, Locke, Hume, etc.)—confined the secondary qualities to the subjective mind, since the new natural scientists (Galileo, Newton) had seemingly demonstrated that objective reality is nothing but matter in motion. The book of nature is written in the language of mathematics, Galileo said, implying a metaphysics of quantities as the primary reality. A new subjective/objective dichotomy thus arose, relegating human experience and all the sounds, sights, and smells that we live with to the realm of the subjective. In many ways, today's qualitative researchers still struggle with this issue and are sometimes accused of being unscientific due to the significance of subjectivity in their endeavors, having inherited the problem of objectivity versus subjectivity in large parts from seventeenth century metaphysics.

Not all philosophers after Locke, or scientists after Galileo and Newton, were satisfied with the division of the world into “objective” primary qualities (that can be studied scientifically) and “subjective” secondary qualities. There is a great difference, Goethe would argue in 1810 in his *Theory of Colors*, between studying colors in terms of Newtonian optics and in terms of human experience, and although the latter cannot reasonably be reduced to the former, it does not mean that it is any less important or amenable to systematic scientific studies. As an example of a field of human experience, Goethe argued that our understanding of colors has suffered greatly from being understood in terms of mechanical optics (see Robinson, 2002, p. 10), and one can read his theory as an early *qualitative* study of the phenomenology of colors (see also Giorgi & Giorgi, 2008, for a reading of Goethe as a phenomenologist *avant la lettre*).

Moving from discussing the term “qualitative” to “qualitative *research*,” we may note that it was only quite late in the twentieth century that qualitative research became a self-defining field of inquiry, although researchers had been employing similar methodologies before. In his book on writing up qualitative research, Harry Wolcott (2009) reminds us that, “Prior to the past three or four decades, not much had been written about field methods” (p. 80), and, he continues, “As best I recall, the phrase ‘qualitative research’ was rarely (never?) heard in the 1960s. Of what had been written earlier, outside their respective academic disciplines, the same few references and the same few illustrative studies were cited almost to the exclusion of all others.” (p. 80). He mentions Bronislaw Malinowski's introduction to his 1922 classic *Argonauts of the Western*

Pacific and William F. Whyte's 1943 *Street Corner Society*, both of which were first and foremost ethnographies—and only secondarily methodologies treatises. Prior to around 1970, researchers in sociology and anthropology would look to such classics for inspiration rather than to specific methodological handbooks on “qualitative research.”

Wolcott's memories seem to be corroborated by a search in contemporary scientific databases. A general search in all databases of the Web of Knowledge, Science Citation Index Expanded (which contains articles that date back to 1899 from all sciences) reveals that the term “qualitative” was widely used from 1900 but *only* in natural sciences such as chemistry. Even today, qualitative analysis remains an important sub-discipline in chemistry (working with the analysis and classification of chemical compounds) alongside the quantitative sub-disciplines of this science. The first article that appears in a broad search is from 1900 and bears the title: “On the qualitative separation of nickel from cobalt by the action of ammonium hydroxide on the ferricyanides” by Browning and Hartwell. If one excludes the natural and technical sciences, then the term “qualitative” appears in early psychological papers—for example, “A qualitative analysis of tickling—Its relation to cutaneous and organic sensation,” published in 1908, and “Some qualitative aspects of bitonal complexes” from 1921, both appearing in the *American Journal of Psychology*. These texts belong to the psychology of perception and come quite close to physiology (or “psychophysiology” as it was called). The term “qualitative” in the early twentieth century was thus quite closely connected to natural science disciplines such as chemistry, physiology, and the psychology of perception and appeared much later in the social sciences as such. According to Karpatschof (2010), who has studied the emergence of qualitative methods within the social sciences, the term is hardly used until 1970, which is a kind of historical take-off point, after which there is an exponential growth in the discourse of qualitative methods in the social sciences. This has continued to the present day, and we have recently witnessed a veritable boom of qualitative research in the human and social sciences, which is not just seen in the output of research publications that employ qualitative methods, but especially in the numerous methodology books that are published every year. As an example, if one takes a look at most catalogues from academic publishing houses and scans the pages of new titles within disciplines such as sociology, the

amount of new qualitative research titles will often greatly outnumber the new titles within quantitative methodology.

The question then becomes: Why did a need arise around 1970 for qualitative research to define itself as such in the social sciences, often antagonistically in relation to what it is *not* (i.e. quantitative research)? Why at this particular point in time? After all, books employing interviewing and fieldwork had been published earlier in the twentieth century but without invoking the qualitative-quantitative binary. And why do we find in recent decades a need to overcome this distinction again, witnessed, for example, in the wave of so-called “mixed methods?” There are no simple answers to these questions, but it seems likely that the general growth in knowledge production in the latter half of the twentieth century, with a new “knowledge economy” and increased significance of techno-scientific knowledge, pushed researchers to identify with specific traditions of knowledge production. Karpatschof (2010) has argued that social anthropologists have always used qualitative methods because they have as a rule studied “traditional societies,” whereas sociologists have more often used quantitative methods because they have studied modern or “serialized” societies with demographics that easily lend themselves to quantitative studies. We may speculate that qualitative research gains in importance after 1970 with the emergence of postmodernity, signaling a new dynamic, multiperspectival, and emergent social complexity that cannot easily be captured with the use of quantitative methods (we return to this idea when we address the social history of qualitative research below). Also, with the disputes around positivism as a philosophy of science, which began in the middle of the century, a need arose to signal that one can work systematically and methodically without subscribing to the tenets of positivism, and here the term “qualitative research” came in handy. Another way of expressing this argument has been put forward by Jovanovic (2011), who has argued that in order to fully understand the emergence and development of qualitative approaches, one needs to put the historical trajectory of the quantitative–qualitative divide under scrutiny. As Jovanovic points out, qualitative research is much more than just methods, procedures, and techniques. It is in fact an entire worldview. Qualitative research thus may entail an understanding of human beings and the world that is fundamentally different from quantitative research and therefore “a plausible positioning of qualitative research in the history of

social sciences and in its social context requires a historical reconstruction of the processes by which the quantitative-qualitative distinction has become an intellectual as well as a social tool” (Jovanovic, 2011, p. 4). In conducting a reconstruction of the socio-historical processes that laid the grounds for the emergence of modern science—a process that is labeled “the quest for certainty”—Jovanovic illuminates some of the very important processes in both the emergence of qualitative research as well its re-emergence in the late 1960s and 1970s. All in all, it was seemingly a mix of political and philosophical discussions that would drive the development of qualitative research forward, as we will see further reflected in the different histories that follow.

The Internal History of Qualitative Research

There are many—at times conflicting—schools of thought, traditions, paradigms, and perspectives included under the heading of “qualitative research.” Moreover, it seems as if the realm of what is defined as “qualitative research” is constantly expanding (Flick, 2002). Telling the internal history of qualitative research means articulating how the history looks to dedicated qualitative researchers from inside the field. We will unfold this history by emphasizing three philosophical foundations of qualitative research: (1) the German tradition of *Verstehen* (Schleiermacher, Dilthey, Gadamer) leading to different hermeneutic perspectives such as Geertz in anthropology, (2) the phenomenological tradition (Husserl) leading to different phenomenological research methods, and finally (3) the North American traditions of pragmatism, Chicago sociology, Goffman’s dramaturgical approach, symbolic interactionism, and ethnomethodology that in different ways remain important to current concerns in the social sciences. We will also briefly address ethnographic fieldwork as an approach that cuts across most of the paradigmatic differences in qualitative inquiry.

Hermeneutics

Hermeneutics is the art of interpretation and thus fundamental to much if not all qualitative research. Originally, with Friedrich Schleiermacher (1768–1834), hermeneutics was developed as a methodology for interpreting texts, notably biblical texts (see Brinkmann, 2005). There was at the time a pressing need to find a way to understand the scriptures correctly. With Wilhelm Dilthey (1833–1911) in the late nineteenth century, hermeneutics was

extended to human life itself, conceived as an ongoing process of interpretation. Dilthey developed a descriptive psychology, an approach to understanding human life that was fundamentally different from how the natural sciences work. We *explain* nature through scientific activity, Dilthey said, but we have to *understand* human cultural and historical life. A life, as the hermeneutic philosopher Paul Ricoeur said a century after Dilthey, “is no more than a biological phenomenon as long as it has not been interpreted.” (Ricoeur, 1991, p. 28). And qualitative researchers are (or should be, according to the hermeneutic approach to human science) in the business of understanding the interpretations that already operate in people’s lives, individually and collectively, which is in effect to interpret a range of interpretations (as we touch upon below, sociologist Anthony Giddens once referred to this as one aspect of a “double hermeneutics”; 1976).

The dichotomization of *Erklären* and *Verstehen* has been very influential in separating quantitative from qualitative research, with the implication that explanation is about bringing individual observations under a general law (this is known as the covering law model of scientific work; see Hempel, 1942), while understanding is something more particularistic that rests on the specific qualitative features of the situation in which someone acts. There is a difference, for example, between explaining the movements of objects in space by invoking laws of nature as articulated in physics and understanding why someone decided to do something at a particular moment in that person’s life. In the latter case, Dilthey would say, we need to understand the particularities of that person’s life, and putative universal laws of human behavior are of little use. The situations and episodes studied by qualitative researchers are, like historical events, most often unique in the sense that they only happen once. For that reason, it is not possible to bring them under universal laws.

Martin Heidegger’s (1889–1976) *Being and Time* from the early twentieth century is often cited as the work that inaugurated a shift from Dilthey’s life hermeneutics to what Heidegger would call “ontological hermeneutics” (Heidegger, 1927). The question of Schleiermacher’s methodological hermeneutics had been, “How can we correctly understand the meaning of texts?” The epistemologically oriented hermeneutics from Dilthey had asked, “How can we understand our lives and other people?” But ontological hermeneutics—or “fundamental ontology” as Heidegger also called

it (p. 34)—prioritizes the question: “What is the mode of being of the entity who understands?” (Richardson, Fowers, & Guignon, 1999, p. 207). *Being and Time* aims to answer this question and can thus be said to be an interpretation of interpreting, or a philosophical anthropology, which has been formative in relation to much qualitative research in the hermeneutic tradition.

Heidegger’s name for the entity that understands is *Dasein*, and the being of *Dasein* is unlike the being of other entities in the universe. Physical entities such as molecules, tables, and chairs are things that have categorical ontological characteristics, whereas human beings, or *Dasein*, are *histories* or *events* and have what Heidegger called existentials as their ontological characteristics (Polkinghorne, 2004, pp. 73–74). These are *affectedness* (Befindlichkeit) (we always find ourselves “thrown” into situations where things already matter and affect us), *understanding* (Verstehen) (we can use the things and episodes we encounter in understanding the world), and *articulation* or *telling* (Rede) (we can to some extent articulate the meanings things have) (Dreyfus, 1991). In short, humans are creatures that are affected by what happens, can understand their worlds, and communicate with others, and together, these features can be said to comprise an interpretative qualitative stance in human and social science research.

For Heidegger and later hermeneuticists such as Hans-Georg Gadamer (1900–2002) and the contemporary philosopher Charles Taylor, understanding is not something we occasionally do—for example, by following certain procedures or rules. Rather, understanding is, from the hermeneutic perspective, the very condition of being human (Schwandt, 2000, p. 194). We always see things *as* something, human behavior *as* meaningful acts, letters in a book *as* conveying some meaningful narrative. In a sense, this is something we do, and hermeneutic writers argue that all such understanding is to be thought of as interpretation, and it is exactly this process that interpretative social science aims to engage in. To study culture is, in Clifford Geertz’ words, to study “a system of inherited conceptions expressed in symbolic forms by means of which men communicate, perpetuate, and develop their knowledge about and attitudes toward life” (Geertz, 1973, p. 89). When seeking to understand the cultural symbolic system, the qualitative researcher should engage in “thick description,” Geertz said, that captures the contextual features that render any individual action or event meaningful. The researcher interprets members

of a culture, who already operate with more or less implicit self-interpretations of their own actions. This, however, should not be understood as implying that people normally make some sort of mental act in interpreting the world. “Interpretation” here is not like the mental act of interpreting a poem, for example. It is not usually an explicit, reflective process, but rather, in the Heideggerian tradition, seen as something based on skilled, everyday modes of comportment (Polkinghorne, 2004; Packer, 2011). This also means that many hermeneutic qualitative researchers have been skeptic about “method” as the way to understanding other people (which is one goal of qualitative inquiry). Instead, they argue, we understand others by spending time with them and talking to them, and this cannot be put into strict methodological formulas.

The idea of *reflexivity*, which is central to much qualitative research, has also been articulated within hermeneutic philosophy. Interpretation depends on certain *pre-judices*, as Gadamer famously argued, without which no understanding would be possible (Gadamer, 1960). Knowledge of what others are doing and of what our own activities mean “always depend upon some background or context of other meanings, beliefs, values, practices, and so forth.” (Schwandt, 2000, p. 201). There are no fundamental “givens,” for all understanding depends on a larger horizon of non-thematized meanings. This horizon is what gives meaning to everyday life activities, and it is what we must engage with as we do qualitative inquiry—both as something that can break down and necessitate a process of inquiry, and as something that we can reflexively try to make explicit in an attempt to attain a level of objectivity (in the sense of objectivity about subjectivity). The latter is often referred to by qualitative methodologists as making one’s pre-understandings or pre-judices explicit. Gadamer said:

In fact history does not belong to us; we belong to it. Long before we understand ourselves through the process of self-examination, we understand ourselves in a self-evident way in the family, society, and state in which we live. The focus of subjectivity is a distorting mirror. The self-awareness of the individual life is only a flickering in the closed circuits of historical life. *That is why the prejudices of the individual, far more than his judgments, constitute the historical reality of his being.* [Gadamer, 1960, pp. 276–277]

Gadamer argues that this makes the condition of human and social science quite different from the

one we find in the natural sciences “where research penetrates more and more deeply into nature” (Gadamer, 1960, p. 284). In the human and social sciences, there can be no “object in itself” to be known (p. 285), for interpretation is an ongoing and open-ended process that continuously reconstitutes its object. The interpretations of social life offered by researchers in the human and social sciences are an important addition to the repertoire of human self-interpretation, and influential fields of description offered by human science, such as psychoanalysis, can even affect the way whole cultures interpret themselves. This means that “social theories do not simply mirror a reality independent of them; they define and form that reality and therefore can transform it by leading agents to articulate their practices in different ways” (Richardson, Fowers, & Guignon, 1999, p. 227). Like the pragmatists would say (see the section “North American Traditions” later in this chapter), social theories are tools that may affect and transform those agents and practices that are theorized. Thus, Giddens (1993, pp. 9–13) has used the term “double hermeneutics” to describe the idea that social science implies researchers interpreting the knowledge (or interpretations) of research participants and that the findings of social scientists (i.e., concepts and theories) continuously re-enter and reshape the social worlds that they describe. Others, such as Kenneth Gergen (2001) have conceptualized this as “generative theory,” thus connecting hermeneutic ideas with contemporary forms of social constructionism within qualitative inquiry.

In short, hermeneutics is one of the most important philosophical traditions to have informed qualitative inquiry. Denzin and Lincoln (2011, p. 13) simply refer to the many qualitative paradigms, ranging from constructivism and feminism to cultural studies and queer theory, as *interpretative* paradigms, thus stressing this legacy from hermeneutics.

Phenomenology

Phenomenology is, in one sense, a more specific philosophical tradition that informs qualitative inquiry, but, in another sense, it can be used in to encompass almost all forms of qualitative research. Phenomenology in the general sense is the study of *phenomena*—in other words, of the world as it appears to experiencing and acting human beings. A phenomenological approach will insist on taking human experience seriously, in whichever form it appears. According to Amedeo Giorgi, a leading phenomenological psychologist who concentrates

on phenomenology in the more specific sense, it is “the study of the structure, and the variations of structure, of the consciousness to which any thing, event, or person appears” (1975, p. 83).

As a philosophy, phenomenology was founded by Edmund Husserl around 1900 and further developed as an existential philosophy by Martin Heidegger (who was also counted among the hermeneuticists above), and then in an existential and dialectical direction by Jean-Paul Sartre and Maurice Merleau-Ponty. The subject matter of phenomenology began with consciousness and experience, and was expanded to include the human life world and to take account of the body and human action in historical contexts by Merleau-Ponty and Jean-Paul Sartre (see Kvale & Brinkmann, 2008, on which the following is based). The goal in Husserlian phenomenology was to arrive at an investigation of essences, or to describe the essential structures of human experience from a first person perspective. Phenomenology was then a strict descriptive philosophy, employing the technique of *reduction*, which means to suspend one’s judgment as to the existence or nonexistence of the content of an experience. The reduction is today often pictured as a “bracketing,” an attempt to place the common sense and scientific foreknowledge about the phenomena within parentheses in order to arrive at an unprejudiced description of the essence of the phenomena (Kvale & Brinkmann, 2008, p. 27). So, a phenomenologist can study the experience of any human phenomenon (e.g., the experience of guilt) without taking a stand on the issue whether the phenomenon is real, legitimate, or illusory (e.g., one can study guilt as an experienced phenomenon without discussing whether there is a reason to feel guilt in a given situation or whether it is correlated with this or that neurochemical process or physiological response). The subject’s experience is the important phenomenological reality.

The important concept of the life world eventually became central to Husserl. He introduced the concept in 1936 in *The Crisis of the European Sciences* (Husserl, 1954) to refer to the intersubjective and meaningful world in which humans conduct their lives and experience significant phenomena. It is a pre-reflective and pre-theorized world in which phenomena appear as meaningful before they become subject to theoretical analysis. If the whole range of experienced phenomena did *not* appear in the life world, there would be no reason to investigate them scientifically, for there would in a sense be nothing to investigate. For phenomenologists, there is thus a primacy of the life world as experienced, since this

is prior to the scientific theories we may formulate about it. This was well expressed by Merleau-Ponty:

All my knowledge of the world, even my scientific knowledge, is gained from my own particular point of view, or from some experience of the world without which the symbols of science would be meaningless. The whole universe of science is built upon the world as directly experienced, and if we want to subject science itself to rigorous scrutiny and arrive at a precise assessment of its meaning and scope, we must begin by re-awakening the basic experiences of the world of which science is the second order expression. [Merleau-Ponty, 1945, p. ix]

Using a metaphor, we can say that the sciences may give us maps, but the life world is the territory or the geography of our lives. Maps make sense only on the background of the territory, where human beings act and live, and should not be confused with it. Phenomenologists are not against scientific abstractions or “maps,” but they insist on the primacy of concrete qualitative descriptions of experience—of that which is prior to maps and analytic abstractions.

Today, phenomenological approaches have branched and proliferated in many directions within qualitative inquiry. There are specialized phenomenological approaches within psychology (Giorgi & Giorgi, 2003) and anthropology (Jackson, 1996), for example, and in sociology, phenomenology was introduced primarily through the writings of Alfred Schütz and later his students Peter L. Berger and Thomas Luckmann, whose approach heavily influenced some of the North American traditions mentioned in the following section.

North American Traditions

Apart from the characteristically Continental European traditions, a number of traditions developed on the North American continent during the twentieth century that in important ways supplemented, consolidated, and expanded the focus from hermeneutics and phenomenology. Many of these at the time novel, theoretical perspectives are still today very much alive on the American continent and elsewhere. These qualitatively inspired traditions that saw the light of day particularly in the US during the twentieth century are often described as “microsociology,” “social psychology,” or the “sociologies of everyday life” (see Jacobsen, 2009).

One of the most influential, significant, and lasting internal stories of qualitative research has its roots in the pragmatic philosophy that developed

on the North American continent in the latter part of the nineteenth century and which later spread also to the European continent. *Pragmatism* is a philosophical tradition that is concerned with the practical outcomes of human action and which is therefore also concerned with the use value of science and the practical evaluation of “truth.” Truth, for the pragmatists, is always something that finds its expression in practical circumstances (an instrumental view of truth) and thus is not a pre-established, fixed, substantial, or sedimented dimension of knowledge. Contrary to representationalist theories of science, pragmatism is distinctly non-representative; the purpose of scientific practice is not to represent reality as it is, but rather to allow humans to understand and control the world they are part of through knowledge. The key protagonists of pragmatism in the early years were Charles Sanders Peirce, William James, John Dewey, and George Herbert Mead. Each contributed in his own way to the development of pragmatism, not as a coherent whole, but rather as a new perspective on science, democracy, and education. Specifically, pragmatism supports an empirical—as opposed to a theoretical or scholastic—perspective on science. It is in the practical utility of knowledge that science ultimately stands its test. As James once insisted:

A pragmatist turns his back resolutely and once and for all upon a lot of inveterate habits dear to professional philosophers. He turns away from abstraction and insufficiency, from verbal solutions, from bad a priori reasons, from fixed principles, closed systems and pretended absolutes and origins. He turns towards concreteness and adequacy, towards facts, towards action and towards power. . . . It means the open air and possibilities of nature, as against dogma, artificiality and the pretence of finality in truth. (James, 1907, pp. 30–31)

Early on, pragmatists were particularly critical of the prevalence of behaviorist science, according to which human beings were seen as mechanically responding to stimuli from the outside. Instead, pragmatists proposed that humans are meaning-seeking subjects who communicate through the use of language and constantly engage in reflective interaction with others. According to pragmatic philosophers, human beings are therefore concerned with the situational, the practical, and the problem-solving dimensions of their lives. This also goes for social scientific endeavors. In his book *How We Think*, John Dewey developed a five-step research strategy or investigation procedure—sometimes also referred to as “abduction”

(according to Peirce as a supplement to the approaches of deduction and induction)—according to which the investigator follows five steps towards obtaining knowledge. First, there is the occurrence of an unresolved situational problem—practical or theoretical—which creates genuine doubt. Second, this is followed by a specification of the problem in which the investigator might also either systematically or more loosely collect data about the problem at hand. Third, the investigator—now equipped with a specification of the problem—by way of his creative imagination introduces a hypothesis or a supposition about how to solve the problem. Fourth, the proposed hypothesis is now being elaborated and compared to other possible solutions to the problem, and the investigator based on reasoning carefully considers the possible consequences of the proposed hypothesis. Finally, the hypothesis is put into practice through an experimental or empirical testing by which the investigator checks if the intended consequences occur according to expectations and whether the problem is solved or not (Dewey, 1910). This research strategy thus starts out with genuine puzzlement and ends with problem solving.

In general, pragmatists therefore have been concerned with what they term “practical reasoning”; they are thus preoccupied with knowledge that has some practical impact *in* and *on* the reality in which it is used. Knowledge is active, not passive. Without privileging any specific part of the methodological toolbox, with its emphasis on abductive procedures, pragmatism has proved very useful particularly in explorative qualitative research as a framework for practice- and problem-oriented investigation, and pragmatism has for instance inspired researchers working within the so-called “grounded theory” perspective (Glaser & Strauss, 1967)—in fact one of the first self-denoted and systematically described qualitative methodologies—in which the purpose is to create workable scientific knowledge that can be applied to daily life situations. In recent years, sociologists, philosophers, and others have begun to take up pragmatism after quite a few years of absence from the intellectual agenda. There is thus mentioning of a “revival of pragmatism” in the new millennium (Sandbothe, 2000) that, for example, is evident in the works of Richard Rorty and Richard Sennett, just as French sociologist Luc Boltanski and his colleagues have heralded a pragmatic turn within French social theory, and within German sociology Hans Joas has been one of the key exponents of pragmatist-inspired social science.

Pragmatism heavily influenced the founding of the discipline of sociology on the North American continent. The official “date of birth” of sociology is often regarded as the opening of the first sociology department at the University of Chicago in 1892. The *Chicago School of Sociology* during the first decades of the twentieth century was instrumental in developing the discipline in general and “members” such as Robert E. Park, Florian Znaniecki, and William I. Thomas were particularly prominent in advancing a specifically qualitative stance in sociology. As such, and due to their inspiration from pragmatism, the Chicago sociologists were not keen on theoretical refinement in itself, believing sociology should be an empirical science and not a scholastic endeavor. As Park said, “We don’t give a damn for logic here. We want to know what people do!” Knowing “what people do” thus became a trademark of the Chicago sociologists, and a range of empirical studies from the early twentieth century illustrate the prevalence of qualitative approaches and methods such as document analysis, interviews, and participant observation. The Chicago sociologists were keen to get out and study social life directly, often by use of participant observation. The purpose was to create conceptual apparatuses and theoretical ideas based on empirical material. Inspired by pragmatist notions about the use-value of science, Robert E. Park wanted sociology through empirical research to be part of public discussions, debates, and politics as a crucial part of modern democratic society. According to him, sociologists should leave the library and their offices and go out and “get the seat of their pants dirty in real research,” as he once told his students (Park in Lindner, 1996, p. 81). Moreover, some of the early Chicago sociologists—Jane Addams, for example—also pioneered social work and action research and wanted to use sociology to promote social reform. By using the city of Chicago—a city with a population size that increased tenfold in less than one hundred years—as an empirical laboratory for all sorts of investigations, the sociologists explored—and still explore—city life as a concrete environment for understanding more encompassing social changes and transformations. In general, the Chicago School has throughout the years been characterized by a distinct qualitative and ethnographic orientation, focusing on studying people in their natural surroundings (the city), being critical of non-empirical research and theory, and being driven by a desire to uncover and understand patterns of human interaction. As Martin Bulmer pinpointed:

[All the Chicago sociologists were] in some ways empiricists, keen upon the use of hypotheses and experimental verification... Axioms, postulates, rational deductions, ideas and ideals are all deemed valuable when they can be made to function in actual experience, in the course of which they meet with constant modification and improvement... All display the attitude of enquirers rather than of expositors of absolute knowledge; their most confident affirmations are expressed in a tone that shows that they do not regard them as final. [Bulmer 1984, p. 32]

Despite their preference for qualitative methods, Chicago sociologists have used any kind of material available for studying social life. Thus there are different strands within the Chicago School: the human ecology strand, the (dis)organization strand, the social psychology strand, and the action research strand used especially within social work. Each of these strands has prioritized different methodological approaches, theoretical understandings as well as different outcomes of research, but common to all has been an intense interest in qualitative empirical work. Some of the most prominent classic and today still-often-quoted studies conducted by Chicago sociologists during the early years were Harvey W. Zorbaugh’s *The Gold Coast and the Slum* (1929), Clifford R. Shaw’s *The Jack-Roller* (1930), Paul G. Cressey’s *The Taxi-Dance Hall* (1932) and *The Polish Peasant in Europe and America* (1918–1920) by William I. Thomas and Florian Znaniecki. Common to these otherwise methodologically different studies—respectively using participant observation, document analysis on letters and diaries, and interviewing and official statistics—were their interest in knowing what people do in particular situations and circumstances and to uncover the types of activities often taking place on the outskirts of society: deviance, crime, subcultures, and the like. In the first half of the twentieth century, Chicago sociology thus functioned as a pioneer in promoting a distinctly qualitative mentality that was later superseded by other institutions (Harvard and Columbia) and other methodological preferences but which is still today a vital force in American sociology.

Building on the insights from the early Chicago School of sociology (often referred to as the “first generation of Chicago Sociology”), several sociologists and social anthropologists—some of whom were themselves students of the early Chicagoans—during the 1940s and onwards began to develop the idea of *symbolic interactionism*, sometimes more

broadly described as *interactionism*. What began as a distinctly North American project later spread to the European continent. Some of the early proponents of symbolic interactionist social science with a strong emphasis on qualitative methods were Charles H. Cooley, Everett C. Hughes, Erving Goffman, Howard S. Becker, Herbert Blumer, and Norman K. Denzin—with Blumer responsible for originally coining the term “symbolic interactionism,” which he admitted was a “barbarous neologism” (Blumer, 1969). Symbolic interactionism often refers to the social philosophy of George Herbert Mead as the founding perspective, which was later developed, refined, and sociologized by others. Mead was a central force in the development of pragmatism. Symbolic interactionism is based on an understanding of social life in which human beings are seen as active, creative, and capable of communicating their definitions of situations and meanings to others. According to Blumer, there are three central tenets of symbolic interactionism: (1) humans act toward things on the basis of the meanings they that the things have for them, (2) the meaning of such things is derived from or arises out of the social interaction that one has with one’s fellows, and (3) these meanings are handled in and modified through an interpretive process used by the person in dealing with the things he encounters (Blumer 1969, p. 2). As is obvious from this, symbolic interactionists are concerned with how humans create meaning in their everyday lives and in how, as the term “symbolic interaction” indicates, this meaning is created and carved out through interaction with others and by use of various symbols to communicate meaning. As Blumer stated on the methodological stance of symbolic interactionism:

Symbolic interactionism is a down-to-earth approach to the scientific study of human group life and human conduct. Its empirical world is the natural world of such groups and conduct. It lodges its problems in this natural world, conducts its studies in it, and derives its interpretations from such naturalistic studies. If it wishes to study religious cult behavior it will go to actual religious cults and observe them carefully as they carry on their lives. If it wishes to study social movements it will trace carefully the career, the history, and the life experiences of actual movements. If it wishes to study drug use among adolescents it will go to the actual life of adolescents to observe and analyze such use. And similarly with respect to other matters that engage its attention. Its methodological stance,

accordingly, is that of direct examination of the empirical social world. [Blumer, 1969, p. 47]

Blumer argued for the development of “sensitizing concepts”—as opposed to “definitive concepts”—to capture social life theoretically; such concepts “gives the user a general sense of reference and guidance in approaching empirical instances” (Blumer, 1954, p.7). Symbolic interactionism does per definition not privilege any specific methods or research procedures—anything capable of capturing human meaning making through symbolic interaction in everyday life and capable of providing sensitizing concepts will do. However, historically, due to its close association with Chicago sociology, symbolic interactionists have primarily worked with a variety of qualitative methods and used these to discover, represent, and analyze the meaning-making processes involved in human interaction in a variety of contexts. Although a branch of symbolic interactionism under the auspices of Manford Kuhn began to develop at the University of Iowa (the “Iowa School” as opposed to the “Chicago School” of Blumer and others) that prioritized more positivistic aspirations and used quantitative methods and experimental research designs, symbolic interactionism is to a large degree associated specifically with qualitative research, privileging the careful observation (and particularly *participant* observation) of social life in concrete and often naturally occurring circumstances (Manis & Meltzer, 1978). Today, symbolic interactionism is still very much alive and kicking—through conferences, book series, and a journal devoted to studies in symbolic interaction—and is an active part of American sociology and elsewhere, although the originality and initially provocative ideas of the pioneering protagonists of symbolic interactionism have gradually waned throughout the years.

One of the main proponents of interactionism was Erving Goffman, who throughout his career, which started at the University of Chicago in the early 1950s, gradually developed a perspective to study the minutiae of social life that still today is one of the most quoted and used within contemporary social research. Goffman in many ways personified qualitative social science in the mid-twentieth century due to his particular topics of interest as well as his specific means of investigating them. Goffman’s main preoccupation throughout his career was to tease out the many miniscule and often overlooked rituals, norms, and behavioral expectations of the social situations of face-to-face interaction between people in public and private places—something

that at the time was often regarded with widespread skepticism by more rigorously oriented social researchers. This was indeed a time when the center of intellectual development and priority within the social sciences on the American continent had gradually shifted from the University of Chicago in the earlier parts of the twentieth century to Harvard University and Columbia University at mid-century with a concomitant shift from qualitative and particularly ethnographic methods to much more experimental, quantitative, and statistical methods. Not surprisingly, Goffman is often described as a maverick with his impressionistic and to some extent obscure approach to research methodology and ways of reporting his findings. Like one of his main sources of inspiration, Georg Simmel, Goffman keenly used the essay as a privileged means of communicating research findings, just as other literary devices such as sarcasm, irony, and metaphors were part and parcel of his methodological toolbox. Goffman was particularly critical of the use of many of the methods prevalent and valorized in sociology at his time. For instance, against the preference for statistical variable analysis and the privilege of quantitative methodology, he once stated:

The variables that emerge tend to be creatures of research designs that have no substance outside the room in which the apparatus and subjects are located, except perhaps briefly when a replication or a continuity is performed under sympathetic auspices and a full moon. Concepts are designed on the run in order to get on with setting things up so that trials can be performed and the effects of controlled variation of some kind or another measured. The work begins with the sentence “we hypothesize that . . .,” goes on from there to a full discussion of the biases and limits of the proposed design, reasons why these aren’t nullifying, and culminates in an appreciable number of satisfyingly significant correlations tending to confirm some of the hypotheses. As though the uncovering of social life were that simple. Fields of naturalistic study have not been uncovered through these methods. Concepts have not emerged that re-ordered our view of social activity. Understanding of ordinary behavior has not accumulated; distance has. [Goffman, 1971, pp. 20–21]

Instead, Goffman opted for an unmistakable and distinctive qualitative research strategy aimed at charting the contours and contents of the all too ordinary and ever-present but nevertheless

scientifically neglected events of everyday life. His work was characterized by an apparent methodological looseness that consciously and stylistically downplayed the importance of his own findings but which covered over the fact that his work actually uncovered heretofore empirically uncharted territory. Many of the titles of his books thus contained consciously diminutive subtitles such as “reports,” “essays,” or “microstudies” that gave the impression, however mistaken, that it should not be taken all too serious. Goffman willingly admitted on what others might have regarded as a dubious research strategy:

Obviously, many of these data are of doubtful worth, and my interpretations—especially some of them—may certainly be questionable, but I assume that a loose speculative approach to a fundamental area of conduct is better than a rigorous blindness to it. [Goffman 1963, p. 4]

In his work, Goffman relied heavily on all sorts of empirical material. He conducted interviews with housewives; he explored an island community through in-depth ethnography; he investigated the trials and tribulations of patient life at a psychiatric institution by way of covert participant observation; he performed the role as a dealer in a Las Vegas casino in order to document and tease out the gambling dimensions of human interaction; he listened to, recorded and analyzed radio programs; and he more or less freely used any kind of qualitative technique, official and unofficial, to access the bountiful richness of social life. Despite his reliance on a varied selection of empirical input (or what he termed “slices of social life”), throughout his career, Goffman gradually developed and refined a unique research methodology by way of various metaphors intended to capture and highlight specific features of everyday life interaction. Goffman’s perspective on qualitative research therefore is often referred to as “dramaturgy” because his main and most popular metaphors was the theatrical analogy in which he—in *The Presentation of Self in Everyday Life*—in detail described social interaction *as if* it was a performance made by actors on a scene (Goffman, 1959).

However, Goffman’s metaphorical cornucopia was much more than mere dramaturgy. He also invented and refined other metaphorical schemas: “The ritual metaphor” (looking at social life *as if* it was one big ceremonial event), “the game metaphor” (investigating social life *as if* it was inhabited by conmen and spies), and “the frame metaphor”

(concerned with showing how people always work towards defining and framing social situations in order to make them meaningful and understandable). All these different metaphors concentrated on the very same subject matter—patterns of human interaction, or, put in another way, social life at the micro level—and each metaphor spawned a spectacular number of analytical terms and sensitizing concepts, many of which today are household concepts in the social sciences (just think of “stigma,” “impression management,” “labeling,” or “framing”). Moreover, they served as useful devices in which to embed the aforementioned varied empirical material, thereby giving it shape, meaning, and substance. Goffman’s perspective later inspired new generations of sociologists in particular and qualitative researchers in general who have used him and his original methodology and colorful concepts to study a variety of conventional as well as new empirical domains such as tourist photography, mobile phone communication, and advertising (see, e.g., Jacobsen, 2010).

Ethnomethodology is another important tradition in the internal history of qualitative research that simultaneously builds on and extends the perspective provided by pragmatism, interactionism, and the dramaturgical work of Goffman. Like Goffman, ethnomethodologists take an interest in studying and unveiling the most miniscule realm of human interaction, and they rely on the collection of empirical data from a variety of sources in the development of their situationally oriented sociology. Ethnomethodology was initially a project masterminded by American sociologist Harold Garfinkel who in *Studies in Ethnomethodology* (1967) outlined the concern of ethnomethodology as the study of the “routine actions” and the often-unnoticed methods of meaning making used by people in everyday settings (hence the term *ethnomethodology* meaning “folk methods”). These routine activities and the continuously sense-making endeavors were part and parcel of the quotidian domain of everyday life (described by Garfinkel, in the characteristically obscure ethnomethodological terminology, as the “immortal ordinary society”) that rest on common-sense knowledge and practical rationality. Inspired by the phenomenological sociology of Alfred Schütz as well as to some extent also the functionalism of Talcott Parsons, Garfinkel concerned himself with a classic question in sociology: how is social order possible? But instead of proposing abstract or philosophical answers to this question or proposing “normative force” as the

main arbiter between people, Garfinkel—as a kind of “phenomenological empiricism” (Heap, 1980)—set out empirically to discover and document what people actually do whenever they encounter each other. True to the general pragmatist and interactionist perspective, ethnomethodologists rely on an image of human actors as knowledgeable individuals who through such activities as “indexicality,” the “etcetera principle” and “accounts,” in Ludwig Wittgenstein’s terminology, “know how to go on.” Social reality and social order are therefore not static or pre-given—rather they are the active outcome or “accomplishment” of actors’ local meaning making amidst sometimes bewildering, confusing, and chaotic situations. As Garfinkel stated on the purpose and procedures of ethnomethodology—phrased in typical tortuous ethnomethodological wording:

Ethnomethodological studies analyze everyday activities as members’ methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes, i.e. ‘accountable’, as organizations of commonplace everyday activities. The reflexivity of that phenomenon is a singular feature of practical actions, of practical circumstances, of common sense knowledge of social structures, and of practical sociological reasoning... I use the term ‘ethnomethodology’ to refer to the investigation of the rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life. [Garfinkel, 1967, p. vii, p. 11]

According to ethnomethodologists, there are many different methods available to tease out the situational and emerging order of social life that is based on members’ methods for making activities meaningful. Ethnomethodology is, however, predominantly a qualitative tradition that uses typical qualitative methods such as interviews and observation strategies for discovering and documenting what goes on when people encounter everyday life, but they also like to provoke our ingrained knowledge of what is going on. Thus, in classic Durkheimian-inspired fashion, one particularly opportune ethnomethodological way to find out what the norms and rules of social life really are and how they work is to break them. For example, Garfinkel invented the “breaching experiments” aimed at provoking a sense of disorder in the otherwise orderly everyday domain so as to see what people do to restore the lost sense of order. Of these “breaching experiments” or ‘incongruence

procedures”—that Garfinkel asked his students to perform—he wrote:

Procedurally it is my preference to start with familiar scenes and ask what can be done to make trouble. The operations that one would have to perform in order to multiply the senseless features of perceived environments; to produce and sustain bewilderment, consternation and confusion; to produce the socially structured affects of anxiety, shame, guilt and indignation; and to produce disorganized interaction should tell us something about how the structures of everyday activities are ordinarily and routinely produced and maintained [Garfinkel, 1967, pp. 37–38]

Garfinkel, his colleagues, and students throughout the years performed a range of interesting studies—of courtroom interaction, jurors’ deliberations, doctors’ clinical practices, transsexuals’ attempts at “passing” in everyday life, piano players’ development of skills and style, medical staffs’ pronunciation of patients’ deaths, police officers’ craft of peace keeping, pilots’ conversation in the cockpit —aimed at finding out how everyday life (and particularly work situations) is “ordinarily and routinely produced and maintained” by using breaching experiments, but also less provocative methods. Later, ethnomethodology bifurcated into a “conversation analysis” strand on the one hand and what has been termed “conventional ethnographical ethnomethodology” on the other. Common to both strands has been a concern with uncovering the most meticulous aspects of human interaction—non-verbal and verbal. Just as Garfinkel studied the natural patterns of interaction in natural settings (the living room, the courtroom, in the clinic or elsewhere), so conversation analysts studied natural language (but also professional jargon) as used by people in ordinary circumstances. For instance, conversation analysts, such as Harvey Sacks and Emanuel Schegloff, intimately studied and analyzed the minutiae of turn-takings, categorizations, and sequences of verbal communication in order to see how people through the use of language create meaning and a coherent sense of what is going on. Characteristic of both strands of ethnomethodology is the strong reliance on qualitative research methods aimed at capturing and detailed describing the situational and emerging character of social order. In fact, ethnomethodologists strongly oppose positivistic research procedures aimed at

producing universal “truths” or uncovering “general laws” about society and instead opt for a much more mundane approach to studying the locally produced orders and thoroughly episodic and situational character of social life (see, e.g., Cicourel, 1964). In a typical provocative respecification of Schütz’s classic dictum, Garfinkel thus suggested that we are all sociologists, because we constantly search for meaning. The means and methods of inquiry of professional sociologists are thus not all that different from the various ways ordinary people in everyday life observe, inquire, or talk to one another. This is a principle shared with the hermeneutic strand, which was addressed earlier.

Most of the North American traditions mentioned here can be covered by the label of “creative sociologies” (Morris, 1977) because they first of all regard human beings as creative actors capable of and concerned with creating meaning in their lives, and secondly because they emphasize creative qualitative approaches to capture and analyze those lives. As Monica B. Morris recapitulated on these creative sociologies:

The basic assumption underlying the ‘creative’ approaches to sociology are: that human beings are not merely acted *upon* by social facts or social forces; that they are constantly shaping and ‘creating’ their own social worlds in interaction with others; and that special methods are required for the study and understanding of these uniquely human processes. [Morris, 1977, p. 8]

These “special methods” have predominantly been varieties of qualitative methods. Common to most of the North American creative sociologies is also a distinct microsociological orientation aimed at mapping out and analyzing the distinctly quotidian dimensions of social life and society. Besides the various traditions that we have chosen to delineate as part of the internal story of qualitative research, we can also mention the important insights from social semiotics, existentialism, critical everyday life sociology, cultural studies, sociology of emotions, interpretive interactionism, and more recently actor-network theory that, however, will not be presented here.

A final tradition that can be mentioned, but which we will not analyze in detail here, is the tradition originating with structuralism in the first half of the twentieth century—the linguistics of Ferdinand de Saussure and the structural anthropology of Claude Lévi-Strauss, for example, which eventually

developed into post-structuralism in the latter half of the century in the hands of figures such as Michel Foucault, a French philosopher and historian of ideas, who is among the most referenced authors in the social science as a whole. Structuralism was based on the idea that language is a system of signs whose meaning is determined by the formal relations between the signs (and not with reference to “the world”) and post-structuralism pushed this idea further by arguing that the system is constantly moving and in flux, which is why, as Jacques Derrida (the leading exponent of deconstruction) would say, meaning is endlessly “deferred.” In relation to qualitative research, we should say that Foucault (and to a lesser extent Derrida) was a significant inspiration for many forms of discourse analysis, which today exist in many different variants. One variant is heavily inspired by Foucault and an awareness of power relations in social worlds (e.g., Arribas-Ayllon & Walkerdine, 2008), while Discursive Psychology as another is not closely associated with Foucault or post-structuralism, but originates in the aforementioned ethnomethodology and conversation analysis (Sacks, Schlegoff), which was mentioned earlier (see Peräkylä & Ruusuvuori, 2011).

Ethnography

Before concluding this internal history, it is appropriate with a note on the early trade of anthropological and sociological ethnography, which cuts across the different philosophical paradigms discussed previously. In anthropology, Bronislaw Malinowski, who held the first chair in social anthropology at the London School of Economics, is together with Franz Boas, one of the founders of American cultural anthropology, considered as the pioneers of ethnographic fieldwork. Contrary to the armchair anthropology and “anthropology of the verandah” conducted by earlier members of the discipline, and thus in a situation in which there was practically no professional discourse on field work practice and experiences, Malinowski insisted on and practiced fieldwork methods of the kind that is performed by today’s ethnographers. Conducting his famous study of the culture of the Trobriand Islanders, he stayed and lived among the natives for a period of almost three years. Inspired partly by psychologist Wilhelm Wundt, Malinowski conceptualized culture as a kind of toolbox containing the specific tools and means that people use in order to satisfy their needs. This functionalistic understanding had, of course, certain methodological implications. In order to obtain an adequate understanding

of the culture under scrutiny and the functional meaning of its various elements, Malinowski introduced at least three important principles that still appear among the most important requirements of anthropological fieldwork. First, the researcher should live in the community and among the people that are being studied; second, the researcher should learn the specific language of the community and not rely on interpreters who might add a distance between researcher and community; third, researchers should participate and observe at the same time (participant observation) (Kristiansen & Krogstrup 2012).

In contemporary textbooks on anthropological fieldwork methods, Malinowski’s study among the Trobriand Islanders is mentioned as a paradigm example, and generations of anthropological scholars have conducted fieldwork employing the principles laid out by Malinowski in the first decades of the twentieth century. And, as it has been indicated earlier in this chapter, anthropological fieldwork methods have been embraced by scholars from many other social science disciplines, especially sociology. The important point to be learned here is not necessarily the specific principles of ethnographic research per se, but the idea that ethnographic fieldwork should be considered among the important roots of qualitative research and thus that the development of ethnographic fieldwork by pioneers such as Malinowski and Boas in anthropology, and Robert E. Park, Ernest Burgess and Nels Anderson in (Chicago) sociology was triggered by a conception of the world as culturally pluralistic and diversified, which in turn called for the development and refinement of methods and procedures suited for grasping pluralities of the contemporary social world.

The Marginalizing History of Qualitative Research

After this tour de force through the internal history of qualitative research focusing on intellectual forerunners, theoretical paradigms, and methodological developments, let us turn to another way of describing the rise of qualitative research. It is difficult to understand current discussions in qualitative journals and handbooks without taking into account a widespread experience of not just studying the marginalized (something qualitative researchers often take pride in doing), but also of qualitative researchers themselves being marginalized as a research community. Several decades ago, Fritz Machlup (1956)

insisted that the social sciences as a whole suffered from an “inferiority complex” because the knowledge they could provide lacked the accuracy, law-like character, value-freedom and rigor of “real” science (such as natural science). Although this might be nothing less than a caricature of the social sciences in general and qualitative research in particular, perhaps qualitative sociologists, in this respect, may have suffered from an even more strongly felt inferiority complex than, for example, their colleagues working with statistics, surveys, or quantitative data analysis because qualitative sociology—almost per definition—has been seen by others and sometimes also by its own proponents as being opposed to the principles of “real science.” As Stephen Jay Gould once asked, “Why do we downgrade . . . integrative and qualitative ability, while we exalt analytical and quantitative achievement? Is one better, harder, more important than the other?” (Gould in Peshkin, 1993, p. 23). There is little doubt that during the decades in the mid-twentieth century, qualitative research lived a rather shadowy and marginalized existence and was regarded with some suspicion (Mottier, 2005). These were the decades of the “orthodox consensus” (Giddens, 1976) within the social sciences, relying heavily on positivistic research methods, a behaviorist image of man and a general functionalist theoretical foundation. Only later did we witness a revival or renaissance of qualitative research (Gobo, 2005). However, there is also little doubt that some qualitative researchers—for example, Goffman—consciously sought out such a marginalized position vis-à-vis prevailing positivistic research methods that in many ways not only gradually helped changing the game regarding the validity or applicability of certain research methods, but also made some qualitative researchers almost immune to critique from colleagues working within more quantitative or statistical traditions. As reported by Norman K. Denzin and Yvonna S. Lincoln, “qualitative researchers are called journalists or soft scientists. Their work is termed unscientific or only exploratory or subjective. It is called criticism, and not theory, or it is interpreted politically, as a disguised version of Marxism or secular humanism” (Denzin and Lincoln, 2011, p. 7). While there is some truth to this, we believe that much of the marginalization history of qualitative research is based on a myth. For example, the classical positivists, as Michell (2003) has recently demonstrated, were not against qualitative research, so

when qualitative researchers distance themselves from positivism, they most often construct a straw man and rarely, if ever, go back and read what early positivists such as Comte, Schlick, or Carnap in fact had to say about research and human experience.

Kvale and Brinkmann (2008) have even asked if the time has come to rehabilitate the classical positivists, perhaps in order for qualitative researchers to counter the marginality myth. It is noteworthy that August Comte (1798–1857) was responsible for founding both positivist philosophy and the science of sociology. His positivist philosophy reacted against religious dogma and metaphysical speculation and advocated a return to observable data. Émile Durkheim was another early sociologist who was influenced by positivist sociology and gave penetrating qualitative analyses of social phenomena. Positivism had in general a significant influence on culture and the arts of the nineteenth century, inspiring a move from mythological and aristocratic themes to a new realism, depicting in detail the lives of workers and the bourgeoisie (for some of this history, particularly in the British context, see Dale, 1989). In histories of music, Bizet’s opera *Carmen*, featuring the lives of cigarette smugglers and toradors, has been depicted as inspired by positivism, and Flaubert’s realistic descriptions of the life of Madame Bovary can likewise be considered as a positivist novel. Impressionist paintings sticking to the immediate sense impressions, in particular the sense data of pointillism also drew inspiration from positivism. The founder of phenomenological philosophy, Husserl, was even led to state that if positivism means being faithful to the phenomena, then we, the phenomenologists, are the true positivists!

It is no doubt true that many qualitative researchers have felt marginalized because of what they feel is a threat from the positivist philosophy of science. But if one goes back to Comte, and even to twentieth century “logical positivists” like Carnap and Schlick, one finds a surprisingly great methodological tolerance instead of the oft-insinuated hostility towards qualitative methods (Michell, 2003). The threat to qualitative methods has not come from a philosophy of science, but from research bureaucracies and funding agencies, witnessed, for example, in the recent movement towards “evidence based practice” in the professions, which impend on the possibilities of conducting qualitative studies. As we will argue in the next section with reference to Latour (2000), it seems clear that the natural sciences are full of qualitative studies, which is further

indication that qualitative researchers have no reason to feel inferior or marginalized in relation to their peers, who employ methods normally associated with the natural sciences.

The Repressed History of Qualitative Research

As we have seen in the internal history of qualitative research, in some disciplines such as sociology, qualitative approaches have been “out in the open” for decades and remain so today. However, for other disciplines the situation has been quite different, and it is this that we wish to highlight by briefly telling what we call the “repressed” history of qualitative research. This analysis pertains to psychology in particular, but it may also be relevant for other disciplines. Psychology was born as a science, it is said, in 1879 when Wilhelm Wundt established the first psychological laboratory in Leipzig. Wundt then began to conduct psychological experiments, but he also inaugurated the tradition of *Völkerpsychologie*, a cultural-historical approach of studying human life through customs, myths, and symbols, somewhat along the lines suggested by Dilthey in the hermeneutic tradition addressed above. So Wundt both initiated a tradition of experimental psychology, which has since become the mainstream approach, using quantitative measures, but also a long qualitative tradition in psychology. The qualitative tradition, however, has been forgotten by the official journals and handbooks of psychology to an extent that makes it resemble repression.

The case is that many “founding fathers” in psychology that today are not particularly associated with qualitative research in fact based their work on exactly that. It has likely been seen as embarrassing to textbook writers to include such figures as Freud and Piaget among qualitative researchers, since qualitative research has not figured among the respectable methods of the science of psychology. Psychology has been described by Sigmund Koch as unique among the sciences in having decided on its methods before defining its subject matter (see Robinson, 2001). Psychology has had, as its subject matter, something almost as elusive as the soul (i.e., the mind, which is an entity that psychologists have never been able to agree on). It has been defined as inner experience, outer behavior, information processing, brain functioning, a social construction, and many other things. But instead of agreeing on the subject matter of their discipline, the majority of psychologists have since the mid-twentieth century

constructed their science as a science of numbers in an attempt to emulate the natural sciences. There is something like a “physics envy” running through the history of psychology and related disciplines, which has implied an exorcism of qualitative research. The reader can try for herself to locate a standard textbook from psychology and check whether qualitative research is mentioned. The chance is very high that qualitative methods are not mentioned at all. Bruno Latour, an anthropologist who has actually entered into and observed research behavior in natural science laboratories, concludes laconically, “The imitation of the natural sciences by the social sciences has so far been a comedy of errors” (Latour, 2000, p.14). It is a comedy of errors chiefly because the natural sciences do not look at all like it is imagined in psychology and the social sciences. The natural sciences like physics, chemistry, biology, zoology, and geology are not built around statistics but often around careful qualitative descriptions of their subject matters. It can even be argued that such fields as paleontology rests on interpretative methods (Rorty, 1982). Anatomy and physiology are qualitative disciplines in large parts, describing the workings of the body, and it can—without stretching the concept too far—be argued that Darwin was a qualitative researcher, adept at observing and interpreting the natural world in its qualitative transformations.

If this analysis is valid, it means that qualitative research in psychology—as in most, if not all, human and social sciences—looks much more like natural science than is normally imagined and is much older than usually recognized. Here we can mention not just Wundt’s cultural psychology, but also William James’ study of religious experience, Freud’s investigations of dreams and his clinical method more broadly, Gestalt psychologists’ research on perception, Piaget’s interviews with children, Bartlett’s studies of remembering, and Merleau-Ponty’s phenomenology of the body. These are routinely addressed in psychological textbooks—after all they have all been formative of the discipline—but their *qualitative research methods* are almost always neglected or repressed. The history of interviewing as a qualitative research method is closely connected to the history of psychology (especially in its clinical and therapeutic variants), and some of this history is told in this book’s chapter on qualitative interviewing. Suffice it here to say that interviewing became a method in the human and social sciences with Freud’s psychoanalysis around 1900, and we refer the reader to the interview chapter for the details.

Although Freud's status as a theorist of the mind has been much debated in recent years, perhaps his main contribution—simultaneously using the conversation as a knowledge-producing instrument *and* as a “talking cure”—remains as relevant as ever. This makes it even stranger that Freud and the other psychological pioneers have been repressed as *qualitative* psychologists from the mainstream of the discipline. It is hard to imagine that psychology and similar sciences could have achieved their relatively high impact on society had they not employed what we call qualitative methods to zoom in on significant aspects of human and social life.

The Social History of Qualitative Research

Like all forms of social science, qualitative research exists in social, economic, cultural, and historical contexts, and must be understood in relation to these. Taking this as a point of departure, it makes good sense that qualitative research experienced a renaissance from the late 1960s onwards. On a basis of a somewhat Western-biased or ethnocentric worldview, the 1960s can be considered as a starting point for some major changes in life forms, social institutions, and the whole social fabric of society. As Gordana Jovanovic (2011) has argued, the legitimacy of some of the apparently solid social institutions such as the marriage and the family were questioned, and a more pluralistic and differentiated picture began to appear in terms of social groups, and new social movements making claims in favor of the environment, global peace, and women's and student's rights emerged. Together with the already existing critique of positivism and a universal rational method put forward by scholars such as Paul K. Feyerabend (1975), these changes, Jovanovic argues, spurred the belief that traditional natural science and causally oriented research models were inadequate in terms of studying and understanding these new forms of social life. Therefore there was a need to develop approaches that could uncover the meanings and nature of the unexpected and apparently provocative, disturbing, and oppositional social phenomena:

In these altered social circumstances, in which views concerning both science and the position of science had changed, it became possible to pose different research questions, to shift the focus of research interests, to redefine the research situation and the role of its participants—in a word, conditions were created for what histories of qualitative methods

usually describe as the ‘renaissance’ of qualitative research. [Jovanovic, 2011, p. 18]

In other words, changes in life forms, world views, and cultural practices were constituent of the re-emergence of qualitative research on the scientific scene in the 1960s and 1970s. And as we have touched upon earlier in this chapter (see section “The Internal History of Qualitative Research”), to some extent this re-emergence of qualitative research (at least among some of the early Chicagoans) has been associated with emancipation and with a practical use of social and human science knowledge in favor of underprivileged groups in society. Such history writing, however, unveils only one specific aspect of the interconnectiveness of qualitative research on the one hand and the social fabric on the other.

The social history of qualitative research has not yet been written, but it should also approach its development in another way, namely as deeply related to management and industrial organizations (cf. the famous Hawthorne study that involved interviews with thousands of workers with the aim of increasing productivity) and also advertisements and commercial research (focus groups, consumer interviews, etc.). From a Foucauldian perspective, qualitative research does not just spring from the countercultural and emancipatory movements of the 1960s and 1970s but may also have become part of the soft and hidden forms of power exertion in the confessional “interview society” (Atkinson & Silverman, 1997), and—contrary to its self-understanding—qualitative research may quite often function as a tool in the hands of the powerful (cf. the use of focus groups for marketing and political purposes).

As discussed by Brinkmann and Kvale (2005), the focus of the economies of Western societies has shifted from efficient production of goods to customers' consumption of the goods produced. What is important is no longer to make products as stable and unchanging as possible, but rather to make markets by influencing buyers through marketing. Henry Ford is supposed to have said that customers could get the Model T in any color they wanted, as long as they preferred black, but in today's post-Fordist economy, such standardization is clearly outdated. What is important today is not just the quality of the product, but especially its style, the story behind it, the experiences it generates, and what it reveals about the owner's self—in short, its hermeneutic qualities. Products are sold with inbuilt planned

obsolescence, and advertisements work to change customers and construct their desires continually in order for new products to find new markets. Softer, more concealed forms of power gradually replace the bureaucratic structures of industrial society with its visible hierarchies and governance through reward and punishment.

To begin writing the recent social history of qualitative research, we may note how, in consumer society, soft qualitative research has been added to the repertoire of social science methodologies, often superseding the bureaucratic forms of data collection in standardized surveys and quantitative experiments. While a textbook on quantitative methodology may read like a manual for administrators and engineers, qualitative guidebooks read more like manuals for personnel counselors and advertisers, stressing emotions, empathy, and relationships. Although qualitative methods are often pictured as progressive and even emancipatory, we should not overlook the immersion of these methods in a consumer society, with its sensitivity towards experiences, images, feelings, and lifestyles of the consumers (Kvale, 2008). The qualitative interview, for example, provides important knowledge for manipulating consumers' desires and behavior through psychologically sophisticated advertising. One of the most significant methods of marketing in consumer society is—not surprisingly—qualitative market research. More than a decade ago, it already accounted for \$2 billion to \$3 billion worldwide (Imms & Ereaut, 2002), and according to one estimate, 5 percent of all British adults have taken part in market research focus groups. Although a major part of qualitative interviewing today takes place within market research, the extensive use of qualitative research interviews for consumer manipulation is hardly taken into account in the many discussions of qualitative research and its emancipatory nature.

Concluding on the sketchy social history of qualitative research, we may return to the sociology department at University of Chicago, which has been mentioned already as an important institution in terms of nurturing qualitative research in a variety of forms from the late 1920s. We have not, however, reflected on the socio-historical conditions that might explain why the emergence of qualitative research approaches emerged exactly here at this specific time. In our view, there seem to be a least two answers to this admittedly complex question. First, the sociology department was initially uniquely crowded with intellectuals who

were influenced by pragmatic and interactionist thought, by Continental (particularly German) thinking stressing description and understanding before causal explanation, and also by journalism, by ecology models, and essayistic writing. At the same time, there was a strong spirit of wanting to link sociological research with engagement in social issues and social reform (Abbott, 1999; Bulmer, 1984). Second, the early Chicagoans' initial interest in immigrants, patterns of urban development, crime, and the general social dynamics of city life stimulated scholars such as Thomas, Znaniecki, Park, and Burgess to develop and employ research strategies that were different from the quantitative ones (see Jørgensen & Smith, 2009). One might say that the study of the complexity of new city life craved methodological considerations and research strategies that made the qualitative perspective come in handy. Thus in order to understand and grasp the cultural complexities of immigrant communities, the social worlds of marginalized people, and the segmentation of cities in distinctive zones, these researchers were somehow bound to employ and advance qualitative methods and techniques such as biographical research, fieldwork, and mapping.

The Technological History of Qualitative Research

Qualitative research indeed depends on human beings observing, interacting with, and talking to each other, but its history has also been driven by technological developments. It is difficult to imagine qualitative research as we know it today without the invention of the portable tape recorder, and later digital recording devices, and also the whole range of software that enables computer-assisted analyses of qualitative materials. The development of these technologies has created new opportunities and possibilities for researchers in regard to collecting, managing, and analyzing qualitative data (Schwandt, 2001, p. 27). However, not only have qualitative researchers adopted and made direct use of different technological devices in the research process, technological advances have also spurred new qualitative approaches and methods. The technological history of qualitative research, we contend, is thus a history of researchers making use of technological artifacts not specifically or purposely developed for qualitative research, of revising their methods in response to technological innovation, and of the development of technologies specifically designed for research purposes. We briefly summarize this technological

history by examining the ways that technological innovations have transformed and developed both the collection and the analysis of qualitative data.

Data Collection

Just as technological inventions have affected the general history of mankind in a variety of ways, technological innovations have triggered a number of major changes or shifts in the history of qualitative research and methodology. The first, and admittedly trivial, technologically driven shift was brought about by advances in transportation technology. In the very early days of anthropology (i.e., before Malinowski's groundbreaking works in the Trobriand Islands), anthropologists typically relied on secondhand materials gathered by others such as documents, travel logs, and reports written by colonial officials, missionaries, participants in scientific expeditions, or travelling salesmen. Unsurprisingly, this production of knowledge about cultures and social groups (later known as "armchair research") without ever meeting or interacting with them has later been criticized for lacking authenticity and thus for drawing conclusions on the basis of insufficient or inadequate data (Markle, West, & Rich, 2011). However, as transportation technology improved and made long-distance travelling easier and affordable, anthropologists began to travel around the globe and to practice what has become known as fieldwork, thus immersing themselves in the lives of the people under study, interacting with them, and taking part in their practices and producing data on site. In some cases, these traveling scholars brought with them new technologies such as travel typewriters and typed field notes while staying in the field. At this early stage of qualitative research, qualitative researchers invested massive energy in recording data. Researchers conducting interviews or doing observations often made handwritten summaries of interviews or conversations or wrote detailed field notes in their notebooks. At this point, a great deal of the researcher's work consisted of making records of her experiences in the field, or simply to produce data and make them storable.

This situation was dramatically changed by the invention and use of audio recorders. The introduction of these devices in the practice of qualitative research also constitutes a substantial methodological advance since they made it possible for researchers to collect and record information from observations or from interviews simultaneously. Being able to record information as an integrated part of the data-gathering process enabled researchers to collect

larger piles of data and to dedicate more efforts to the process of analysis. Furthermore, the fact that researchers could record conversations with participants, have them transcribed, and thus be able to return to them *as they actually appeared* constituted a major methodological progress. The process of making transcripts, and the following reading and re-reading, enabled the researcher to familiarize herself with the data in a completely new way (Gibbs, Friese, & Mangabeira, 2002). The making of transcripts gradually has become conceived of as an integral part of the qualitative research process since the intense listening to recordings makes the researcher aware of subtle and taken-for-granted dimensions in the participant's talk that researchers without recordings "would routinely fail to notice, fail to remember, or be unable to record in a sufficient detail by taking hand-written notes as it happened" (Rapley, 2007, p. 50).

In a somewhat similar way, photographic technology has had an impact on qualitative research. The use of photography as an aspect of qualitative research goes back to the early works of Gregory Bateson and Margaret Mead and their photographic ethnography of Balinese character (1942). Bateson and Mead's photographic report has achieved a landmark status among anthropologists and, although their innovative work was greeted with some puzzlement (Jacknis, 1988), the use of photographs has become popular not only within a special branch of anthropology but among a much broader community of qualitative researchers working within the field of visual methods (see Collier & Collier, 1986; Harper, 2012; Pink, 2007).

Still another shift was brought about by video recording and analysis (Gibbs, Friese & Mangabeira, 2002). Digital technologies have opened up new ways of collecting, managing, and analyzing qualitative data. The use of video recordings have been employed within a broad field of qualitative studies, and since it allows the researcher to re-observe situations over and over again and thus discover new facets and aspects of their structure and processes, this technology appears among the standard data-collecting techniques in qualitative research. The most recent qualitative methodological innovations have been catalyzed by the development of the Internet. Not only has the Internet made it possible to collect data in new ways, it has also created new forms of sociability, which in turn have catalyzed the development of existing qualitative methods.

The E-Interview represents one such example of how modern information and communication

technology have spurred innovative data collecting processes. E-Interviewing may be found in a variety of forms, but basically it entails a researcher and a research participant (or a group of participants) communicating through a sequence of e-mails involving questions and answers. As such, E-Interviewing appears similar to conventional e-mail communication and thus is quite different from face-to-face interviewing, where interviewer and interviewee interact directly in a real-time social encounter. Obviously (and to some extent similar to telephone interviewing, which of course is another technologically facilitated data-collection technique), such Internet-based data collection has some advantages: it is cost-effective since it eliminates travel and transcription expenses, it makes it possible to interview people who would not have accepted to participate in a face-to-face interview, and it may provide opportunities for accessing data that would have been difficult to obtain through direct face-to-face interaction (Bampton & Cowton, 2002). Thus some qualitative researchers, such as Holge-Hazelton (2002), have found that, in researching sensitive and personal topics using E-Interviews, there was a remarkable lack of inhibition among participants as rapport was quite easy to establish. On the other hand, being a distanced, asynchronous form of interaction, the E-Interview provides no access to the non-verbal and tacit signs that are highly valuable in terms of managing the interview process and thus in improving the quality of data collecting (Bampton & Cowton, 2002).

Whereas technological innovations and new devices have been adapted by social scientists, thereby facilitating the use of well-established research strategies and methods, technological inventions do, of course, also lead to or mediate new forms of social life, which in turn may call for a rethinking of common textbook methods. One illustrating example is found within ethnography. Traditional ethnographic techniques cover a variety of procedures that may assist the researcher in her face-to-face dealings with people, be it individual human beings or groups of people. However, as more and more social interactions unfold on the Internet or are otherwise mediated by information technologies, ethnographers and other qualitative researchers have been urged to adapt their strategies to the nature of these rapidly developing online social worlds.

Robert V. Kozinets is a pioneer in the field of adapting traditional ethnography procedures (of entrée, collecting data, making valid interpretations, doing ethical research, and providing

possibilities for participant's feedback). Extending the strengths of ethnographic methods to series of qualitative studies of online communities, he coined the term "netnography" to grasp the special trade of ethnographic study on online communities. In the words of Kozinets (2002, p. 62), this approach "is a new qualitative research methodology that adapts ethnographic research techniques to study the cultures and communities that are emerging through computer-mediated communications." Netnography, then, exemplifies how technology affects the nature of social life and how, in turn, qualitative researchers adapt to new and emerging forms of sociability by rethinking and extending well-established techniques and procedures. Netnographies have been conducted in a variety of online communities in order to grasp their specific meanings and symbolisms. One recent example is O'Leary and Carroll's study (2012) of online poker subcultures in which netnography proved to be a useful and cost-effective method of providing insight into the social ecosystem of online poker gamblers and specific attitudes pertaining to this community.

Data Analysis

Not long ago, management and analysis of qualitative data typically involved (and often still does) an overwhelming amount of paperwork. Qualitative researchers buried themselves in their handwritten field notes, interview transcripts, or other documents. Trawling systematically through their material, researchers marked chunks of data and organized these bites of data in more or less complicated index systems, drew models of emerging analytical patterns, discovered data that challenged the emerging conceptual framework, and ended up, in most cases, with a final report, dissertation, or research paper. For today's qualitative researchers, this caricature lacks an important dimension: the computer and often also various types of data analysis software.

As pointed out by Raymond Lee & Nigel Fielding (2004), the launch of the first generations of word-processing programs was a great help to most qualitative researchers. These programs made it possible to store, edit, systematize, and modify collected materials in a far more effective and less time-consuming way. Qualitative researchers no longer had to make large piles of photocopies in which chunks of text were marked or cut out and placed in separate holders since the new word-processing packages provided very useful

searching, copying, cutting, and pasting facilities. Similarly, conventional database programs (such as Microsoft Access) found their way into the realm of qualitative research supporting the analysis of interviews and other qualitative materials (Myer, Grube, & Franz, 2002).

In the early 1980s, the first generation of qualitative analysis programs was introduced (Weitzman 2000, p. 804). These types of programs, which have later been referred to as CAQDAS, or Computer-Assisted Qualitative Data Analysis Software (Lee & Fielding, 1995), facilitated direct coding of the data and subsequent searches in the coded material. Later versions of these first generations of CAQDAS allowed for quick assessments of overlapping or inter-relating concepts, retrieval of data on specific themes from participant with assigned with specific attribute values (Lee & Fielding 2004). Obviously, such facilities support the more sophisticated and conceptual work of qualitative research since they enable the systematic investigation of emergent patterns and relationships in the data. These later generations of programs that assist more complex and interpretive analytic tasks have been termed “theory builders” since they contain tools and procedures that support the development of theoretical schemes and conceptual frameworks. Some of these programs also support collaborative qualitative research processes allowing members of a research team to merge their analytic work in an integrated project and similarly for assessing quality measures such as inter-coder reliability. Furthermore, some packages support the integration of various kinds of digitized qualitative data such as photographs, video recordings, and rich text files, and some also contains tools for coding not only in textual data but directly in digitized speech and video recordings.

The introduction of computer-technology in the processes of collecting, managing, and analyzing qualitative data has triggered important discussions in the research community on the nature of qualitative research and on the limitations and potentials offered by these new technologies. A core issue in these debates has been the possible (and perhaps non-reflected) ways that technology impacts on the practice of qualitative research and analysis (Buston, 1997). In terms of data analysis software, technological skeptics have expressed concerns that most software packages stimulate a specific (code-based) analytic strategy (Seidel, 1991)—that the widespread use of CAQDAS eventually may result in an unhappy homogenization and convergence towards a certain type of analysis and even towards

a new kind of data management orthodoxy (Barry, 1998; Coffey, Holbrook, & Atkinson, 1996; Welsh, 2002); that use of computers and software packages creates a distance between researcher and data and prevents the researcher from immersing herself in the data (Roberts & Wilson, 2002); and finally that many software packages are somewhat incompatible with the ambiguous nature of qualitative data and thereby pose a threat to the holistic nature of qualitative research (Kelle, 1995; Mason, 2002; Weaver & Atkinson, 1994). On the other hand, technological optimists (e.g., Richards & Richards, 1994) do not neglect the potential pitfalls of non-reflexive use of CAQDAS, but emphasize how software packages enable management and analysis of large pools of qualitative data, and that CAQDAS provides procedures for rigorous and transparent analytic work and thus potentially for enhancing the quality of qualitative research. Similarly, optimists also argue that, although the quantitative tools in analysis software may be used recklessly, sensible use may provide the researcher with a quick and thought-stimulating overview of characteristic patterns or indicate possible relations or hypotheses to be explored. The powerful search engines at the heart of most CAQDAS packages are also effective tools for improving the validity of analysis, which is also the case concerning the visualizing or model building facilities with direct data access. Although this somewhat exaggerated polarization between technological skeptics and optimists is grounded in the nature and specific features of the available software packages, the different positions often also reflect some more fundamental differences in terms of qualitative methodology approaches. Researchers within the phenomenological tradition that emphasize the subjective understanding and interpretation of behavior and verbalizations often tend to view CAQDAS more negatively than qualitative researchers working within the paradigm of grounded theory, content analysis, or other approaches that may profit from the coding and quantification tools available in many programs (Berg, 2003, p. 266).

From this technological history, it appears that technological advances have transformed and advanced key elements in the practice of qualitative research (i.e., collecting, managing, and analyzing data). Technological developments (sometimes carried out by qualitative researchers themselves in collaboration with technicians and computer engineers) have broadened the methodological repertoire of qualitative researchers and have brought about new ways of gathering, managing, and analyzing

data. Thus technological innovations have changed and transformed the practical tasks of qualitative research as well as its scope and potentials. Due to technological development, qualitative researchers today spent less time recording and producing data than they did a few decades ago, just as new ways of working with and looking at data became possible with the launch of analysis software and when audio and video recordings enabled the researcher to store and return to situations as they originally appeared. The technological history of qualitative research thus reminds us that qualitative researchers continually reflect on and adjust their methods not only to fit the actual phenomenon under study, but also to a broader milieu of cultural factors such as technological innovations (Markle, West & Rich, 2011).

Concluding Thoughts About the Future

It would be no exaggeration to conclude that, during the last decades, the broad church of qualitative research has reached a strong position within the human and the social sciences. As our six histories have suggested, different social, cultural, material, intellectual, and technological changes have spurred the emergence of new qualitative methods and innovations of well-known and celebrated approaches. Furthermore, strong efforts to describe and delineate qualitative procedures and research guidelines (in textbooks and qualitative curricula at universities) within the variety of approaches from grounded theory, content analysis, interaction process analysis, discourse analysis, and others have contributed to the relative success and widespread acceptance of qualitative research as “real science” in the research community as well and in the public sphere. As qualitative researchers, we of course welcome this situation. However, it might be fruitful to consider the possible, often neglected side effect of this “scientification” of qualitative research. Almost twenty years ago, Valerie Janesick warned qualitative researchers against the pitfall that cultivating and outlining procedures of qualitative methods could result in researchers losing sight of the subject matter and thus gradually undermining the potential of qualitative research. Like others, she referred to this tendency as “methodolatry” that she designated:

a combination of method and idolatry, to describe a preoccupation with selecting and defending methods to the exclusion of the actual substance of the story being told. Methodolatry is the slavish attachment and devotion to method that so often overtakes the

discourse in the education and human service fields. [Janesick, 1994 p. 215]

Whether methodolatry in the qualitative research community is interpreted as an expression of some sort of “physics envy” among qualitative researchers, or as an adjustment of qualitative research to the public demand for evidence-based knowledge (which often is confused with positivist and experimental studies), the consequences of such qualitative methodological fetishism might be detrimental for qualitative research. Psychologist Kerry Chamberlain has discussed how privileging questions of method over all other important questions pertaining to the research process deprives qualitative research its distinctive characteristics as a creative, flexible, interpretive enterprise with a strong critical potential. If qualitative research is confused with categorizing and illustrating talk, instead of interpreting and theorizing the contents of it, and if qualitative researchers uncritically adopt conceptions of validity and reliability from positivism and fail to acknowledge the ideological base of the trade, we will compromise essential aspects of our historical legacy (Chamberlain, 2000) and perhaps even the *raison d’être* of qualitative research.

We make no claim that methodolatry is standard among qualitative researchers. However, we have registered that discussions of such tendencies have emerged within several subfields of qualitative research. Some (e.g., Steiner 2002) have even concluded that the majority of qualitative research could be characterized as “scientistic” due to its concern with generalizability, objectivity, and rationality. Others have used George Ritzer’s (2008) McDonaldization thesis to argue that we are witnessing a McDonaldization of qualitative research. According to Ritzer, the cultural process of McDonaldization is characterized by efficiency, calculability, predictability, and control—all of which seem to favor standardized methodologies in qualitative research. Nancarrow and colleagues have concluded the following about the impact of McDonaldization on qualitative research:

Just as McWorld creates ‘a common world taste around common logos, advertising slogans, stars, songs, brand names, jingles and trademarks’ [...], the qualitative research world also seems to be moving towards a common world taste for an instantly recognisable and acceptable research method that can be deployed fast. [Nancarrow, Vir, and Barker, 2005, p. 297]

With this risk in mind, we find it appropriate to remind ourselves of the core values and

characteristics of qualitative research. Privileging method over the subject matter of research and developing rigid methodological straitjackets will not bring qualitative research closer to “the royal road of scientificity” (Lather, 2005 p. 12), but rather the opposite. Only by reminding ourselves of our historical legacy and embracing the unpredictable, flexible, and messy nature of qualitative research can we practice, develop, and fertilize our trade.

Taking a look into the future of qualitative research necessarily involves a reflection on the possible lines of development within the field of computer-assisted qualitative research. Since technological advances keep a steady pace and since qualitative researchers continuously seek out the potential of newly available research technologies, innovations that strengthen the nature and widen the scope of qualitative research are to be expected. In the early 2000s, it was still considered an open question whether the development of voice-recognition software could lead to computer-supported interview transcription (Flick 2002, p. 17). At present, however, some voice-recognition software packages have transcription modes and speech-to-text modes that support the transformation of (certain kinds) of talk into text. Although the speech-to-text software still needs some improvement in order to free research assistants or secretaries from the work of transcription, reaching this goal is not at the forefront of the innovative efforts put forward by the proponents of computer-facilitated qualitative research. The cutting-edge developments of CAQDAS seem to point at new and interesting directions. One emerging and promising field is the integration of geographical information systems with the use of CAQDAS. Lately, qualitative researchers such as Fielding & Cisneros (2009) and Verd & Porcel (2012) have described how data from Geographical Information System (GIS) could be integrated in software packages supporting qualitative analysis. Thus Verd & Porcel applied a form of qualitative GIS in a study of an urban transformation project in the city of Barcelona in order to investigate the social production of urban space. And in addition to opening a completely new strand of qualitative urban research (or perhaps more correctly revitalizing the urban sociology of the early Chicagoans by adding new data and technologies) that stimulates a new form of sensitivity towards the spatial dimensions of social world, such creative synthesis of GIS technology and CAQDAS has added new concepts to the vocabulary of qualitative research such as geocoding or georeferencing, or “the type of information

processing that consists in the geographical localization and placing of qualitative material such as photographs, field notes, text fragments of documents and any other information.” (Verd & Porcel, 2012, paragraph 14). The CAQDAS trend in qualitative research can be seen as aligned with the scientific push for standardization, but it can also be looked upon in a more balanced way. Although uncritical use of CAQDAS admittedly might fuel processes of methodolatry (stimulating the technical side over the interpretive side), there still seems to be strong potential in using CAQDAS to strengthen the qualitative investigation of some forms of audio-visual data (such as video data) or data sources (geographical and spatial) that until recently have been used primarily by quantitative social researchers. The fruitful mixing of qualitative analysis software with seemingly non-qualitative data rests on the creative and imaginative work of qualitative researchers that dare challenge traditional conceptions such as the sharp demarcations of qualitative and quantitative research. This might be an example of a more general development related to the whole mixed-methods movement.

Other contemporary qualitative researchers argue that we need to move in the exact opposite direction of methodolatry. The traditions that are prevalent in the *Handbook of Qualitative Research*, edited by Denzin and Lincoln, favor a more political, even activist, attitude to qualitative research, which is based on ethical values of care and community (rather than validity and reliability) and employs aesthetic means (e.g., borrowed from literature and the arts) to favor social justice. Today, the tension between those on the one hand who seek to use qualitative methods to do “normal science” (in a Kuhnian sense) and employ standardized formats to communicate their findings, and those on the other hand who experiment with non- and even anti-methodological approaches (e.g., drama, poetry, autoethnography) is central to the field of qualitative research. The time might have come to ask if there is anything that holds the many different practices together that go by the name “qualitative research”—other than the name itself. Some scholars give a negative answer and go so far as to argue that we are—or should be—in a position of “post” qualitative research (St. Pierre, 2011), meaning that the term has lost its rhetorical force and simply freezes inquiry rather than setting our thinking free. Others (e.g., Hammersley, 2011) find that the current fragmentation and experimentation in qualitative research risks rendering qualitative research

redundant in the eyes of society. A field with so much inner tension might not be taken seriously.

Our goal in this context is of course not to settle this discussion once and for all. As the historical contributions presented in this chapter demonstrate, qualitative research represents a range of rich and vibrant approaches to the study of human lives and social phenomena. As we have seen in this chapter, the term itself—qualitative research—is barely 100 years old, and we are confident that if the term is no longer useful, then researchers of the future will have to invent other concepts to designate the process of studying our social and personal worlds. That it is worthwhile and necessary to study ourselves as human beings, with all the qualitative characteristics of our experiences and actions, seems to be as true as ever. And the fact that the landscape of qualitative research is extremely variegated might not be too surprising given the complexities of the subject matter.

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The History of Historical-Comparative Methods in Sociology

Chares Demetriou and Victor Roudometof

Abstract

This chapter offers an overview of the historical trajectory of comparative-historical sociology while focusing on the issue of development of specific methodological approaches. The legacy of sociology's founding fathers is discussed first, followed by an overview of the post-World War II US-based academic research program that led to the institutional organization and academic acceptance of the specialty. The chapter then offers an assessment of effect of the cultural turn to historical sociology by addressing the themes of temporality and narrative. It argues that today the field displays a variety of different methodological strategies or perspectives and that this tolerance toward varied methodologies is likely to be a permanent feature. It concludes with an assessment of those new or emerging areas that seem to offer room for further research.

Key Words: Sociology, comparative, historical, methods

By way of introduction, it is necessary that we caution the reader with regard to the following: discussions of methodology within the field commonly known as comparative historical sociology are nearly inescapably analyses that involve the entire *practice* of the field—and what constitutes the field's state of the art. In this sense, they encompass a huge array of issues, including epistemological concerns. At the core of this predisposition lies the professional necessity of mostly US-based historical sociologists to acquire cherished professional credentials for their practice and to subsequently gain legitimacy, acceptance, and visibility within the discipline. That is quite unlike the situation in other fields—for example, in urban or medical sociology—that historically faced no such requirement of proving their worth to the profession. This is in large part the consequence of the historically entrenched distance between social sciences and history in the US educational system. In Europe, unlike in the United States, this issue is far less pronounced. As a result,

outside the United States, historical sociology does not appear as a coherently organized field to the extent that it does in the US-based educational system and its transatlantic satellites.

To the casual observer, comparative historical sociology may appear to be an area of sociology aiming to draw comparisons from historical material. However, as this chapter shows, this is quite contested. In fact, the extent and nature of comparisons is part of the ongoing discussion among sociologists themselves. This is evident when reading mainstream work in this field. Although explicit comparisons of historical cases are made, analyses and reconstructions of single historical trajectories are far more typical. These treatments of history are indirectly comparative; that is, they feature comparisons between a single specific historical case and a theory, framework, or research paradigm. This can take several forms. For example, in one instance, there might be a comparison made between what is being analyzed and an abstract

model—for example, the outlook of a civilization, the archetype of modernization sequence, or any other sort of ideal type. In other instances, there might be a comparison made between what is analyzed and a recurrent historical trajectory—for example, the comparison between a historically specific turning point toward democratization and an often-traversed turning point (such as a particular level of economic or institutional development). In light of this broad and theoretically informed research strategy, a multitude of analyses of historical change are undertaken in dialogue with broader theoretical agendas. These serve the expansion of sociological understanding and can be considered as part of historical and comparative sociology. This understanding renders the adjective “comparative” almost redundant. It might seem a strange conclusion but, wittingly or unwittingly, most practitioners tacitly accept it when they describe themselves simply as “historical sociologists.”

Being so broad, this description of the field of (comparative) historical sociology raises the issue of boundary setting: is the field of historical sociology coterminous with the field of history as such? Was not Herodotus himself a historical sociologist? After all, Durkheim (Durkheim & Giddens, 1972, pp. 78–79) has advocated the position that when sociological explanation becomes causal it also becomes historical. This chapter’s response to the quagmire looming behind the mixture of logical taxonomy and disciplinary practice is unoriginal. We shall avoid describing the field of history, however reasonable this might seem, and focus simply on self-identified historical sociology; that is, we shall obey the standard rules of disciplinary practice. In itself, this is a complicated task, for some works are less forthcoming than others in declaring their disciplinary allegiances. In the stories historical sociologists tell about themselves, there are familial references to the work of those who, strictly speaking, would be classified as nonsociologists (historians, philosophers, and numerous others). These linkages are part of the tradition of historical sociology and therefore claim their rightful place in this chapter’s pages. But we will eschew an “independent” mapping of the logical connections between historical sociology and its kin in other academic fields.

As one may already suspect, the field of historical sociology does not feature a coherent set of methodological guidelines, and this makes it wiser to speak not of a single method but of *methods* in the plural. Just as with other fields, historical sociologists lack a shared consensus regarding the field’s general

methods. Indeed, when considering historical sociology’s most influential examples of scholarship, it is necessary to admit that they vary greatly in the logic and epistemological presuppositions involved in their implicit or explicit comparisons. Therefore, a comprehensive view is needed to sort out the methodological variety. Such a view should be capable of navigating from the substantive historical account to the various facets of meta-theoretical reflection. We anchor this effort on a meta-theoretical scheme suggested by Charles Tilly. This scheme considers the logics of explanation as being elements of meta-theory independent from other elements of meta-theory—such as, most notably, ways of data collection and epistemology. Tilly (2008, p. 9) classifies the variety of explanatory logics employed in general sociology as follows:

1. *Proposals of covering laws for complex structures and processes*: Explanations here consist of subjecting robust empirical generalizations to increasingly higher levels of generalization, with the most general of all standing as laws
2. *Specification of necessary and sufficient conditions for concrete instances of the same complex structures and processes*
3. *Variable analyses*: In which statistical analysis shows the extent to which one or more predictor variables (often called “independent variables”) account statistically for variation in an outcome variable (often called the “dependent variable”)
4. *Location of structures and processes within larger systems they supposedly serve or express*: For example, through the claims that element X serves function Y within system Z
5. *Stage models*: In which placement within an invariant sequence accounts for the episode at hand; for example, the stages of revolution or of economic growth
6. *Identification of individual or group dispositions just before the point of action as causes of that action*—propensity accounts
7. *Reduction of complex episodes, or certain features of those episodes, to their component mechanisms and processes.*

As already mentioned, this scheme’s merit lies in avoiding the conflation between the logic of explanation and issues of epistemology or data collection. In principle, the logic of explanation is independent of the other two dimensions. It is possible to consider a given logic of explanation as it is applied through different types of collected data and different epistemologies (e.g., empiricist/idealist/realist or skeptic/

positivist). The ability to juxtapose these logics of explanation with other dimensions becomes particularly important vis-à-vis some epistemological tensions characterizing historical sociology, such as the analytical versus narrative tension, because it casts them in a new light. This does not mean that we prioritize the logic of explanation over epistemology. We do not believe, in fact, that the major stakes in the field of historical sociology, when it comes to the question of comparative method, can be reduced to such logic any more than to epistemology. But these logics of explanation, although not exhaustive, capture historical sociology's methodological varieties.

The Predisciplinary Masters and Their Early Legacies

Sociology in general and historical sociology in particular trace their roots to the work of Karl Marx, Max Weber, and Emile Durkheim. If their profound role in shaping subsequent scholarship explains their contemporary status as the discipline's founding fathers, what is particularly entrancing for historical sociology is their approach to explaining modernity. Indeed, this approach—essentially consisting of conceptualizing the modern condition as a phase in a historical process and of analyzing that process sociologically—was later considered as historical sociology's trademark. Instead of historicizing or philosophizing modernity along the earlier paradigms of Enlightenment or Romanticism, each sought to understand it more rigorously and “technically” as a form of social interaction and related immanent meaning. But what is the respective logic of their method and what is comparative about it?

“Marx is not a sociologist,” Henri Lefebvre (1972, p. 22) writes, “but there is sociology in Marx.” Much of it, in fact, is historical sociology, including the use of the comparative method in the narrower sense. Of course, Marx's theory encompasses knowledge about reality into a synthesis that integrates philosophy, sociology, history, psychology, and so on. Because of this, his writings seem to take different methodological colors. He offers, in the first place, a sophisticated stage model of history based on interactive path dependency and a dialectical approach, a style that was turned into a caricature by later followers and critics. But when he takes up less macroscopic analyses, he also appears to be a systems thinker, locating structures and processes within larger bifurcated systems sustained through these structures—to wit, economic exploitation is thought to be constitutive of class structures and other sociopolitical edifices. And then one can

distinguish between “old Marx” (e.g., the author of *Capital*) and “young Marx” (the author of the *Economic and Philosophical Manuscripts* of 1844). Although traditional interpretations have used “old Marx” as their point of reference (just like most Marxists did until the mid-twentieth century), a far more complex picture emerges once both “young” and “old” Marx are reconciled (for a discussion, see Zeitlin, 2001). Marx's fundamental epistemological perspective therefore emerges as one that acknowledges the irreducible richness of reality and the violence that reduction does to such richness. Accordingly, as the consensus in recent decades points out, Marx does not reduce social relations to a systemic whole or to essentialized classes. Rather, his primary unit of analysis remains the form of social relations, and this is also his key unit of comparison (Kelly, 2003). In this sense, these relations have an analytical role that places them close to Tilly's (2008) last logic of explanation: relations that are explanatory components of processes—although Marx never put it that way.

Marx's key form of social relations—the relations of production—is very specific. But for Marx, relations of production do not entail a rigid, unchanging structure, nor is their historic path closed to contingency. For one, their development relates to the development of the broader mode of production, including variable productive forces such as technologies and other material means of production, as well as labor power. Furthermore, relations of production feature power asymmetries that are not strictly (or not always) determined by economic forces but potentially also by the political application of power, as in the feudal system. And as “the Eighteenth Brumaire of Louis Bonaparte” (Marx, 1869/1973) makes clear, political events also can have a role to play in the way power asymmetries are configured, at least in the relatively short run. It is in this theoretical context that Marx can use the form of relations/modes of production as a unit of comparison. Following Marx, therefore, it is possible to investigate relations and modes of production in a given historical context, to develop generalizations on that basis, and to reduce their form for the sake of understanding that historical context in relation to other historical or geographical contexts. In his own writings, and particularly in the *German Ideology*, Marx differentiates among the tribal or communal, slave-owning, Asiatic, feudal, and capitalist modes of production (for an overview, see Zeitlin, 2001, pp. 152–180). Thus, he arrives at a model that allows him to advance cross-temporal as

well as cross-spatial comparisons. However, he did not conclude the historical-sociological project pivoting on this comparative hinge, and he left numerous issues open for lively debate among scholars of later generations.

By comparison, Max Weber's comparative project makes more assured strides, both methodologically and historically. Methodologically, in fact, Weber seems to have clarified the heuristic and comparative ideas Marx had originally pioneered—although Weber did not see himself as someone working along the same lines. In large part influenced by Georg Simmel's notion of the "form," Weber's concept of the ideal type acknowledges both reality's elusive complexity and the necessity for formulating tentative generalizations. In turn, such generalizations operate both as heuristic tools for specific historical investigation and as comparative tools for broader historical understanding. Intellectual progress thereby becomes possible because ideal types are open for further refinement down the path of research, hence constructing a feedback loop whereby concepts further refine historical research and vice versa. It is precisely this move that enabled Weber to develop his analytical approach to history, as opposed to the traditional approach of description through conjecture. Importantly, Weber's analytic approach, when judged through his entire *oeuvre*, is open to multicausality, which rejects cultural as well as material determinisms (Holton, 2003).

Weber's main preoccupation was to understand the distinctiveness of the West. The series of ideal types that he developed—*vis-à-vis* rationalization, secularization, bureaucratization, and so on—were in the service of this quest. By far the most celebrated and widely read of Weber's works, *The Protestant Ethic and the Spirit of Capitalism* (1905/2010), initiated an entire debate concerning the extent and validity of Weber's thesis about the causal influence of Protestantism in the consolidation of Western capitalism. Although that debate was influential in promoting further historical and quantitative research on the relationship between religious affiliation and "work ethic," it is important to note that the book itself was only the first step in a larger and long-term research agenda Weber pursued in the course of his life. His systematic focus on the comparative study of world religions—which matured in the course of at least two decades of research and writing but was ultimately left incomplete—should be viewed in light of his broader master plan of juxtaposing the Western path to modernization with the alternative Oriental historical paths. In studying

Confucianism, for example, he sought to understand the reasons that prevented capitalism from developing in China (Weber, 1915/1951). Thus, he understood the Confucian worldview to be counter to such development because it valued individuals' cultural growth and devalued productive activity—the latter, of course, being the hallmark of Protestant asceticism. It must be stressed, however, that Weber considers additional factors, beyond religion, in understanding the "case of China," particularly factors relating to patrimonialism and bureaucracy. In light of contemporary postcolonial criticism, it is perhaps self-evident that Weber's conceptualizations, however brilliant, did ultimately succumb to the dominant Eurocentrism of his era. For example, Weber's famous use of *caesorpapism* to understand church-state relations in Byzantium is not an interpretation supported by contemporary historical writing. Still, in spite of such shortcomings, Weber's analytical perspective—and in particular the multicausality explicitly endorsed in his writings—promoted a pioneering comparative project with lasting value. This project's legacy was felt mostly in the second half of the twentieth century and was closely connected to the rise of historical sociology as a distinct specialty (for an overview and explication, see Kalberg, 1994).

Durkheim's legacy in historical research is actually enormous, but his main impact has been in the field of history (both in social history and the history of ideas), not in self-declared comparative-historical sociology. Durkheim's work on the systems of classifications provided a cornerstone for the articulation of French structuralism in the mid-twentieth century and still can be felt in French post-structuralism, especially in the work of Michel Foucault. In addition to the history of ideas, Durkheim's paradigm of "social morphology" as a multicausal map of factors shaping "society" (demography, geography, quantitative analysis based on hard data, etc.) provided the fundamental methodological template on which Fernand Braudel (1949/1972) conceived his framework for the analysis of social change in the *longue durée*. Braudel was perhaps the most influential of the group of French scholars who introduced the notion of social history, a version of history that was opposed to history as narration of the past actions of "great men," thereby taking to heart Marx's argument that history is not made by great men but, rather, by the lives led by the majority of the people. Social history effectively altered the practice of history as a

profession as well as the range of issues historians consider as falling within their purview. Braudel's methodological adoption of Durkheim's approach has been enormously important. Braudel (1982) even has addressed methodological and disciplinary issues concerning the dialogue between history and sociology. His immediate effect was in post-World War II France and later Europe, where a tradition of close collaboration emerged between social scientists and historians. It eventually was transferred across the Atlantic, but at a later stage and with much less of the transdisciplinary intent it originally possessed. In an interesting twist of intellectual history, Braudel's own work provided the intellectual springboard for Immanuel Wallerstein to construct the intellectual path that eventually became world system analysis (and which is discussed later in this chapter).

In terms of immediate antecedents within the discipline of sociology, Durkheim's legacy is most famously felt in the work of Maurice Halbwachs. A student of Durkheim, Halbwachs is the pioneer scholar of the field of collective or social memory. His approach builds on Durkheim's notions of collective solidarity (1893/1997) and commemorative rituals (1912/2008). For Halbwachs (1925/1992), individuals remember through group membership. This makes all memory collective and a living reality available as *social facts*, in sharp contrast to history, which—according to Durkheim—is available facts of a dead reality. In this sense, collective memory is inherently historical-sociological because it links past and present. Although Halbwachs's theory was a promising point of departure, it did not start a comparative research program on collective memory—his untimely death in 1940 is a biographical detail that aptly illustrates the extent to which World War II had a great impact in delaying intellectual developments. Social memory studies eventually took off in the course of the last decades of the twentieth century (for an introduction and overview, see Olick, Vinitzky-Seroussi, & Levy, 2011).

In all, Durkheim's influence on the disciplinary specialty of historical sociology has not been as prominent as that of Marx or Weber. Most historical sociologists, in fact, would not claim him as an intellectual forerunner, even though, unlike Marx and Weber, he was a self-identified sociologist. Durkheim has been viewed by historical sociologists as leaning more toward static analysis than social change. That is accurate only up to a point. In the first place, Durkheim arguably did not theorize on change as much as he might have; for example, he did not take

his work along the path of evolutionary theory, like many nineteenth-century scholars had done, despite his preoccupation with systemic ideas similar to those of the evolutionists. Also, if one considers his basic explanation of modernity in *The Division of Labor in Society* (1893/1997), one will not see much historical analysis in it: modernity's key characteristic is organic solidarity, whereas premodernity's is mechanical solidarity. The movement from one to the other is not analyzed in concrete historical terms; rather, it is attributed to an abstract, summary notion of changes in demographic density. But the claim of Durkheim's disregard of historicized social change must not be exaggerated because the systemic change with which he explains emergent modern structures has apparent historical references. For example, the advent of restitutory law in modernity is explained by the need to make commercial contracts in a society that has become individualistic and is thus seen as a change that is part-and-parcel of a historically all-too-obvious broader change in social configuration and form of solidarity. Still, viewing himself as a sociologist and not a historian, Durkheim understood his job to be the description of the form(s) of society, leaving more specific historical formulations to the historians, whom he respected (Burke, 2003).

The intellectual legacies of Marx, Weber, and Durkheim were already well acknowledged by the 1960s and 1970s, the era when historical sociology emerged fully as a disciplinary specialty. Some aspects of those legacies of historical sociology would eventually be appropriated. This was particularly the case with the scholarship of Marx. Because Marxists of various intellectual and disciplinary orientations were typically interested in understanding capitalism's historical antecedents, they were widely engaged in historical sociology. A review of Marxist tradition is outside the scope of this chapter. Most of the main emerging subtraditions are relatively well known: on the one hand, the "scientific" branch (reductionist and deductive to the point of knowing the covering laws, rather than searching for them) and, on the other hand, a variety of perspectives typically grouped under the label of "Western Marxism." But whereas Marxists created early on a pluralistic tradition of historical analysis, Max Weber's comparative program did not have immediate antecedents. In the immediate decades following his death, his work was hardly used in Germany or elsewhere during the period of inter-war Nazism and fascism. It was only after the conclusion of World War II that the process of rediscovering the classics' legacy eventually took root.

Of course, a famous piece of sociology's history is Talcott Parsons's (1937) "discovery" of Max Weber's work and his importation of Weber into the United States. The effects of this discovery were felt mostly after World War II; it was not until the 1950s that references to Weber's work would increase, building up an influence that would eventually become considerable. For the most part, this influence pertained to the "interpretive" facet of his method. His Kantian-rooted intent to construct many of his key ideal types from the social actors' point of view therefore grew to become his mark. Even in historical sociology, his work was seen through this epistemological prism, thus taking the role of counterweight to materialist, determinist, and empiricist traditions. But historical sociology also heeded his substantive contributions, particularly his ideas on institutions, which influenced those historical sociologists working on state formation, among others (Bendix, 1962; Kelly, 2003).

Karl Polanyi's (1945/1957) *The Great Transformation*, a work offering its own explanation of the emergence of capitalism, is an interesting instance of early historical sociology because it alludes to both Weber and Marx. Polanyi's take, to be sure, does not purport to belong in the paradigms set by Weber or Marx, although at first glance it would seem to be closer to the latter than the former. It may be said, in fact, that Polanyi's argument turns Weber on his head with some help from Marx. Although Weber considers the premodern Protestant religious creed and resultant work ethic to be critical antecedents of modern capitalism, Polanyi argues that modern capitalism's survival is enabled by the modern state. Thus, where Weber sees modernization destroying old social vestiges as it develops rational institutions governing polity, economy, and society, Polanyi sees the state actively destroying the old order through its effort to facilitate capitalism. However, although not manifestly Weberian, Polanyi's work is part of a historical-sociological tradition with a Weberian flavor, a tradition both institutionalist and against neoliberal explanations of change. It is around this institutional approach that his comparative method rests, for his analysis compares concrete institutions and their subtypes. Such institutional analysis counters neoliberal economics' conceptualizations of markets, society, and state. Polanyi, an early critic of the Austrian School of Economics, anticipates historical sociology's self-conscious distancing from neoliberal paradigms, just as Weber's and Marx's respective brands of economic history do (Block & Somers, 1984).

The Birth of Historical Sociology's Self-Consciousness

It is in the United States that historical sociology developed a self-identity and came to be considered a subdiscipline of sociology. American sociology provided much of the impetus and many of the intellectual targets behind the advent of the disciplinary specialty that has become known as historical sociology. To simplify a complicated story, in the 1960s and 1970s, a relatively small number of sociologists with Marxist-inspired intellectual questions and an interest in history published work that challenged the functionalist paradigm influential at the time. These sociologists did not act in concert, and functionalism may have been more "collateral damage" than their actual target. Nevertheless, forming a "movement," in the loosest sense of the term, they created a collective system of intellectual foundations (Adams, Clemens, & Orloff, 2005, p. 7) that signaled the emergence of the subdiscipline as such. Several fine collections and individual volumes contain a summary of the state of the field and its historical evolution (Abrams 1982; Delanty & Isin, 2003; Mahoney & Rueschemeyer, 2003; Skocpol, 1984; Smith, 1992). Our current discussion is not meant to offer a general comprehensive analysis of the entire field; instead, we are concerned mainly with its methodological features.

Functionalism, an intellectual school led by Talcott Parsons in the 1950s and 1960s, championed the fourth logic of explanation in Tilly's (2008) list: location of structures and processes within larger systems they supposedly serve or express. In functionalist sociology, this logic was applied without much regard for history. The theory was general and abstract and essentially retained this dual characteristic even when concerned with questions regarding social change and its sequences. Neil Smelser's (1962) *Theory of Collective Behavior* is a fine example of this style of theorizing. From the 1950s forward, modernization of mostly post-colonial societies became a huge issue in the US public policy arena. The end of colonial rule and the creation of newly independent states not formally aligned with either the communist East or the capitalist West were coupled with the necessity for developing public policy strategies for their steadfast modernization. US policy viewed the articulation of modernization theory as an important component in popularizing the Western road to modernity as preferable over the route championed by the communist East. It was in this highly politicized context that modernization became one of functionalism's

areas of interest (for a useful review, see So, 1990). Functionalist theorizing, critics argued, viewed modernization essentially through the conceptualization of social differentiation, something that led to standardized and unproductive explanations of (real or alleged) pathways to modernization—although this feature is prominent only in the first-wave or early modernization theories.

What was far more distressing to the left was that functionalist modernization theory implicitly promoted a teleology in which the *telos* resembled an idealized version of American society: liberal yet nonideological, capitalist yet nonexploitative, plural yet conflict-free (Skocpol, 1984, p. 3). In reality, the blending between functionalism and modernization theories remained confined to the level of theory or, more accurately, meta-theory. In terms of explicit references, functionalist authors did not engage directly with contemporary events, and functionalist theorizing paid little attention to contemporary historical context. Parson's (1971; Parson & Inkeles, 1966) most explicitly historically oriented statement adopted a bird's-eye view of long-term historical trends, whereby the triumph of democracy and the West was foretold. Ironically, the success of democracy came far earlier than anticipated, but the 1989 collapse of communism in Eastern Europe was certainly within the scope of Parson's theorization.

In most conceptualizations of modernization, though, evolutionary change was seen as far more typical and normative compared to dramatic or revolutionary breaks with the past. Although structural functionalism has apparent parallels with Durkheim's version of functionalism, it did not push the door first opened by Durkheim regarding historical causality. Durkheim made a clear distinction between the determinants producing structures and the social functions of structures; US functionalists focused only on the latter point, ignoring the former. The functionalist dictum that structures come into and remain in existence insofar as they are functional is therefore not one that Durkheim would have made—nor one particularly conducive to historical sociology. In the climate of the 1960s, such dominant intellectual predispositions did not sit well with the post-World War II generation, a generation hungry to challenge the “establishment,” fascinated with heroic figures (like Che Guevara), and bent on inquiring into history's critical turning points.

One of the most influential functionalist analyses of change, however, Shmuel S. Eisenstadt's analysis of empires, does not fit easily into this sort

of modernization theory. A pioneering work on a neglected topic, *The Political Systems of Empires* (1963) is also an ambitious comparative work. It covers thirty-two political systems—from the “pre-bureaucratic” classical Athens and Carolingian Empire to the “proto-bureaucratic” ancient Egypt, Sassanid Persia, and the absolutist monarchies of Europe—and seeks to explain their stability and instability, survival, and failure. Because Eisenstadt considers each political system to be part of its broader social system, he understands his cases through functionalist conceptualizations. But his comparison rests on a series of case attributes that, at times, follow functionalist ideas, such as the level of complexity and differentiation in society, and, other times do not, such as when they relate to the political goals of rulers. As a result, Eisenstadt advances an explanation just as multifaceted as the analytical attribute he utilizes: search for necessary and sufficient conditions, location of structures in systems, and tracing of institutional formation in a way reminiscent of Weber. Ultimately, Eisenstadt's analysis features not only the jargon of functionalism but also a measure of its teleological bias; after all, some surviving “proto-bureaucracies” do enter the path to modernization. However, if in this sense his approach stands in opposition to the historical sociology brewing at the time his publication appeared, it will be seen that this approach does not stand opposed to some of the logics of explanation on which historical sociology operated.

Historical sociology's distancing from functionalism was not illusory. Importantly, in contrast to the functionalist conceptual edifice, most historical sociologists considered sociological concepts to be inherently *temporal*, *spatial*, and *contingent*—thus, concepts that make sense *only* in historical contexts. The concepts they employed at this early stage of the subdiscipline typically stemmed from structuralism—mostly in its Marxist variant—but these did not lead to structural determinism. If nothing else, contingency and events offered counterbalance in the analyses. Along the way, historical sociologists sought to analyze social power and social inequality, dynamics of conflict and political alliance, and, in the final analysis, complex sociopolitical pathways to change with due attention paid to contingency and timing. But perhaps the most unifying characteristic of historical sociology at this time was its macrosociological orientation. Its subject matters, after all, were often processes, such as state formation, industrialization, modernization, and so on, that stretched over a great span of time; when more

temporally contained phenomena, such as revolutions, were to be explained, the explanation itself went back considerably in time. In this way, historical sociology indirectly rejected various micro-analytical methodological paradigms existing in sociology (as well as in economics and, of course, in psychology) in favor of ways capable of painting the “big picture.”

But which logics of explanation did these early historical sociologists employ? A prominent tradition involved attempts to specify necessary and sufficient conditions for concrete instances of the same historical processes—that is, the second logic of explanation on Tilly’s (2008) list. This was to be done by following as closely as possible John Stuart Mill’s (1843) twin principles of comparison: the method of agreement, which holds the causal factors of an outcome to be those, and only those, that are present in all of the cases examined; and the method of *difference*, which complements the method of agreement by looking for additional cases in which the absence of the causal factors corresponds to the absence of the outcome. In historical sociology, this approach was not to be an exact emulation of the same approach in general sociology, which often relied on statistical analysis of covariance to reach conclusions on necessary and sufficient conditions (thus merging two logics of explanation, co-variance and necessary/sufficient condition, although this merger was not always sought because it is not logically necessary). It was not an exact emulation of general sociology, not only because conditions could not be treated statistically, but also because historical sociology needed to think about conditions in terms of or in connection with dynamic pathways rather than in terms of static qualities. Also, the independence of cases that general sociology often takes for granted was not easily established in historical sociology (Axtmann, 1993). Although in Mill’s logic the independence of cases is paramount for establishing validity, in historical time, events—especially when occurring simultaneously or in close historical proximity with each other—are hard to conceive as truly independent from each other; the revolutionary wave of 1848 is a good case in point, and the 1989 collapse of communism in Eastern Europe offers another far more contemporary example.

Still, the so-called comparative-historical school of historical sociology was formed on the putative basis of Mill’s methods. Barrington Moore’s (1966/1973) *Social Origins of Dictatorship and Democracy* became an early example to emulate. Through an

examination of British, French, American, Chinese, Japanese, and Indian history, Moore identifies two processes leading, respectively, to dictatorship and democracy. He examines several factors relevant in these processes, such as the presence or absence of a balance of power between the landed aristocracy and the king, as well as the development (or not) of an alliance between bourgeoisie and aristocracy at the expense of the peasantry and the working classes. He maintains, however, that the critical determining condition was the presence or absence of a strong bourgeois class at the later stages of these processes—its presence being conducive to democracy and its absence conducive to dictatorship. Theda Skocpol, a student of Moore, followed his general example in her *States and Social Revolutions* (1979). She uses a combined Marxist and institutionalist perspective to analyze the Russian, French, and Chinese revolutions, ultimately emphasizing the role of class struggle at moments of state crisis. Despite the merits of this comparison, what is useful to note here is the apparent shortcomings of its underpinning research design: although it selects cases in which revolution was not supposed to happen according to Marx’s account of history, this is ultimately a selection of cases in which revolution *was* the outcome—a selection on the dependent variable, to use the jargon of quantitative sociology. So, Skocpol does clearly what Moore does less clearly; that is, disregards Mill’s method of difference. Her case selection, as that of Moore’s less directly, illustrates a limitation inherent in macroscopic historical comparison, namely, the lack of a sufficient number of cases to draw from and/or the lack of resources to treat history sufficiently if many cases are found. In general, then, the application in historical sociology of the logic of necessary and sufficient conditions was only partial.

Yet the underpinning general logic behind Mill’s principles—if not necessarily their strict logical form—had a wide appeal. Consider the so-called Brenner Debate, an exchange in the field of economic history much celebrated by historical sociology. Aiming to understand and explain the causal pathways creating economic growth in the late Middle Ages—the all-important primitive accumulation of capital discussed by Marx—Robert Brenner developed an argument pivoting on the relation between landowners and farmers. As the power of the landowners grew at the expense of the farmers, Brenner argues, the former pushed for land enclosures, which, together with technological innovations, created economic growth. Countering

Brenner's Marxist thesis, a group of arguments paid more attention to ecological factors, such as demographics, disease, and the infrastructure of commerce (for the debate, see Ashton & Philopin, 1985). In methodological terms, the debate—and more so the Brenner side of it—engaged in a search for variation in economic growth among different places in Europe, although placing most of the emphasis on England and France. It is the presence and absence of certain factors that is thought to lead to the explanation of the variation in historical pathways—hence Mill's ideas on comparison. But the square logic of Mill's principles, taken as the yardstick, clashed once again with the multiterminacy characterizing history; the application of these principles ends up negotiating both the issue of reduction of complexity and the issue of competing explanations. The latter is as murky as the former because it is often difficult for historical analysis to tell mutually exclusive factors from codetermining factors. (Quantitative sociology addresses this challenge by “controlling variables.”) Going back to the Brenner Debate, it may well be the case that the social institutional factors favored by Brenner and the ecological factors favored by his challengers codetermined the primitive accumulation of capital.

Faced with acute comparative design constraints, historical sociology reacted variously. One main line of reaction formed around arguments for critical case selection, that is, limited but theoretically logical case selection as opposed to universally exhaustive case sampling (Eckstein, 1975). What makes critical cases logical, of course, varies with the theoretical context at hand; for example, the logic of Skocpol in selecting cases of “unexpected” revolutions is an application of a logic influenced by Marxian theory. But this method could support even a single case study, and this was something particularly appealing to historical sociologists. Thus, many historical sociologists who were uncomfortable with applying Mill's principles—arguably the comparative method par excellence—opted for analyses of single historical trajectories. One can mention the work of Seymour Martin Lipset (1963) on US state formation, of Michael Burawoy (1972) on the interplay of class and state in Zambia, and of Michael Hechter (1975) on the Irish exception to the pattern of British state formation. It is not possible to comment at once precisely and collectively on the logics of explanation underpinning these works and others like them, but it is perhaps possible to hint at the general area in which they operated and, hence, also the areas that they avoided. Focusing on

single pathways to change, these works dealt with comparison with other cases only marginally, if at all. As such, they avoided the strict application of a comparative method, whereas, for other reasons, they avoided systemic explanations and appeals to stage models and covering laws. The space they occupied was an uneasy one situated between the logic of process tracing and the logic of specification of conditions affecting the pathways to change, without being positivist about the latter.

Although the aforementioned sociologists (Moore, Skocpol) worked to articulate the mainstream research agenda identified by the label of comparative-historical sociology, two additional groups of scholars offered different accounts of social change: the world system group and a loose “group” of “Weberians” that included occasional contributions by scholars not typically considered historical sociologists per se (for example, Collins, 1979; 1998).

World system analysis offered a distinct view of historical processes. By the early 1970s, Braudel's thinking along the lines of the *longue durée* met with functionalist methodology and neo- or post-Marxist ideas—especially in their “heretical” (vis-à-vis “orthodox” Marxism) interpretation originally championed by Leo Trotsky. The sociological synthesis of all these influences was Immanuel Wallerstein's (1974) *The Modern World System I* (which was followed by two additional volumes in 1980 and 1989 and yet a fourth volume published in 2011). The group of scholars that formed around Wallerstein's ideas was soon institutionalized as the American Sociological Association's (ASA's) section on the Political Economy of the Modern World System. Binghamton University's Fernand Braudel Center became the institutional hub for this agenda, and the center's *Review* became the journal that championed this research agenda. Eventually, a second electronic journal—the *Journal of World Systems Research*—was also founded.

Although there has been considerable evolution within world system analysis, the approach's basic tenets—as presented by Wallerstein—have remained central to its orientation. His argument, simply put, was that the emergent European capitalism of early modernity transformed the world into areas of center and periphery in terms of economic development and derivative power, thereby creating a global interconnected system (see Chase-Dunn, 1998). The world system described was one whose components follow paths of change largely determined by the mode of production. Class, gender,

nation, and race were viewed as conceptual categories born out of capitalism's impact on people. To deal with the challenge of generating theory through evidence from history, this theory proposed a deductive logic. Strict comparative work in this theoretical framework therefore became circumscribed, "explaining variation across regions, countries, cities, and other sites within the same mode of production within the world system" (Adams et al., 2005, p. 17). But if the theory led to a deductive logic of analysis, this was so only up to a point, for, as Ragin and Chirot (1984) maintain, it also left space for explanations of change through factors exogenous to a given regional context. All in all, world system theory's explanatory mode operated on a series of logics. More clearly than in the case of Marx, this mode included references to functions in systems, to the succession of stages, and to the role of constituent ingredients of processes, with the mode of production being the most important.

In terms of generating research in historical sociology, therefore, this argument led to a theoretically logical but empirically limiting scheme. World system analysis, though, performed important disciplinary tasks. First, the ASA section's membership became a point of attraction for US-based researchers interested in the Third World, and hence, it offered the means of inserting non-US issues into the debates—an important move in challenging US academic ethnocentrism. Second, world system analysis offered an alternative to the Mill-inspired method of difference; hence, it opened the door to broader theoretical horizons. Third, it contained a major challenge to the state-centered Skocpol–Moore strategy. In fact, key criticisms against Wallerstein came from among the "state" theorists who complained that world system analysis adopted essentially Marx's notion that the state is simply the governing committee of the bourgeoisie. In Wallerstein's (1991) work, though, the challenge to the centrality of the nation-state as the unit of analysis goes even further. Wallerstein argues that the state cannot be viewed as the central organizational reference point but, instead, that a system of states offers the basic template within which individual states are constructed. This decentering of the state in favor of broader trends—later to acquire the label "global"—is an important contribution in the evolution of sociological thinking at large (for an interesting assessment, see Waters, 1995).

The "Weberian" school of historical sociology never truly formed a single or coherent group of

scholars working together; rather, it constitutes a label that can be used to subsume the work of individual scholars. It has become a rather conventional academic mainstream; as a label, Weberianism registered not only a scholar's departure from the basic tenets of political Marxism or neo-Marxism but also a willingness to accept issues of culture as legitimate topics of social concern alongside the centrality of power and inequality. In terms of epistemological position, Weberian research strategies varied, as an examination of the works of two central figures, Reinhard Bendix and Michael Mann, can show. Mann's work is discussed in the next section; a brief sketch of Bendix's research strategy is presented here.

Bendix was not the typical historical sociologist of his time because he operated within Weberian epistemological skepticism—perhaps taking it to its logical conclusion. Bendix's (1984) research strategy consisted of a *contrast of contexts*: instead of treating historical cases in accordance to Mill's method, Bendix used historical context as the basic point for historical comparison. This strategy allowed him to get around the problem of the independence of historical cases—which cannot be dealt with using Mill's method. As a result, his studies include huge comparisons between historical trajectories—with causal explanation confined only to historically informed, limited generalizations. Strictly speaking, generalizations emerge only on the basis of the historical cases examined; these are not general, but are specific and bounded statements with no predictive power. For Bendix, causal explanation and comparison were not ready tasks, but ones bound to violate reality and hence to be approached with great restraint. What historical sociology must aim for, Bendix holds, is a balance between generalization and the empathetic understanding of historical particularity and richness. This solution echoes Weber's attempt to synthesize causality and interpretation. Bendix's (1978) masterpiece, *Kings or People: Power and the Mandate to Rule*, is based on this epistemology. It focuses on the question of the legitimacy of rule but avoids drawing firm generalizations on the social bases of such legitimacy. Rather, the goal is to clarify the diversity of pathways toward legitimacy by juxtaposing contrasting episodes of history, most particularly patterns in agrarian societies that rely on religion versus patterns in modern societies that rely on domination in the name of the people, as well as on divergent pathways within the latter group of historical experience. For those more

inclined toward causal analysis, Bendix's concepts retain only a sensitizing role in the service of historical description, and they do not become comparative tools for theory building (Rueschemeyer, 1984). Ultimately, the analysis limits itself to mere historical narrative. This issue is explored further because the themes of narrative and the event made a forceful return in later sociological research and writing.

After the Cultural Turn: Temporality, Narrative, Processes

In the 1960s and 1970s, historical sociology acquired a degree of self-consciousness. This identity grew slowly and remained essentially American. It was not until 1983 that the "Comparative and Historical Sociology" section of the ASA was launched, thus giving the field a more formal outlook (for an excellent guide to this ASA section, bibliographies, book reviews, sources, and additional information, see the *Comparative and Historical Sociology Section of the American Sociological Association*, n.d.). However, outside the United States, the field's struggle for identity has been even more protracted and tentative; the International Sociological Association's Thematic Group on Comparative and Historical Sociology was founded only in 2003 and has not yet been elevated to the status of a full-fledged research committee, and the most recently instituted international professional society, the European Sociological Association (founded in 1992), lacks a research network on historical sociology. Historical sociology, to be sure, has become institutionalized in other organizations—such as in the UK's International Studies Association (see *Historical Sociology: A Working Group of the British International Studies Association*, n.d.)—but these developments have hardly contributed to the field's growth and legitimacy on an international scale.

The emergent identity of US-based historical sociology did not mean that the field acquired an undisputed profile, let alone a distinct comparative methodology. Indeed, the opposite is the case: the more plural the field became, the more it reflected on itself. The historical sociology of the 1960s and 1970s, although featuring its share of pluralism, gravitated toward the logic of necessary and sufficient conditions via an elastic treatment of Mill's principles, although a number of practitioners also avoided covering the law paradigm and statistical analysis. The latter might have been due to lack of available data or technical expertise as opposed to

a deeper epistemological objection; for example, a reviewer of Eisenstadt (1963) wondered why the author, having amassed data that invited statistical analysis, ended up not employing it (Zinnes, 1965). By the early 1980s, however, US-based historical sociology was already moving down the path toward increasing pluralism. New epistemological tendencies and thematic orientations were gradually being added to an expanding pool of perspectives, and several of the new perspectives drew from nonsociological or neglected sociological traditions. Some of the characteristics of historical sociology, as discussed earlier, lost their dominance. Gone, for example, was the strong predilection to analyze big processes that account for important contemporary predicaments; as Roger Gould (2005, p. 287) notes, latter-day historical sociology has been ready to pay attention also to episodic patterns of the past that only marginally or indirectly affect the present, such as instances of collective contentious action. Historical sociology was therefore being challenged, not only on account of its method and tradition, but ultimately also on account of its *raison d'être* as a distinct intellectual field.

It is outside our current aim to present a rounded intellectual history of the field from the 1980s to the present. Instead of narration, our aim here is more on clarifying the related parameters regarding logics of explanation and epistemology. To some extent, of course, the intellectual fissures in historical sociology occurring during the final decades of the twentieth century, as well as the debates developing around them, had antecedents in the earlier periods; part of our aim in the previous section was to offer a critical assessment of historical sociology of the 1960s and 1970s so as to make the subsequent reaction to it more comprehensible. The celebrated "cultural turn," most notably, revisited some of the issues already covered by Bendix and his intellectual opponents: skepticism versus positivism, interpretive sociology versus realism or positivism. But the cultural turn was actually quite multifaceted and forceful, ultimately affecting the next generation of scholars and the way they conducted work in historical sociology.

This orientation has been prominently featured in *Remaking Modernity* (Adams et al., 2005), the latest landmark volume to define historical sociology's agenda over the past decade. The volume introduced the notion of three waves of historical sociology, suggesting an aesthetic rift between the more recent third-wavers versus the established second-wavers (Skocpol, Moore, Wallerstein, and

so on). The first wave refers to the renewed historical and comparative sociology in the 1960s and 1970s, building on the classical foundations of historical sociology, criticizing structural-functional modernization sociology, and developing a variety of path-breaking historical macrosociological studies. The second wave, occurring in the 1980s, established a primarily social-scientific program of historical sociology, directing research toward systematic comparative-historical analysis and explanation of varying (national) modernization paths in combination with meso- and microsociological social history. The third wave, under way since the 1990s, has been strongly influenced by the cultural turn in the social sciences, critically reflecting the modernist premises of the second wave, but continuing microsociological trends in combination with cultural history, historical institutionalism, rational choice, feminist orientations, and postcolonial studies and thus pluralizing the approaches, methodologies, and topics of historical sociology. Although the wave metaphor has great appeal, it is not entirely supported by the record of scholarship itself. The third-wavers are more coherent in terms of their aesthetics, tone, and themes as opposed to a unity found in any meaningfully shared research program.

In this sense, *Remaking Modernity* (Adams et al., 2005) is usefully contrasted with another major attempt to define the current state of historical sociology: *Comparative Historical Analysis in the Social Sciences*, edited by J. Mahoney and D. Rueschemeyer (2003). Building particularly on the second wave of a systematic social-scientific version of historical sociology (as represented by Skocpol, 1984 and Tilly, 1984), the editors are less interested in giving an encompassing overview on the multiple strands in historical sociology than in defining the accumulative progress made by the application of comparative historical analysis in the social sciences. Both editors pursue a vision of comparative historical analysis oriented to the explanation of substantively important outcomes of sociopolitical change in the modern world and defined by a concern with causal analysis, an emphasis on processes over time, and the use of systematic and historical-contextual comparison. In the context of the broader trends in the third wave, the editors are clearly critical of overly postmodernist, constructivist, and historicist orientations (and are inversely characterized by Adams et al., 2005, as friendly amendments of the modernist second wave). They extend the second-wave orientation to macrohistorical processes, particularly

with historical-institutionalist approaches toward the notion of path-dependent development. Furthermore, they are particularly interested in combining quantitative and qualitative research while they view sympathetically the inclusion of contextualized rational-choice approaches and other causally oriented perspectives (e.g., those perspectives that aim at systematically explaining socio-historical processes) in the cultural-scientific strands of historical sociology.

Mahoney and Rueschemeyer (2003), of course, do not so much describe the field as prescribe how, in their view, it should look. These prescriptions actually came from a variety of vantage points. Throughout the 1990s, for example, a series of commentators (Goldthorpe, 1991; Kiser & Hechter, 1991) argued in favor of preserving the nomothetic mode of explanation and charged that the practice of historical sociology falls short of appropriate methodological criteria. Like the call to “bring in” rational choice, however, the call to renew the search for covering laws has been hardly heeded in actual historical-sociological research. Yet the desire to transfer to historical sociology some of the analytical rigor of general sociology presents a widespread and longstanding phenomenon and one which sociology’s “cultural turn” brought to the fore, given that it renewed the interest of historical sociology’s methodological and epistemological issues. From moderate positivist perspectives, therefore, arguments for the merit of marrying statistical analysis and/or network analysis with the explanation of historical processes have been put forward, and indeed innovative monographs along these lines were produced (e.g., Mische, 2007). But the appeal of mainstream sociology in historical sociology is also seen in subtler ways, such as the borrowing by historical sociologists of the jargon and/or logic of variable analysis. Calhoun (1997) critically notes this tendency vis-à-vis the historical sociologists of the second wave, and Steinmetz (2005) argues that the tendency is still present vis-à-vis the third-wavers, albeit not as pronounced as before.

Countering such neo- or post-positivist tendencies, Calhoun (1998) and Steinmetz (2005), as well as Gotham and Staples (1996), argue in favor of moving away from totalizing modes of explanation and toward temporality and narrative analysis as more appropriate strategies for capturing historical contingency and the nature of historical events. These debates further serve to solidify the viability of interpretative perspectives within a hitherto

comparativist camp. They demonstrate that the field has gained sufficient legitimacy to accommodate a variety of methodological perspectives—in a manner similar to sociology at large. This dialogue should not be viewed in isolation as a solely subdisciplinary debate. In contrast, it practically extends a broad topic that concerns the interdisciplinary dialogue between history and sociology (or, more broadly, among all the social sciences). After all, historians' practices have been the subject of a long debate concerning their methodology. The social scientific debates over the status of narrative and its construction or its explanatory power are therefore not fundamentally different from similar debates on historical narrative as such. In addition to these differences in research foci and orientation, it is also fair to suggest that the field's unity has been substantively reduced in large part because of a growing distance between historicized sociology and comparative sociology (for two overviews that highlight these differences, see Mahoney, 2004; Clemens, 2007). Although the latter assumes a definite strong methodological stance and insists on using a battery of mostly methodological—either positivist, post-positivist or realist—indicators, the former increasingly assumes an interpretative stance at odds with realism or positivism. Moreover, historical sociologists suggest that there is no inherent necessity to invoke a special methodology to justify the field. In this respect, the methodological orientation is said to be the same as for other fields of the social sciences. Practically, comparativists concur (see, for example, Ragin, 1987), but the two sides subscribe to radically opposing epistemologies. Ironically, these concerns are not unique to the field because they extend to sociology itself. Therefore, the debates between adherents to objective knowledge and followers of the interpretative tradition are likely to continue. But their rise suggests that the field itself has gained considerably in terms of scholarly legitimacy as not to insist on presenting a distinct method to justify the quest into the historical record.

The foundations for shifting the mode and logic of inquiry away from the older and more established comparativist camp lie with a renewed attention to time (Aminzade, 1992). The significance of time lies in its irreversibility and linearity; unlike experimental time, historical time is nonreversible. In turn, this means that likely outcomes are not going to be observed as a result of identical factors. Additionally, the independence of cases compared is often nonexistent. This paves the way

for reconsidering the foundations of historical sociology. Sociological historical narrative becomes far more meaningful as a strategy that presents a series of events, organized in a given sequence, and in which causality is attributed within the narrative itself. For Franzosi (1998, p. 517), “narrative texts are packed with sociological information and a great deal of our empirical evidence is in narrative form.” Narratives are therefore relevant formats that include sociological explanation as such. Griffin (1995, p. 1245) further argues that historical sociology is “a distinct way of approaching, explaining, and interpreting general sociological problems.” Because historical sociology situates social action and social structures within historical contexts and examines their historical unfolding, it can creatively use the temporality of social life to raise questions of central significance for social theory. Such a perspective highlights Weber's interpretative predisposition and adds phenomenological and hermeneutical aspects to it. It is also a perspective that fits admirably well with the necessity to develop new narratives or logics of explanation for the globalized world of the twenty-first century. More recently, Gorski (2004) has put forth a meta-theoretical model that rejects deductive logic, with interpretation having priority over causal explanations. That is, “explanations are constructed through a work of interpretation in which theoretical terms are used to construct causal models of social processes” (Gorski, 2004, p. 19). This “constructive realism” model explicitly attempts to transcend the conventional epistemological divide between interpretation and causal analysis, aiming toward the formulation of a novel synthesis.

Culture and agency also have resurfaced as key themes in recent scholarship. Such approaches increasingly take into account both the analytic as well as the concrete autonomy of culture (Kane, 1991). Culture is thus viewed as containing structured elements—a view that effectively resists cultural reductionism, while, once the concrete autonomy of a cultural form is established, rendering possible the tracing of culture's influence or causal effect on historical situations. So this approach to culture has fed what Tilly (2008) calls “propensity accounts,” that is, explanations of historical situations through group dispositions just before the emergence of the situation. It has also generated scholarship that makes culture itself the subject matter of analysis rather than the explanatory tool for the analysis of other phenomena. Yale-based cultural sociology, for example, has an elective affinity with the latter

perspective. In Alexander's (2003) work, cultural structures are explicitly evoked; specific structures constructed include the notion of the Holocaust as a world-historical event or that of cultural trauma. Alexander's (2006) description of the civil sphere as the foundation of American democracy can be further used as a historical sociology view of US-based democratization.

Although the aforementioned considerations refer to the state of the art mostly in US-based sociology, the *Handbook of Historical Sociology*, edited by Delanty and Isin (2003) offers a distinct perspective on the field. In contrast to the two American syntheses, this handbook assembles, in a more European reflexive style, contributions with the aim of rethinking and reorienting the undertaking of historical sociology from a postmodern, post-disciplinary, and post-Orientalist perspective. Historical sociology is identified less with a social-scientific methodology of explaining and interpreting sociohistorical processes (as in the second wave) and more with the interpretation and deconstruction of the formations and transformations of modernity. The influence of postmodernism leads to a historically reflexive approach to modernity, whereas post-disciplinary trends attempt to overcome the divide between social-scientific and cultural-scientific approaches. The volume's post-Orientalist orientation tries to transcend the still predominant Eurocentrism as well as its vague Occidentalist counterpart. This postmodern, post-disciplinary, and post-Orientalist reorientation of historical sociology forms a broader attempt to transcend the opposition between the second-wave social-scientific and the third-wave cultural-scientific varieties of historical sociology and introduces, albeit in a more reflexive than analytical-methodological direction, a variety of new transnational, civilizational, and global orientations.

By way of concluding this section, it is worthwhile to discuss yet another turn taken by sociology and historical sociology. Alongside the cultural turn, there was a turn taken toward relational sociology. The growth and popularity of the relational perspective came from many sources, including Pierre Bourdieu's general theory and American structuralism/network theory, and, ultimately, various brands of it have remained attached to distinct epistemologies. However, instead of reviewing works representing divergent epistemological trends—and hence rehashing some of the oppositions already discussed—in the remaining space, we review three samples of relational historical sociology that share a similar logic of analysis, one that emphasizes process

analysis. Conducting historical sociology as process analysis has proven enduring—as the reviews on these pages, going back to Marx, show—and the works adopting this sort of historical sociology with self-awareness and reflection deserve special attention.

Norbert Elias's (1939/1994) *The Civilizing Process* represents a pioneering statement of relational historical sociology. Originally published in German during World War II, this work is a quintessential, if also original, sample of historical sociology, one that only found recognition in the (English-speaking) field decades later. It is therefore just as appropriate to consider this work a sample of latter-day historical sociology as it is to consider it an old classic. The intellectual breakthrough achieved by this work is particularly evident in the face of the current tendency to connect the concept of social relations with the concept of process. Concerned with the centuries-long and very encompassing process entailing changes in behavioral orientation and cultural values in the West, Elias's book describes a process that pulls together into a process grand patterns of social-political organization and modes of psychological self-discipline: a "sociogenetic and psychogenetic investigation," to use the work's subtitle. What is important to underscore regarding Elias's approach is that the civilizing process is not explained by factors exogenous to it but is rather self-explanatory. Thus, for Elias to explain the civilizing process is to describe it. This logic of explanation falls squarely within the last logic listed by Tilly (2008)—reduction of complex episodes into their components. Although the components of the civilizing process are historically very rich in Elias's account, ranging from transformations in political rule to transformations in table manners, they pivot on a relational conceptualization of action and actors that Elias calls *figuration* (see Elias, 1978, for a theoretical exposé). Figurations, then, are enduring yet dynamic formations that intertwine with each other to form the larger process. They are at once constitutive elements of the process and elements for explaining comparative variation within the process.

If Elias's self-reflexive historical sociology meant creating novel relational concepts, those who wrote about relational sociology during the closing decades of the twentieth century had no need to create concepts anew. But if they had to apply existing relational concepts to sweeping historical analyses, as Michael Mann did, they had to be daring. Mann, in fact, gave his relational concepts a

task of Weberian proportions. In two long volumes, *The Sources of Social Power, Volumes I and II* (1986 and 1993, respectively), he uses a four-faceted conceptual scheme to account for nothing less than the history of humanity. Societies in history are conceptualized as organized webs of power relations, particularly featuring networks stemming from political power, military power, ideological power, and economic power. As they interweave, these networks of power shape the flow of history, enabling the emergence of specific polities, economies, and other enduring configurations, as well as fueling their decay and eventual replacement by other configurations. The story Mann tells, therefore, is a complicated story of multiple, parallel, and often intertwined processes that nonetheless end up being a reduction of great complexity; but it is a reduction not in the fashion of singling out conditions but of delineating key elements in a long-term process—this is shown in his employment of analytical categories such as extensive, intensive, authoritative, and diffuse networks of power. At the same time, Mann’s analytical narrative, although chronological, is inherently comparative, for the task constantly facing his project is to explain why power accumulates in some places but not in others, even during the same period of time.

Charles Tilly’s approach to relations, processes, and comparisons, although in the service of less ambitious historical projects than those of Elias or Mann, is actually more forceful than that of either. In *Dynamics of Contention* (McAdam, Tarrow, & Tilly, 2001), Tilly puts forward and applies to history an analytical model designed particularly for the analysis of processes. For Tilly and his co-authors, processes comprise other constituent processes, which can be called subprocesses and/or mechanisms, and the task of analysis is to specify how the larger phenomena emerge from combinations of the smaller phenomena. Describing a process through its parts therefore provides the explanation of the process. Aiding in the tracing of processes, this perspective also aids comparison because these processes and mechanisms are held to recur in a variety of historical contexts. Described and named accordingly, mechanisms and processes become units of comparison whereby different concatenations of mechanisms and their emergent processes are compared from context to context. Democratization is one of the areas of research to which Tilly applies his analytical perspective. In *Democracy* (2007), he argues that the process of democratization is constituted by three subprocesses: the integration of

trust networks into public politics, the insulation of public politics from categorical inequality, and the reduction of autonomous power clusters. Each of these subprocesses is articulated in different historical contexts by an array of possible mechanisms. For example, insulation of public politics from categorical inequality took place in South Africa through two mechanisms stemming from the antiapartheid struggle: a mechanism producing sustained popular resistance against the direct inscription of racial categories into politics and a mechanism forging powerful coalitions across racial and ethnic categories. This sort of historical research, Tilly maintains more generally, can generate knowledge on recurring patterns, thus also generating an inventory of mechanisms in the service of comparative work. His own inventory of relational mechanisms includes boundary activation, brokerage, certification, diffusion, escalation, and scale shift, to name but a few (Tilly & Tarrow, 2007).

Conclusion and Future Directions

Unlike in the 1980s and 1990s, the field currently does not display the spirited debates of the past. For example, today it is commonly recognized that historical sociology should pay attention both to social regularities (such as the emergence of modernity) as well as to irregularities (such as revolutions). If general sociology aims at apprehending and analyzing repeated patterns in social reality, then historical sociology, trying as it does to explain historical outcomes, cannot rely only on the effect of regular social patterns. It must pay attention also to such “irregularities” as contingency, sequencing, and timing of events. As this chapter has demonstrated, this recognition was not always present in historical sociology. It is characteristic, for example, that although William Sewell’s (1996) argument about the transformative role of certain historical events challenged the discourse of the late 1990s, it is essentially taken for granted among historical sociologists today—whereas professional historians found it intuitive from the outset (see also Sewell, 2005).

Currently, historical sociology is characterized by increasing plurality and eclecticism in terms of epistemology, logics of explanation, and thematic orientation. Although the “epistemological wars” ushered in by the cultural turn and the advent of postmodernism are not likely to resurface in the near future, disagreements over epistemology likely will continue to exist. A case in point is the “contained” epistemological rift over the processual approach to

historical sociology. Advocates hold that processual historical sociology is in a unique position to deepen the understanding of how contingency, conjuncture, and temporality intertwine with more regular elements of historical processes. Accordingly, the apprehension and reconstruction of historical processes are tasks that can be carried out with limited efficacy. Therefore, these tasks must be carried out with due emphasis placed on discovering regularity, and researchers should not adopt the various logics that analyze regularity in mainstream sociology, such as the logic of variable covariance (Demetriou, 2012). The same approach is claimed by “analytical” sociologists stressing regularity over the irregular. Although not all of them are historical sociologists, this group is analyzing models with great emphasis placed on analytical rigor (Hedstrom & Bearman, 2009; Hedstrom & Ylikoski, 2010). This epistemological rift is, of course, in many respects a replay of older epistemological disagreements in historical sociology, although the current stakes appear to be over a renewed interest in processes.

It is fair to say that historical sociology has learned to live with its plurality out of necessity because it can hardly do otherwise. This tolerance is underpinned by the relative lack of organizational structure. The specialty’s lack of unequivocal boundaries clearly defining insiders and outsiders means that practitioners may receive rewards both within and outside the field. Moreover, scholars with an occasional involvement in the specialty can make meaningful contributions. But although this tolerance results from acquiescing to (and not embracing) difference, it is nevertheless entrenched. To describe the field’s future directions is foolhardy. Other than understanding the field’s current state of affairs, the best one can offer is a shortsighted propensity account—to borrow from the meta-theoretical framework adopted by this chapter. Therefore, we see two broad patterns standing out from the current state of affairs: (1) an entrenched diversity of epistemologies, logics of explanation, and thematic orientations; and (2) a “globalization” of the field, which means both the engagement with globality or globalization and the inclusion of non-Western voices. Some of the developments that the field may see in the near future, then, may stem from these two broad patterns.

Current and future historical sociology most likely will exhibit fragmentation, leading to eclecticism in epistemology, methods of data collection, thematic orientation, and logics of explanation. Although growth in positivist terms might not be forthcoming, fragmentation and eclecticism could

enable reflection on longstanding ways of conducting historical sociology. One potential area where such reflection may develop is in *paired comparison*—a widely employed but hardly theorized approach of historical-sociological analysis. Scholarship based on paired comparison has long been produced by historical sociologists, from the first-waver Tilly (1963) comparing a counter-revolutionary region with a nonbellicose region in post-revolutionary France, to the third-waver Brubaker (1992) comparing citizenship in France and Germany. It is worth adding that Bendix’s work also can be viewed as being, in part, aligned with this strategy. What this method does, above all, is foster comparison between detail-researched historical contexts. In a rare meta-discussion on paired comparison, Sidney Tarrow (2010) argues that the method has the benefit of unpacking two processes and of putting in perspective the respective factors that account for the development of the processes. Even though, in Tarrow’s opinion, the logics of process analysis and of variable covariance can be combined, his account demonstrates that paired comparison is most suited to the logic of process analysis.

The second broad pattern that, in our view, characterizes the current state of affairs in historical sociology concerns the growing realization of an effective engagement with the historicity of globalization. Sociologists have played a pivotal role in exploring global–local relations and alternative or competing models and projects of modernity (Arnason, 2003; Eisenstadt, 1986; 2002; Gran, 1996). In fact, the most comprehensive social-scientific treatment of globalization (Held, McGrew, Goldblatt, & Perraton, 1999) is also partly the product of sociological research (for an overview of the recent state of the art, see Rossi, 2008). One major issue underpinning much of contemporary scholarship concerns the extent to which globalization should be viewed as a long-term process predating Western modernization (for examples, see Hobson, 2004; Hopkins, 2002; Robertson, 2003), or, alternatively, whether it should be viewed as its outcome or latest phase or consequence (for examples, see Albrow, 1997; Giddens, 1990). This research agenda is often joined with more recent efforts to track the rise and recurrence of various forms of cosmopolitanism throughout world history (e.g., “The Cosmopolitan Predicament,” 2009; Holton, 2009; Jacob, 2006; “On Cosmopolitanism,” 2008). It is fair to say that, to this day, no comprehensive overview of this emergent research agenda has been produced. That might be a task for the future. Comparisons of the

contemporary situation with other periods have been made, however, and in some instances these completely abandon the conventional use of modernity as sociology's master reference point. For example, in a series of insightful articles, Inglis and Robertson (2004; 2005; 2006) have related the emergence of a cosmopolitan worldview to the growing interconnections of the Mediterranean region during the period of Greco-Roman Antiquity. Using the writings of historian Polybius as one of the main reference points, they argue that it was during that period that ecumenical notions gained the upper hand over more parochial visions. As this example shows, the cosmopolitan research agenda might increase the range of sociological research, which could expand to address such questions not only in terms of Western European historical experience, but also in terms of histories of the rest of the world.

In these debates, sociologists are increasingly joined by historians. But, unlike with sociologists, it took some time for global or world history to fully institutionalize itself in terms of publication outlets. In 2006, the foundation of the *Journal of Global History* was a turning point; this journal achieved an admirable ranking fairly quickly in the Web of Science. Contemporary historical or historical-sociological research on aspects of global affairs often follow on the footsteps of earlier work pioneered by world system theorists; a flurry of articles, books, and volumes has appeared that inquire into commercial, structural, political, or broadly economic or socioeconomic relations and interdependencies spanning world history. In this regard, the institutionalization of this line of research comes at the cost of losing sight of the more cultural aspects of global-local relations. One major aspect of the globalization of historical sociology should be the inclusion of analyses that do not view developments in the non-Western world solely in terms of divergence or convergence from Western patterns but that chart new analytical relations, social interpretations, and causal arguments based on different historical trajectories. Postcolonial criticism (Chakrabarty, 1992; Said, 1978) has rightfully criticized the limits of Western-centered interpretations. But as O'Brien (2006) argues, scholarship should not draw the misleading conclusion that simply because Western accounts are biased, no accounts are possible. Realizing the partial nature of all historical accounts means that scholarship should demonstrate due humility in its claims; but new historical accounts of social processes will undoubtedly

further illuminate the practice and relevance of sociology in the twenty-first century.

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The Centrality of Ethics in Qualitative Research

Anna Traianou

Abstract

This chapter begins by examining some key principles of research ethics: minimization of harm, respect for individual autonomy, and preservation of privacy. It then examines why research ethics is now treated by qualitative researchers as increasingly central to their work. Key factors here are the rise of ethical regulation and a diversification of approaches to qualitative inquiry, with some recent approaches treating ethics as fundamental. The chapter notes an important sense in which ethical commitments are central to research: inquiry must be underpinned by epistemic values and virtues that facilitate the production of knowledge. However, it is argued that the increasingly common tendency to make central those principles concerned with how the people being studied should be treated constitutes a form of moralism, characterized as “the vice of overdoing morality.” The chapter closes with an attempt to outline the proper role that research ethics ought to play in qualitative research.

Key Words: ethical principles, ethical regulation, qualitative research, moralism, epistemic values, ethical reflexivity

In recent years, research ethics has come to be treated as much more central to the research process than it was in the past, particularly in the case of qualitative inquiry. Of course, social researchers have long been concerned with ethical issues, and there is a substantial literature dating from at least the 1960s concerned with how access is to be gained, what harm can come to participants in particular sorts of research, how the autonomy of the participants should be protected, what sorts of reciprocity should be involved between researcher and researched, what should and should not be included in research reports in order to respect privacy, and so on. However, generally speaking, until quite recently ethics was seen as an ancillary matter: as important but not as central to the very task of research. In recent years, this has changed significantly.

I explore this change later and assess it. First, though, I will outline the nature of qualitative research ethics and some of the debates that surround it.

What Is Qualitative Research Ethics?

Most discussions of research ethics focus primarily on how researchers should treat the people whom they are studying or from whom they obtain data. A number of principles are usually identified here, such as minimizing harm, respecting people’s autonomy, and preserving their privacy. There are also some procedures, notably securing *informed consent*, that are frequently used and sometimes seen as defining the requirements of ethical research.

Minimizing Harm

One of the most important ethical concerns in carrying out any research relates to the potential for harm involved. This has been central to much discussion of research ethics generally but especially in the field of medicine, where research often involves painful and perhaps even dangerous interventions: the administration of drugs,

surgical treatment, and the like. But harm is an issue that arises in other areas of investigation as well, including where qualitative research methods are employed.

The identification of harm is not straightforward. Potential threats of harm arising from research can fall into the following categories:

- Pain, physical injury, and permanent disability
- Psychological damage, for instance, emotional distress, erosion of self-confidence, stress-related illness, and so on
- Material damage of some kind, for example, loss of freedom through imprisonment, dismissal from a job, reduction in income or wealth, damage to property, and so on.
- Damage to reputation or status or to relations with significant others, for example, through the disclosure of information that was previously unknown to some relevant audience.
- Damage to a project in which people are engaged, to some group or organization to which they belong, perhaps even to some institution or occupation in which they participate.

The fact that serious harm of the kinds just listed *could* be produced by research does not imply that it is common or is usually very likely. Indeed, it seems that, in most qualitative work, the danger of significant harm of any type is low and that its occurrence has been rare. But this judgment assumes, among other things, that it is possible to assess the seriousness of harm with a reasonable degree of reliability. And this is also required if we are to make defensible decisions, when doing research, about whether risking some potential harm is justifiable. Some accounts of research ethics require that harm should be entirely avoided but, given the range of types of potential harm, of varying levels of seriousness, it is frequently impossible to avoid it completely. Judgments must be made about the level of danger involved and about the seriousness of the harm that could occur. Although making such judgments is not straightforward, and is open to dispute, it is possible to do this in ways that are reasonable in the circumstances (Hammersley & Traianou, 2012, chapter 3).

Some types of study, are, however, generally seen as involving heightened risk for participants. Thus, research topics can be more or less “sensitive” (Renzetti & Lee, 1993), specific sorts of data are viewed as involving danger, and some kinds of participant are often believed to be more vulnerable and therefore seen as needing protection.¹ In the

case of the use of visual methods with children, for example, significant ethical concerns relate to the publication of images. The fear is that these could be misused, that those pictured will be immediately recognizable to people who already know them, or, perhaps, that they will become identifiable by others, with negative consequences. Various strategies are used by researchers to minimize this danger. For example, Flewitt (2005) mentions “fuzzifying” faces to protect identities and the possibility of producing sketches of video stills and photographs that minimize identifiability. However, these techniques have themselves been challenged on ethical grounds; for instance, as “an example of the ‘Othering’ of young children in research” (Nutbrown, 2010, p. 3). The response of many researchers who use visual data to these risks of harm is to take what precautions against them seem reasonable in the circumstances and very often also to obtain informed consent from participants, as far as this is possible.

The people being studied are not, of course, the only ones who can be harmed by research. Others include organizations from which funds were obtained, institutions within which researchers work, colleagues in those institutions, journals or publishers, broader groups or categories of person with whom the researcher has not had direct contact but who might be affected by the publication of findings, and even researchers themselves (see Lee, 1995; Lee-Treweek & Linogle, 2000; Lyng, 1998). Indeed, in some contexts, qualitative researchers may be exposed to the risk of physical harm, whether of assault (Jacobs, 2006; Kelly, 2004) or disease (Lankshear, 2000). Warwick (1982) mentions two other relevant types of harm in relation to researchers: legal jeopardy, the danger of prosecution and even imprisonment, and the psychological effects arising from engaging in deception and manipulation, both in terms of feelings of guilt and self-doubt and also as effects on personal behavior outside of research contexts (see also Homan, 1980). One context in which these dangers take on particular significance is where research is carried out by a team: here, those taking on a leadership role will be responsible, at least to some degree, for the welfare of their junior colleagues (Bloor, Fincham, & Sampson, 2010).

Respecting Autonomy

It is often argued that, in carrying out research, people’s autonomy should be respected; in other words, their capacity and right to make decisions about their own lives should not be undermined.

This value underpins the frequently emphasized requirement of *informed consent*.²

Gaining informed consent, though, is not always necessary (e.g., public domain materials), is not always possible, and is not a straightforward matter (see Hammersley & Traianou, 2012, chapter 4). Where it is believed to be required in principle but is not possible to obtain, researchers are faced with a decision about whether to abandon their investigations or to continue without the consent of those being studied. This issue may arise, for instance, where the real names and contact addresses of participants are not known, where there is a high turnover of participants, or where seeking informed consent would seriously disrupt the processes being studied. Such problems can be faced, in particular, in some forms of online research (Svenigsson-Elm, 2009).

Among social science researchers, there has been much debate around whether *covert* research is ever justified and, if so, under what conditions (Bulmer, 1982; Herrera, 1999; Leo, 1996). Some commentators argue that it is virtually never legitimate (Bok, 1978; Shils, 1959; Warwick, 1982). Others insist that covert research is an acceptable and necessary strategy in particular research settings (Calvey, 2008; Douglas, 1976; Homan, 1980). These discussions have identified a range of considerations that need to be taken into account. In my view, however, any judgment about whether covert research is legitimate must be made in relation to specific cases rather than being formulated either as a general prohibition or even as a globally permissive statement. This is because covertness can vary significantly and so too can conditions in the field that are relevant to making a judgment about its legitimacy.

As already noted, there are also some types of research in which the requirement of informed consent may not apply. This is true in the case of publicly available documents or of observations made in public settings; although, as I discuss later, there are debates about what counts as public or private material or settings. Once again, these issues arise in some forms of online research, as well as in more traditional forms of research.

When informed consent is judged to be a requirement, the researcher must reflect on how this can best be secured: what is needed if people are to be properly *informed* (how much information should be supplied, how should people's level of understanding be gauged, and so on), and how can one be sure that people are in a position to freely *consent* or decline to be involved in the research? In the context of formal interviews, this may be relatively

straightforward. It is much less so when access to a setting is being negotiated to observe events there and to engage participants in informal conversations. Gatekeepers may exert considerable control over access to settings, effectively speaking on behalf of others involved. So, the question arises: is *their* informed consent sufficient, or does the informed consent of all participants need to be secured, and, if so, how is this to be achieved?

Moreover, there are often significant cultural differences about who can and should give consent for whom to be involved in what. In many Western societies, it is usually assumed that, in principle, adults ought to be treated as free agents in terms of their decisions, even if this freedom is curtailed in particular institutional or group settings. By contrast, in the case of children and of adults who have learning disabilities or mental health problems, disputes center on their capacity to consent in a manner that takes account of their own interests (Heath, Charles, Crow, & Wiles, 2007). Finally, in some non-Western cultures and in some subcultures within Western societies, autonomy is given little weight. The head of a kin group or a community leader may be regarded as having the proper authority to give permission for members of the family or community to participate in research. And, once permission has been granted, there may be an obligation on those members to cooperate with the researcher. Such cultural differences are important in ethical as well as practical terms and can pose serious difficulties: should the researcher respect the conventions of the established culture or insist that individuals freely consent?

Finally, another important issue concerns by what means consent is obtained; in other words, what counts, and should count, as consent having been given? Here, questions focus on whether consent can be implicit as well as explicit (Herrera, 1999) and about whether explicit consent can be oral or must take the form of a written contract. Aside from the question of whether the people concerned are literate, there is cultural variation in interpretations of oral and written agreements. For example, insistence on written consent may be regarded as insulting or threatening by some people and may have undesirable effects on the research relationship (see, e.g., Colic-Peisker, 2004, p. 88).³

Privacy

In some important respects, the conflict between the demand for publicity built into the mission of social science and a commitment to respecting

privacy is at its sharpest in the case of qualitative research. It makes a considerable difference whether what is being studied is a public or a private setting, and this distinction can also be applied to the sorts of information that a researcher is seeking. But how is what is public and what is private to be determined? This is far from straightforward and can be a matter of dispute. Researchers' judgments about privacy may be affected by the field relationships that develop around them or by those that they are concerned with cultivating (see Hey, 2002; Hudson, 2004). Interview questions, the use of diaries, or the collection of visual data may result in "disclosure of behaviours or attitudes which would normally be kept private and personal, which might result in offence or lead to social censure or disapproval, and/or which might cause the respondent discomfort to express" (Wellings, Branigan, & Mitchell, 2000, p. 256). Here, too, judgments about what is private, how private it is, and to what extent it is appropriate to try to elicit information about it must be made.

Equally important in discussions about privacy is the ethical question of whether it is legitimate to investigate a particular topic that is seen as *sensitive*; in other words, a topic that touches on private matters, as Goodrum and Keys (2007) note in discussing studies of bereavement and abortion. A related question is whether it is acceptable to study a topic that others, perhaps including the people from whom data are to be collected, are likely to regard as private, irrespective of whether the researcher holds this view. As Renzetti and Lee (1993) point out, however, predictions of what will prove to be sensitive inquiries as far as participants are concerned are open to error, and judgments regarding sensitivity, even about the same topic, will vary across audiences. For instance, as they note, some groups—religious fundamentalists, for example—"quite literally regard research into their beliefs and activities as anathema" (Renzetti & Lee, 1993, p. 6). This sort of tension, sometimes formulated in terms of conflicting "ways of knowing," has been at the center of debates about research on "indigenous cultures" (see Chilisa, 2009; Denzin, Lincoln, & Smith, 2008; Smith, 1999; Walker, Eketone, & Gibbs, 2006).

Online qualitative research presents one area in which privacy has been a particular issue, especially the kind that employs naturally occurring online data. Although the nature and ethos of the internet can give rise to the assumption that it is a public domain or sphere in which whatever is available can automatically be treated as open to legitimate use

by researchers, there is considerable variation in the character of websites and the material they contain.⁴ Judgments about their status as public or private need to be made and are frequently contentious. Furthermore, as Allen (1996) has pointed out, different parts of the same site can vary in this respect. Therefore, as with physical locations, a range of considerations might be taken into account in deciding what is and is not private or *how* private it is. One criterion concerns the nature of the material: the extent to which it relates to the sorts of experiences, activities, or locations that would generally be deemed private. This is not always clear-cut. For example, in her study of a Swedish web community, Sveningsson-Elm (2009, p. 82) argued that the users' practices suggested that they did not consider their personal pages—including their photo albums, diaries, and personal profiles—as private. By contrast, Hudson and Bruckman (2005, p. 298) have argued that "people in public online environments often act as if these environments were private." Another criterion is the degree to which the website is accessible to anyone. In this respect, too, there may be variations across different parts of the same website, with some content hidden and only available to those invited to gain access, as for example with private rooms within publicly accessible chatrooms (see Bakardjieva & Freenberg, 2001).

Up to now, I have concentrated on the ways in which qualitative researchers might invade privacy, but equally important is how researchers handle the data they collect, given that some data may be private or secret, and how they report and disseminate their findings. The precautionary principle that usually operates here is *confidentiality*, and there are a number of strategies researchers use to protect it. The most common one is *anonymization*, which involves replacing the actual names of participants with invented ones. A second strategy used by researchers is to omit from accounts any personal characteristics of people or contextual features of places that may allow them to be identified; alternatively, these may be changed to provide disguise (see Hopkins, 1993; Piper & Sikes, 2010; Sparkes, 1995). It is important, however, to remember that anonymity is a matter of degree. In being referred to in research reports, people are not either identifiable or anonymous. Rather, their identities will be more or less difficult to recognize by different audiences. And sometimes anonymization may not succeed in preventing their being recognized by some people. Aside from the practical difficulties associated

with anonymization, there have also been questions about whether it is a legitimate strategy for researchers to adopt (Nespor, 2000; Richardson, 1973, p. 45; Walford, 2002, 2005, 2008). It has been argued, for example, that if researchers cannot absolutely guarantee that anonymity will be preserved and confidentiality thereby protected, then they should not promise it. Others point out that replacing the names of people—and especially of places—with pseudonyms can lead to inaccuracy: it may prevent readers from using background knowledge that they already have to understand what is reported. Anonymization has also been questioned on the basis that participants sometimes *want* to be named in research reports and/or want their organization and community to be identifiable (Grinyer, 2002; Wiles, Crow, Heath, & Charles, 2008). Indeed, it has sometimes been insisted that informants *own* the data that they have supplied and that their link with such data should not be broken (Lincoln & Guba, 1989, p. 236; Simons, 2009; Walker, 1993); that they ought to be viewed as authors of the data, so that they have a right to be named as sources.

In my view, confidentiality as regards data is an important ethical principle in qualitative research, and anonymization is a useful strategy in achieving it. To abandon it would make some research impossible and damage the quality of much of the rest. But sometimes it will not be appropriate, and there is a range of considerations that need to be taken into account in deciding about this, including the nature of the participants and the researcher's relations with them.

I have outlined some of the central ethical issues involved in qualitative inquiry and some of the complexities surrounding them. In the remainder of this chapter, I focus on the role that these should play in the practice of research. As I noted earlier, ethical issues have come to be treated as much more central to good research than in the past.

The Increased Centrality of Research Ethics

Two developments have changed the perceived significance of research ethics for many researchers today: the rise of ethical regulation, and the emergence of conceptions of qualitative inquiry that treat it as an essentially ethical enterprise.⁵

The Rise of Ethical Regulation

The move toward ethical regulation of social science began many years ago when some social science subject associations established codes to guide

the behavior of their members. This was stimulated in part by earlier developments within medical research after World War II, these being prompted by the appalling experiments carried out by Nazi doctors on people in institutions and concentration camps. The Nuremberg Code of 1947 specified ethical principles that should guide medical experiments, and these were later applied more widely, notably in psychology. They were subsequently clarified, developed, and supplemented in the World Medical Association's Helsinki Declaration of 1964, and in the Belmont Report of 1979 in the United States. The last of these was prompted by further scandals, such as the Tuskegee project in the southern United States, in which African-American men were not given treatment for syphilis in order to allow researchers to understand the variable course of the disease.⁶

Also important for the development of ethics codes by social science associations were various controversies about the role of social research in relation to foreign policy. For example, during World War II, some anthropologists in the United States were employed by a US government agency that was responsible for the internment of people of Japanese descent in California (Mills, 2003; Opler, 1986; Starn, 1986; see also Price, 2008), and this led the Society for Applied Anthropology to produce a code of ethics in 1948, probably the first social science association to do so. In the 1960s and 1970s, a series of further controversies surrounded anthropologists' and other social scientists' involvement in government-sponsored projects concerned with military operations and counterinsurgency in Latin America and East Asia (Wakin, 2008, chapter 2).⁷ The most famous was Project Camelot, in which anthropologists, sociologists, political scientists, and psychologists were to be funded as part of a proposed CIA project concerned with "assessing the potential for internal war within national societies" and identifying "those actions which a government might take to relieve conditions which are assessed as giving rise to a potential for internal war" (Horowitz, 1967, p. 5).

In social science, the ethics codes developed by professional associations did not amount to ethical regulation, strictly speaking, by contrast with medicine, in which codes were generally accompanied by procedures through which complaints could be made and punishment administered. Although medical associations could often prevent a member from continuing to practice, at least within its jurisdiction, this was rarely if ever possible for

social science associations. In short, their codes were largely advisory in function, with little or no policing of members to ensure compliance.

In recent decades, however, a major shift has occurred in the ethical regulation of social science. One aspect of this is that the locus has moved from professional associations to the organizations in which social scientists are employed or with which they must deal in carrying out their research: universities, research institutes, and research sites like hospitals. This process began in the United States, with the introduction of federal regulations in the early 1980s requiring the establishment of Institutional Review Boards to assess research proposals within all institutions receiving funds from what was then the Department of Health, Education, and Welfare. Because future federal funding was contingent on the formation of these boards, universities were forced to comply. Although the review board system was primarily concerned with medical research, the remit of these boards covered social science as well, and their flexibility in interpreting ethical principles across research fields has varied considerably (Israel & Hay, 2006, pp. 41–45). Furthermore, over time, a process of “ethics creep” has involved an intensification of regulation and its extension to examine all aspects of the research process (Haggerty, 2004).

In the United Kingdom, the shift toward this kind of ethical regulation was more recent. Here, too, it began in the field of health, with the Department of Health requiring hospitals to set up research ethics committees and later providing guidelines for the establishment and operation of these committees. More recent changes have led to much tighter regulation through the National Health Service (NHS) Research Governance Framework (RGF), which was introduced in 2001 and now covers most research conducted in healthcare settings in the United Kingdom, not only medical research.

These changes in the health field were important factors in stimulating increased regulation across UK social science, and this parallels similar moves in many other countries (van den Hoonaard, 2002). Ethics committees had already existed in some universities, but these had usually been concerned with medical research and/or with the treatment of animals by biologists and of children by psychologists. However, in 2005, the Economic and Social Research Council (ESRC) published its *Research Ethics Framework* (2005), and this was formulated very much in the language of “research governance.” In effect, it required that most research proposals coming to it be subject to vetting procedures within

universities before they could be funded. In the wake of this, universities extended the remit of existing ethics committees to deal with social research or set up new procedures, and this continues to be done in a variety of ways and with differing degrees of operational effect. More recently, regulation has been tightened and extended through a revised framework (ESRC, 2010; Stanley & Wise, 2010).

The most significant aspect of this shift from codes to regulation is that whereas, even within professional medical associations, the application of codes had been retrospective, responding to complaints, the operation of the new institutional review boards and ethics committees is prospective, effectively determining whether particular research projects can go ahead. Furthermore, it frequently entails a “mandatory requirement for the prior and meticulous review of social research proposals by groups that are representative of a wider constituency than the research community” (Homan, 1991, p. 17). In other words, research proposals are to be judged not simply by members of the relevant research community but by committees that include academics from across diverse disciplines and, increasingly, lay representatives as well.

It should be clear that this rise in ethical regulation has made the consideration of ethical issues a much more central concern for researchers, at least in the sense that they are now forced, prospectively, to give an account of ethical considerations in relation to their research and how they will deal with them. Moreover, they may well have to engage in considerable negotiation with ethics committees to gain agreement to proceed, negotiation that is by no means always successful. A common requirement of regulatory bodies is that research be carried out to “high” or even to “the highest” ethical standards (see, e.g., ESRC, 2010; Gardner 2011), and, on paper at least, this prioritizes research ethics in relation to other aspects of the research process.

Of course, the nature of what is demanded in the context of ethical regulation is open to question and has been subject to considerable criticism. The reorganization and tightening of ethical regulation has had particularly sharp consequences for qualitative research because the model of enquiry on which regulatory guidelines and arrangements have been based is usually at odds with its character. This biomedical model assumes clear specification of objectives and means of achieving them at the start of the research process, a preoccupation with the testing of hypotheses, and the scheduled production of promised

outcomes. It also presumes that research consists of the administration of research instruments in researcher-controlled environments (Reiss, 1979). By contrast, qualitative research generally operates on the basis of a flexible and emergent mode of research design in which the task—in the early stages of data collection at least—is to clarify and develop understanding of the research problem. As a result, it is difficult for qualitative researchers to anticipate, at the beginning, what sorts of data will need to be collected. Furthermore, qualitative research typically takes place in “natural” settings, over which researchers have little control. Even when interviews are involved, these are usually relatively unstructured in character and carried out in territory that is not controlled by the researcher. All these features make it difficult to anticipate what contingencies might arise at various stages of the research process and to plan in any detail how ethical issues will be dealt with.

The growth in ethical regulation has generated a considerable literature. Some of this has concerned the principles on which regulation should be based, some has been designed to assist researchers in thinking about research ethics in ways that allow them to navigate the requirements of ethics committees, and a considerable amount has been concerned with the negative effects of ethical regulation for qualitative work.

The other factor that I mentioned as making ethics more central for researchers is the rise of views of qualitative inquiry that treat it as *essentially* ethical in character, in one way or another. I examine these in the next section.

Changes Within Qualitative Research

Qualitative enquiry raises distinctive ethical issues because, as already indicated, it generally involves emergent and flexible research designs and usually entails collecting relatively unstructured data in naturalistic settings. And there has been much discussion of the ethical issues it generates since the middle of the twentieth century. This was often stimulated by particular studies that attracted adverse publicity or were seen as involving severe problems (Hammersley & Traianou, 2012, chapter 1).

In addition to debates around particular studies, increasing attention to research ethics has also been generated in recent years by the proliferation of sharply discrepant approaches to qualitative research. There are now deep divisions within the research community, relating not just to the means

to be employed, but also to what is seen as the goal of and rationale for qualitative research.

In early debates about quantitative versus qualitative approaches, one criticism made of quantitative work concerned ethics. It was argued that quantitative research tends to force people’s responses into categories determined by researchers, thereby reducing them to objects that can be counted and represented as statistics, rather than portraying them as persons and agents (see, e.g., Mills, 1959, chapter 5). These features were seen as closely associated with the practical functions served by quantitative research, notably its use by governments and big business to control and manipulate employees, citizens, and consumers.

However, with the rise in influence of qualitative work and its fragmentation into competing approaches, ethical criticisms came to be directed at some older forms of this work as well. For instance, the involvement of early anthropological ethnography in the operation of European colonialism was highlighted, with the suggestion that it continues to serve as an arm of neo-colonialism (Asad, 1973; Lewis, 1973; Pels, 1997). Furthermore, there was the claim that qualitative research is, if anything, even more capable of intruding into people’s private lives than quantitative work. Through participant observation, researchers can gain direct access to these individuals, observing what they say and do at firsthand. This has sometimes been denounced as surveillance (Nicolaus, 1968; see also Barnes, 1979, p. 22) or voyeurism (Denzin, 1992). Similarly, open-ended interviewing was criticized, notably by feminists, on the grounds that it could encourage people to disclose aspects of their past and of their experience that they might wish to keep private, with disclosure being a result of false rapport strategically developed by interviewers (Finch, 1984). More fundamentally, the asymmetrical roles played in the research process by researchers in relation to those they are researching came to be challenged as constituting a “hierarchical” relationship that involves the exercise of power and is fundamentally exploitative in character (Stacey, 1988).

In addition to these charges, there were also criticisms that much qualitative research is politically trivial, in the sense that it has little or no impact in changing the world and is, therefore (it was argued), of little or no value. For example, at the height of the Vietnam War, when radicals were challenging their profession to take a stand against it, Gjessing (1968, p. 397) suggested that unless the whole direction of anthropological enquiry were changed,

anthropologists would be “playing an intellectual game in which nobody outside our own tiny circle is interested.”

To remedy these defects, it was insisted by many that qualitative research must be aimed directly at emancipation; in other words, at challenging oppression, social inequalities, or human rights abuses. Thus, over the course of the 1970s and 1980s, there were calls for a “liberation anthropology” (Huizer & Mannheim, 1979) and for qualitative inquiry to “become critical” (see Hymes, 1972). Sometimes, it was argued that a form of participatory action research was required (Hall, Gillett, & Tandon, 1982), one that recognizes the agency of those who need political support in overturning the status quo. Not surprisingly, these criticisms and proposals were often formulated in ethical terms.

Later, these differences in attitude about the methods and goals of social research and about what counts as ethical research practice deepened and diversified, sometimes turning into fundamental philosophical and political divisions, with some approaches treating the realization of particular ethical values as the primary consideration in qualitative enquiry. For example, many feminists criticized mainstream social research for its commitment to abstract ethical principles, proposing instead an ethics of care that gives central concern to the *interdependence* of human beings and their responsibilities to each other, for adopting Western conceptions of the subject, and/or for maintaining a distinction between researchers and researched that reinforces power differences and thereby undermines the production of “authentic” data (Mauthner, Birch, Jessop, & Miller, 2002; Miller, Birch, Mauthner, & Jessop, 2012; see also Hesse-Biber & Leavy, 2006). Within disability studies, there was growing resistance to research by nondisabled researchers (Barnes, 2009; Oliver, 1992), just as, under the influence of antiracism, there were challenges to whites studying blacks. In the field of childhood studies, which emerged in the 1980s, there has been an insistence that research must be designed to secure children’s rights, that it must represent their voices, and, increasingly, that children should themselves carry out research (Alderson, 2000; Kellett, 2010). Parallel developments have also taken place in relation to research on “indigenous communities” (Chilisa, 2009; Denzin et al., 2008; Smith, 1999).

Although these developments have been strongly shaped by sociopolitical changes and “new social movements” like feminism and disability activism, they have also been influenced by shifts in

ideas about the nature and value of social scientific research (Hammersley, 2013). The predominant view in the 1960s and 1970s insisted that the sole operational aim of inquiry is to produce knowledge, albeit knowledge that is relevant to some general human interest, to a body of disciplinary knowledge, and/or to a public policy issue. This traditional view, closely associated with the concept of science, was always subject to challenge but, from the 1980s onward, many qualitative researchers began to distance themselves from it, looking more toward the humanities and arts. In part, this reflected wider cultural challenges to the status and character of science and also attacks on Enlightenment thinking, inspired by critical theory and post-structuralism, portraying it as legitimating oppression and as hiding Western interests behind the veil of objectivity and universalism (see, e.g., Clifford & Marcus, 1986).

In important respects, these developments transformed research ethics and gave it heightened relevance. Ethical issues have come to be seen as crucial both in relation to the goals of research and to how it is carried out. Where, previously, ethical considerations set boundaries within which inquiry could legitimately be pursued, increasingly today they are seen as defining the nature of the task or as demanding exemplification in the research process.

The Dangers of Moralism

Although research ethics is certainly an important topic, some fundamental questions can be raised about the ways in which ethics has become central to research as a result of the rise of ethical regulation and of changes in qualitative inquiry. In many respects, this reflects what might be labeled “moralism,” a term that has been defined as “the vice of overdoing morality” (Coady, 2005, p. 101; see also Taylor, 2012).⁸

Perhaps the most obvious expression of moralism is the claim that qualitative research is, or should be, *essentially* ethical. For example, Clegg and Slife (2009, p. 36) argue that it is “an inherently ethical enterprise” whereas Mertens and Ginsberg (2009, p. 2) insist that “ethics is foundational to the *telos* of the research enterprise.” Similarly, Caplan (2003, p. 3) has claimed that “the ethics of anthropology... goes to the heart of the discipline: the premises on which its practitioners operate, its epistemology, theory and praxis.” In other words, it is concerned with answering the question “*What* is anthropology for? *Who* is it for?” Many qualitative researchers outside of anthropology would concur

that what is at issue here is the whole rationale for and orientation of qualitative inquiry (see Denzin & Lincoln, 2011).

There is an important sense in which this claim about the centrality of ethics is true: social research is necessarily founded on certain intrinsic value-commitments. Traditionally, these centered on the goal of producing knowledge, but today the desirability, or even possibility, of this has been opened to question, and the values often seen as central to qualitative research are of a very different kind: they are practical rather than epistemic values, concerned, for example, with promoting “equality,” “inclusion,” or “emancipation.” In my view, this move away from treating only epistemic values as lying at the center of qualitative research amounts to moralism. This can take at least two forms:

1. The belief that other values than *truth* should be treated as *integral to the goal of research*, so that researchers must direct their work toward, for example, promoting justice, emancipating or giving voice to marginalized groups, or serving other practical activities of various kinds, such as the promotion of health or education.

2. The requirement that *in the course of carrying out their work* researchers must seek to “realize” one or more practical values, that they should adhere to “the highest ethical standards” as regards, for instance, avoidance of harm, protection of privacy, respect for autonomy, equity, care, or some other nonepistemic value.

It is not uncommon to find these two kinds of moralism combined, leading to the demand, for instance, that research both be aimed at producing findings that increase social justice *and* be carried out in ways that exemplify this value. In my view, both these forms of moralism need to be subjected to fundamental assessment.

Redefining the Goal of Qualitative Research

It is a feature of all specialized occupations that they involve the adoption of a relatively narrow perspective, focusing on a particular task and what is directly relevant to it, thereby downplaying other matters that are important from other perspectives. It is precisely from this narrowness that increased gains in tackling the specific task arise. Although it is important to recognize that this specialization also involves losses, it should not be assumed that the gains are available otherwise. Thus, research as a specialized activity maximizes the chances of

producing sound knowledge. Although it should always be practised in ways that are constrained by practical values, these being primarily concerned with how other people ought to be treated, this is quite different from treating those extrinsic values as constituting the goal of the occupation.⁹

In these terms, then, the common claim, for example, that a major function of qualitative research is to “give voice” to marginalized groups involves a fundamental misconception. The task of any research project is to answer a set of factual questions. Although this may well involve drawing data from people whose views are rarely heard or listened to, it will also usually be necessary to *interpret* these views; and, when they are being used as a source of information about the world, to *evaluate* their likely validity. In addition, it will almost always be essential to draw data from other people who are not regarded as marginalized or oppressed, and who may even be viewed as oppressors (Becker, 1964). Furthermore, it is important that the validity of *their* accounts is not simply dismissed (Hammersley, 1998).

Also ruled out is any argument to the effect that research must be directed toward benefitting the people studied, an idea that underpins the notion of participatory research. For example, in the field of childhood studies, it is often insisted that research should not be carried out *on* children but always *for* and *with* them, treating them as having a right to participate in research decision-making (Alderson & Morrow, 2011). Similar views are to be found, as regards other sorts of research participant, among feminists (Fonow & Cook 1991; Mies, 1983; Miller et al., 2012) and disability researchers (Oliver, 1992; Barnes, 2009) and in relation to research about “indigenous” communities (Smith, 1999).

In my view, these arguments involve a violation of the distinctive character of research: a failure to recognize that it is a specialized activity whose distinctive and exclusive goal is the production of knowledge.¹⁰ In other words, they undermine the very concept of research as a professional occupation. In particular, the concept of participatory inquiry amounts to an attempt to erase the researcher role and the responsibilities and license intrinsically associated with it.

Realizing Practical Values Within the Research Process

The second kind of moralism I identified is not concerned with the *goal* of research but rather with the *means* by which it is pursued. Here, the

requirement is that researchers seek fully to exemplify some set of practical values, such as avoidance of harm, protection of privacy, respect for autonomy, equity, care, in how they carry out their work. Alternatively, there is the more generalized demand that they abide by “the highest ethical standards,” a phrase that, as noted earlier, is common in statements underpinning ethical regulation.

Examples of this second kind of moralism include the idea that obedience to the criminal law is *always* required and that all infringements must be reported, or the treatment of informed consent as an absolute human right (Alderson & Morrow, 2011; Homan, 2001). In my view, these kinds of ethical absolutism amount to an unrealistic constraint on inquiry. For example, the insistence that the law must always be obeyed would make some kinds of qualitative research very difficult, if not impossible. This is most obviously true in the field of criminology, but the problem extends beyond that area, since deviance of one kind or another is a feature in many settings. The point here is not that researchers should feel free to break the law, whether in their own country or some other, whenever it is convenient, nor that they should expect immunity from prosecution. It is rather that there may be occasions when this is necessary for the research and justifiable in those terms. Relevant here, of course, is the seriousness of any offence. It is not uncommon for qualitative researchers to witness illegal behavior and to break the law by not reporting it. Much more rare is when researchers themselves commit serious offences, as in the famous example of Whyte’s (1993, pp. 312–317) participation in election vote-rigging. Similarly, I outlined earlier some of the complexities surrounding informed consent and why this sometimes cannot be achieved in qualitative inquiry.

Adherence to “High Standards”: An Indefensible Requirement

My argument is that whereas qualitative research is properly constrained by practical values, what these values mean and what weight should be given to them in any particular situation must be shaped by what is required if the production of knowledge is to be pursued effectively. For these reasons, practical values will sometimes need to be compromised. What can reasonably be expected of qualitative researchers is *not* adherence to the highest standards but rather that their behavior is *acceptable* in terms of practical values, *taking account of the constraints operating in the situations concerned.*

It is also important to remember that social scientists are members of a profession operating *within* societies and that all they can distinctively claim is a high commitment to a specific goal and to the values associated with this, not some general ethical superiority.

A label that could be applied to the position adopted here is “Machiavellianism” (Hammersley & Traianou, 2011), a term that carries an evaluative load that, like “moralism,” is ambiguous if not downright negative. However, contrary to what is sometimes assumed, Machiavelli did not propose that rulers and other political agents should pursue evil ends. Rather, he argued that they will often have to use means that are regarded as morally questionable, such as deception, and even sometimes those that are abhorrent, like war, *in order effectively to pursue ends that are good.* According to Strauss (1975, p. 84), Machiavelli was the first of the early modern political philosophers, whose ethical thinking starts not from “how people ought to live,” in the manner of the ancients, but rather from “how people actually live.” In Max Weber’s terms, Machiavelli rejected an “ethics of ultimate ends” in favor of an “ethic of responsibility” (see Bruun, 2007, pp. 250–259).

One of the problems with the second kind of moralism I discussed, then, is that it is premised on an unrealistic view of human nature and society. Conflicting ideals and interests, and struggles over these, are endemic in social life; and, as a result, the use of coercion, manipulation, and deception is widespread. Given this, moralism is not a viable basis for carrying out any activity, including qualitative inquiry (Douglas, 1976; Duster, Matza, & Wellman, 1979; Littrell, 1993). If researchers are to get their work done in *the world as it is* and produce reliable knowledge, they will often have to engage in actions that fall short of “the highest standards.”

Another way of trying to capture the point I am making is the idea that researchers must claim a certain moral license if they are to pursue their task effectively. This is also true of many other occupational roles, notably but not exclusively those labeled as professions. For example, it is the task of the doctor to try to secure or preserve the health of patients, not to save their souls or serve the interests of a kin-group or a nation-state. Moreover, in pursuing this narrowly specified task, it may be necessary to use means that, from the point of view of some extrinsic values, are undesirable. For example, doctors and other medical personnel will often find it necessary to cause embarrassment or

pain, and perhaps also to turn a blind eye to legal as well as moral offences (e.g., drug use). Similarly, the task of lawyers is not to aim directly at achieving justice; instead, they are obliged to be partisan on behalf of their clients and to operate in terms of the existing law, downplaying some aspects of a case in favor of others with a view to serving the client best. Furthermore, in pursuing their work, they can demand answers from witnesses to highly intrusive questions *in public* and challenge their honesty in order to undermine the persuasiveness of unfavorable evidence.¹¹

So, in serving their goals, occupations may need to be allowed to breach some moral rules that would normally apply. If, by contrast, it is insisted that these rules are always fully enforced, that “high standards” are adhered to in terms of *applying extrinsic values*, then the scope for exercising the discretion needed to pursue specialized occupations, and thereby to achieve the benefits they offer, will be reduced considerably. In my view, this is an argument that applies to social research.

What Sorts of License Can and Should Qualitative Researchers Claim?

For one thing, in collecting data, social scientists may find it necessary to tolerate, and risk being seen as condoning, behavior that they believe (and that others would believe) is wrong. I should perhaps stress here that my argument is not that all immoral or illegal acts must be tolerated, only that researchers must have the leeway to tolerate some such acts where they judge this to be necessary and defensible in doing their work. This includes tolerating the expression of beliefs that one finds offensive or disturbing (Huff, 1999). If the researcher is not able to be tolerant in this way, then access to much data may be blocked or made relatively inaccessible in many fields of inquiry. Similarly, it may sometimes be necessary to deceive people, at least passively (e.g., through not correcting misapprehensions), if the data required are to be obtained. This is most obviously true in the case of groups and organizations that seek to exercise considerable power over their members and over their external environments: from political and business elites, through state and commercial agencies of various kinds, to exclusive religious or political groups. Such deception may also be necessary in cases where individuals or groups have a hostile attitude toward science or social research (see, e.g., Homan, 1980). A further example is that it may be necessary to ask questions whose implications could be taken to be

politically questionable, say, sexist or racist. Equally important, researchers may need to entertain lines of argument whose potential implications could be viewed as objectionable, distressing, or repulsive by lay audiences, and perhaps even by the researcher her- or himself. Any insistence that researchers be “authentic,” in the sense of *fully* living up to their own personal values or to those of others, would put very serious obstacles in the way of pursuing social research, often ones that simply make it impossible to do it well.

Of course, in the case of professions like medicine and law, the moral license claimed is justified by appeals to the benefits produced (both for particular individuals and for the wider society), whereas with qualitative inquiry it might be argued that there are no equivalent benefits, or at least that the benefit is much less. Thus, for academic research at least, there is no client group, and the knowledge it produces is sometimes seen as trivial. However, the balance between the level and kinds of “moral deviance” involved in the work of different occupations and the benefits they generate is a matter of judgment, and one about which there will often be disagreement. For my part, I believe that the minimal moral license required to pursue qualitative research *is* justified by the potential benefit it can bring in terms of knowledge and understanding of the social world.

Ethical Reflexivity and the Problem of Being “Too Ethical”

What lies at the heart of moralism, of all kinds, is the assumption that it is *impossible* to be “too ethical” (see Leiter, 2001; Loudon, 1988). And, closely associated with this is an unrestrained form of ethical reflexivity that generates the conclusion that social research involves a high risk of severe ethical dangers for the people studied, so that rigorous precautions must be taken to protect them; or that, in order for research to be worthwhile and therefore ethically justifiable, it must aim at more than the “mere” production of knowledge. Also involved is the assumption that there are value judgments that could frame research that everyone would or should accept and whose implications for particular situations are quite clear and determinate. However, none of these assumptions is sound. Although it is essential that researchers continually adopt a reflexive stance toward their work—as regards ethical, methodological, and other issues—there are significant limits to how much and what kinds of reflexivity they should exercise, in the sense of what they

should treat as open to question. Questioning all assumptions leads to an inability to engage in any form of action.

Perhaps the other profession to which qualitative research approximates most closely in character is investigative journalism. And the position I am adopting here is similar to the attitude of Janet Malcolm toward the ethics of her profession. Journalists, she suggests, face a “moral impasse.” In a famous opening sentence, she declares that “every journalist who is not too stupid or too full of himself to notice what is going on knows that what he does is morally indefensible” (1991, p. 3). However, she does not take this as grounds for abandoning the occupation or for adopting a highly moralistic stance in her work. Rather, her concern is to highlight the difficulties and unavoidable ethical dilemmas involved in investigative journalism. She elaborates on the problem as follows:

Unlike other relationships that have a purpose beyond themselves and are clearly delineated as such (dentist-patient, lawyer-client, teacher-student), the writer-subject relationship seems to depend for its life on a kind of fuzziness and murkiness, if not utter covertness, of purpose. If everybody put his cards on the table, the game would be over. The journalist must do his work in a kind of deliberately induced state of moral anarchy.

She describes this as a “baffling and unfortunate occupational hazard” (Malcolm, 1991, pp. 142–143). And I believe that much the same can be said about qualitative research; although researchers are not faced with the same level of ethical difficulties as the journalist, largely because they are usually able to anonymize the people and places being referred to, whereas journalists cannot.

It is perhaps necessary to re-emphasize that my argument is not that the pursuit of research should be unconstrained by practical values. Some restraint of this kind is essential: researchers should not feel free to pursue their research goals irrespective of all other considerations and costs. The issue is the *degree* to which and *ways in which* nonepistemic values should shape the actions of the researcher and, equally important, who is to make decisions about this. There is no general answer to the question of how much weight should be given to particular practical values; this must be decided on a case-by-case basis. However, what we *can* say is that it is individual researchers or research teams who must decide in particular cases what is and is not acceptable, in light of both intrinsic and extrinsic values. Such decisions

should not be made by funding bodies, gatekeepers, ethics committees, governments, or anyone else. Others can, of course, express views about the decisions that researchers have made and take action on the basis of these; but researchers are not obliged automatically to treat their complaints as legitimate, even if they must nevertheless face the consequences that follow from them.

Conclusion

Whether ethics is seen as central to qualitative inquiry or to social research more generally depends a good deal on what the word “ethics” is taken to mean. It is frequently treated as primarily or entirely concerned with how researchers treat people in the field: whether they minimize harm to them, respect their autonomy and privacy, and so on. If “ethics” is interpreted in this way, then, in my view, ethics is not central to qualitative research, in the sense that it does not form part of its core task, which is to produce knowledge. Ethical considerations, in this sense, relate to what are and are not acceptable means in pursuing knowledge: they represent an external constraint on the selection of methods and strategies in which researchers engage.

However, “research ethics” can be interpreted in a broader sense to include all of the values that are relevant to the pursuit of inquiry. If we interpret the term in this way, then *some* of the values concerned are indeed central to the practice of research. After all, inquiry necessarily depends on the assumption that gaining knowledge of the social world is desirable, and implicated here also is the value of truth. Moreover, the pursuit of inquiry demands a number of virtues: an openness to unpleasant facts that are at odds with one’s preferences, a willingness to consider and address criticism, a commitment to objectivity, in the sense of seeking to minimize the chances of one’s own values and interests leading to error, and so on. These values and virtues are indeed central to the practice of research, of any kind.

Notes

1. Categories of vulnerable participants include, most notably, the very young, people suffering from serious illness, those who have intellectual impairments, temporary (e.g., as a result of the effects of alcohol or drugs) or more long-lasting (a learning disability or mental illness), and those in marginal positions within society. However, others can be vulnerable in particular respects under certain conditions, for example psychotherapists (Oeye, Bjelland, & Skorpen, 2007) and teachers (McWilliam & Jones, 2005). On “researching the vulnerable,” see Liamputtong (2007).
2. For a review of the social science literature on informed consent, see Wiles et al. (2005).

3. On the issue of written consent forms, see Singer (1980), Bradshaw (2002), and Coomber (2002).
4. There is now a considerable literature discussing these new opportunities, as regards qualitative inquiry, and the ethical issues associated with them. See, for example, Hine (2000, 2005), Buchanan (2004), Markham (2005), and Markham & Baym (2009).
5. There are several other secondary reasons for this change. One is the use of new technologies (from digital photography and audio- and video-recording to the analysis of virtual materials from the internet). This has introduced some distinctive problems, or at least it has given old problems a new form (Prosser, 2000; Buchanan, 2004; Wiles et al., 2008; Markham & Baym, 2009). Another factor is data protection legislation, in the United Kingdom and elsewhere, which carries implications for how researchers store and report data and for its deposition in archives and its reuse. For interpretations of the implications of this legislation in the United Kingdom, see Akeroyd (1988), Le Voi (2006), and Alderson & Morrow (2011). On ethical issues and archiving, see Corti, Day, & Backhouse (2000), Thompson (2003), Erdos (2011a, 2011b), and Williams, Dicks, Coffey, & Mason et al. (2011).
6. The Tuskegee case is one of several “atrocity stories” (Dingwall, 1977) used in discussions of research ethics, particularly in justifying ethical regulation. However, it is open to conflicting interpretations (see Shweder, 2004). Kimmel (1996) provides an account of the development of ethics codes in US psychology (see also Diener & Crandall, 1978, pp. 17–22).
7. More recently, there has been concern over the involvement of anthropologists in the invasions of Iraq and Afghanistan (see Fluehr-Lobban, 2008).
8. There is a parallel between moralism and the religious enthusiasm that the philosopher John Locke and others objected to in the seventeenth century as part of their defence of political liberalism (Locke, 1975, chapter 19; see Tucker, 1972).
9. There are some legitimate ways in which extrinsic values can play a positive role in occupational activities. For instance, a lawyer can specialize in providing legal services for the poor, doctors can focus on those in most serious need. And there is also some room for this kind of selectivity on the part of qualitative researchers. Practical research can be designed to provide information required by particular interest groups; for instance, a charity or political organization. And, although this sort of targeting is not possible in academic work, where the aim is to contribute to a body of disciplinary knowledge designed to serve as a general resource, academic researchers can legitimately select topics for investigation in terms of their own values.
10. Fish (2008) provides a typically bullish defence of the traditional role of the scholar, encapsulated in the title of his book: *Save the World on Your Own Time*. Thus, he insists that academic researchers “do not try to do anyone else’s job” and “do not let anyone else do their job.” This echoes a similar sentiment expressed many years ago by Polsky (1969, p. 140), who suggested that if someone wants to engage in social work, or for that matter police work, that is their privilege, but that they should not do so in the name of social science.
11. When a researcher takes on a participant role in the field he or she may also have to exercise moral license distinctive to that role. O’Brian (2010, p. 119) reports that she

had to perform: “the routine tasks of door security work, including vetting customers at point of entry and managing violent and disorderly customers inside venues. I was also required to undertake the gender specific tasks performed by female bouncers such as searching female bodies, monitoring female toilets and performing first-aid tasks.”

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PART 2

Approaches to
Qualitative Research



Philosophical Approaches to Qualitative Research

Renée Spencer, Julia M. Pryce, and Jill Walsh

Abstract

This chapter reviews some of the major overarching philosophical approaches to qualitative inquiry and includes some historical background for each. Taking a “big picture” view, the chapter discusses post-positivism, constructivism, critical theory, feminism, and queer theory and offers a brief history of these approaches; considers the ontological, epistemological, and axiological assumptions on which they rest; and details some of their distinguishing features. In the last section, attention is turned to the future, identifying three overarching, interrelated, and contested issues with which the field is being confronted and will be compelled to address as it moves forward: retaining the rich diversity that has defined the field, the articulation of recognizable standards for qualitative research, and the commensurability of differing approaches.

Key Words: Philosophical approaches, post-positivism, social constructivism, phenomenology, critical theory, rigor

Much ink has been spilled in what have been called the “paradigm wars,” or battles within psychology and related disciplines about how we know—and who judges—what is real. Efforts to establish the legitimacy of qualitative research have often taken the form of vociferous arguments for the merits of qualitative approaches, typically cast in terms of the contrasts between these and the more widely accepted quantitative approaches to knowledge production. More recently, even as the push toward evidence-based practice gains momentum and predictably lists the field toward greater uniformity in acceptable approaches to establishing what can be deemed credible evidence, qualitative approaches have continued to strengthen in presence and broaden in reach. Once a seeming fledgling movement, despite its long but sometimes forgotten history (Wertz et al., 2011), qualitative research in psychology appears to have come of age. This maturity is reflected in the wide variety of philosophical

approaches to qualitative research that have now firmly taken root.

In this chapter, we review some of the major overarching philosophical approaches to qualitative inquiry and include some historical background for each. Here, we offer a “big picture” view and leave it to other chapters in this section (on interpretive, critical, feminist, and indigenous approaches) to take a more fine-grained look at some of the particular fields of thought within these. Described by Denzin and Lincoln (2013) as “a field of inquiry in its own right” (p. 5), qualitative research cuts across disciplines and is represented in many areas of scholarship. We focus here on psychology, but recognize the substantial work done in related fields such as sociology, anthropology, social work, social policy, humanities, and the health sciences, in particular nursing. We cannot possibly do justice to the work that has been done in this arena in this one chapter. Entire volumes (c.f., Denzin & Lincoln, 2013) are

devoted to introducing researchers to these issues. We offer here what we hope is a concise and practical overview of some of the major philosophical assumptions that undergird qualitative research and shape its implementation today.

Once dominated by quantitative methods anchored in positivistic and post-positivistic research paradigms, a greater balance in the use of methodological and philosophical approaches is now being utilized in psychological research (Ponterotto, 2005; Rennie, Watson, & Monteiro, 2002). The importance of qualitative research has long been justified by many on the basis of Dilthey's argument that the distinctive natures of natural science and human science called for different approaches: "We explain nature, but we understand psychic life" (1894/1977, p. 27; as cited in Wertz et al., p. 80). Today, qualitative methods are viewed as being particularly well-suited to addressing some of our most pressing issues and concerns, such as the influence of culture on psychological development and its role in psychological interventions (Ponterotto, Casas, Suzuki, & Alexander, 2010). The rise of participatory action research (PAR), with its emphasis on social change and the empowerment of community participants (Kemmis & McTaggart, 2005), has also required employing a range of qualitative approaches (i.e., focus groups, interviews, participant observation, photo-voice, and storytelling) to collecting data that contribute to the development of the kind of deeper understandings of the experiences of the participants needed to effect meaningful change.

The diversity of qualitative approaches can be dizzying and makes agreement about their appropriate use, in what forms, and according to what standards difficult, if not impossible. It can be challenging for "insiders" to navigate these issues, let alone the novice researcher wading into this terrain. Seemingly simple questions about sample size and composition or the specific steps one should take in data analysis and how to achieve reliable findings can provoke lengthy discussion and even heated debates, with researchers take opposing positions and rooting their justification for these in foundational principles of qualitative research. Even more maddening for some, such questions may simply yield a repeated singular and highly unsatisfying response of "it depends."

This seeming confusion can stem in part from differences in the purpose or aims of the research and in beliefs associated with core philosophies of science embedded within the varying approaches,

namely ontology, epistemology, and axiology (Creswell, 2007; Hays & Singh, 2012; Ponterotto, 2005). At its core, psychological research may be carried out with markedly distinct purposes, such as explaining and predicting aspects of the human experience, increasing our understanding of the lived experiences of different groups of people, or critiquing and changing the current conditions within which we live and strive to grow (Lincoln, Lynham, & Guba, 2013). These different aims may also be carried out using approaches to research that rest on differing foundational assumptions about the nature of our world (ontology) and our knowledge about it (epistemology), as well as the role of values in the process of knowledge production (axiology), that are conceptualized by Hays and Singh (2012) as falling along separate continuums of beliefs.

Ontology is the study of the nature of reality. Within the context of qualitative research, ontology is discussed in terms of beliefs about the existence of some "universal truth" and about objectivity. At one end of the spectrum is a belief that reality is objective and that there are universal truths about reality that can be known. At the other end is a belief that reality is subjective and contextual, and a universal understanding of psychological experiences cannot be obtained because they must always be understood within the contexts within which they are embedded (Hays & Singh, 2012). The crux of these viewpoints is also represented in the terms "emic" and "etic," which are often used in anthropology and cultural psychology. These terms have been used to capture the distinction between experience-near understandings of culture and human experience, or what an insider within a local context would recognize and resonate with, and more experience-distant conceptualizations or abstractions about cultural processes (e.g., Geertz, 1983). Etic can also be thought of as generalizations about human behavior that are universally true and emic as those that are contextually situated and not generalizable, such as local customs (Ponterotto, 2005).

Epistemology is the study of the process of knowing or "how we know what we know" (Guba & Lincoln, 2008; Ponterotto, 2005). It is concerned with how we gain knowledge of what exists and the relationship between the knower—in this case the researcher—and the world. The researcher and research participant may be considered independent of one another. In this view, researchers can use rigorous, systematic approaches to studying

participants objectively or without researcher bias. This results in much attention being paid to rigor in research, particularly in the form of strict adherence to generally accepted systematic approaches to enhancing objectivity and reducing researcher bias. On the other side of the continuum is an understanding of knowledge as being actively constructed by the researcher and participant, who exert mutual influence on one another. Rather than removing or guarding against researcher bias, the dynamic interaction between the researcher and participant is viewed as central to capturing the inherently contextualized experiences of the participant. Issues of rigor remain but take on different meanings and forms. The goal here is not to eliminate bias—because that would be futile—but rather to enhance the trustworthiness of the findings by including and documenting multiple perspectives on the focus of the inquiry. In some cases, this might mean demonstrating that the researcher became immersed enough in the participants' experiences so as to credibly represent and interpret them. In others, this might involve triangulating the data sources and/or the investigators.

Axiology is concerned with how values and assumptions of the researcher influence the scientific process, as well as what actions the researcher takes with the research produced (Lincoln et al., 2013). What place do the emotions, expectations, and values of the researcher have in the research process? Should systematic steps be taken to ensure that the process is kept free of these so that they do not influence the participants and the results? Or is such a pursuit futile and the best a researcher can do is identify, describe, or even attempt to “bracket” (Wertz, 2011) his or her values? Much qualitative research today rests on the assumption that research is “radically relational” and is inevitably shaped, and even intentionally informed, by the researcher's orientation, values, and personal qualities (Wertz et al., 2011, p. 84). In research that seeks to change the status quo with regard to the unequal distribution of power and resources, such as in PAR, the researcher's experience is central to the process and may be key to achieving the intended outcomes of the research (Ponterotto, 2005). With regard to action, the positions range from researcher as distant observer of the study participants to researcher as change agent who is deliberately striving to achieve social justice through the work produced.

In some cases, the assumptions of a researcher may align more neatly along one side of these continuums. For example, a feminist researcher may hold

that there are multiple truths and that knowledge is constructed in relationship with study participants, with the values and assumptions of the researcher integral to the construction of this knowledge. In others, the assumptions may be more mixed, such as a researcher who endorses a constructivist view of reality but views researcher reflexivity as less central to the research process. When these differing ontological, epistemological, and axiological stances go unacknowledged, the differences among qualitative approaches can seem as vast as those between quantitative and qualitative methods. As Camic, Rhodes, and Yardley (2003), among many, have argued, the principle that should unify us is the need for coherence between the nature of our questions and the methodological and philosophical approach taken to answering them.

In the next sections, we review the following major overarching philosophical approaches that guide and structure qualitative research: post-positivism, constructivism, critical theory, feminism, and queer theory. We offer a brief history of each of the approaches; consider the ontological, epistemological, and axiological assumptions on which they rest; and detail some of the distinguishing features while also attempting to capture some of the diversity within them. We also touch on some prominent applications of these approaches to qualitative research in psychology. We recognize that these approaches have been grouped and defined in varying ways and that they defy this sort of tidy categorization. Still, we draw some lines here in an effort to highlight distinctive ideas within each. Also included are discussions of applications of each of the approaches.

Philosophical Approaches

Post-Positivism

Post-positivism grew out of the positivist view of science, and together these have dominated research in psychology for much of the field's history (Packer, 2011). Positivism rests on the ontological assumption that some objective truth or reality exists that is independent of our beliefs and constructions and can be ascertained through direct observation and experience. The efforts of science, thus, are put toward establishing universal laws of nature and, within psychology, universal laws of human development and experience. The attainment of this knowledge and our confidence in it depends on following systematic procedures through which claims about truth can be verified. Hypothesis generation and testing using valid measures of operationally

defined variables are primary tools, and the goal is to be able, with confidence, to generalize the knowledge obtained to some larger general population. Post-positivism introduces the idea that hypotheses can never actually be proven beyond any doubt and that theory should be tested in order to be falsified as well as verified. Issues of validity and reliability are of central importance in research within this paradigm, as are considerations of credible alternative hypotheses to explain the phenomenon being studied.

HISTORY

Post-positivism is rooted in logical positivism, a term coined by a group of scientists, mathematicians, and philosophers in the early 1900s known as the Vienna Circle. Building on the “positive philosophy” of Auguste Comte, but also emphasizing the importance of formal logic in scientific investigation, these thinkers determined that science required a systematic way of organizing our direct observations of experience and sought to inductively build laws of the natural world based on the construction of meaningful and unambiguous logical statements (Packer, 2011). Only statements of fact that could be verified in some way or tested empirically were considered to be meaningful in the scientific endeavor.

Karl Popper (1934/1959) objected to the idea that this kind of inductive construction and confirmation of factual, logical statements that were purportedly free from personal and theoretical bias could lead to certainty about the natural world. Instead, he argued that the laws of science had to be built through a process of falsification or testing of hypotheses. He argued that data disproving hypotheses are more definitive than those supporting them, as in any given study there is always the risk that the data gathered do not accurately or fully represent the real world being studied. The disconfirming case or cases may simply have not made it into the sample drawn for study.

FOUNDATIONAL ASSUMPTIONS

Post-positivism retains the belief in an observable external reality and the existence of universal truths but contends that a fully accurate representation of them can never be achieved with certitude (Popper, 1934/1959). Although things exist beyond our experience of them, it is recognized that our knowledge of this world is socially constructed. Bias is unavoidable. All observations are fallible because they are inherently laden with our individual and

cultural biases. Although we can never get to the truth with any certainty, post-positivists contend that we should continually strive to come as close as we possibly can. Because all measurement is biased and introduces error, issues of reliability and validity are paramount. Great attention is paid to reducing or controlling for bias through the design of the research and the use of clearly defined techniques such as controls groups and multiple forms of measurement or triangulation. This attempt to remove or at least reduce bias extends to the subjectivity of the researcher as well as to the intentions of the research. The researcher is to remain as neutral as possible throughout the research process and should not engage in research in the service of advocacy for any particular position within their field.

From a post-positivist perspective, the existence of multiple worldviews does not extend into a belief in complete relativism and an incommensurability of perspectives—the belief that our differences in experiences and culture mean that we can never understand each other. Whereas we may never achieve objectivity in the true sense of the word, we can employ systematic ways of checking our biases both individually and collectively through engaging in the scientific enterprise within a community of people who critically review one another’s work.

IMPLICATIONS FOR RESEARCH METHODS

Research rooted in post-positivism aims to explain psychological phenomenon by identifying factors that predict particular outcomes and the relationships between them. A priori theory about how things are related is used to guide the research, which then seeks to verify or falsify these theory-based ideas. Having confidence in the findings from such research rests on the rigor with which systematic steps in the research process are employed. Multiple levels of data analysis and taking steps to ensure validity contribute to the rigor of the research, and the results of these studies are typically written in the form of scientific reports similar in structure to that used for the reporting of quantitative studies.

APPLICATION

Grounded theory, a now widely used approach to qualitative research, as traditionally constructed aligns most closely with positivistic and post-positivistic assumptions (Bryant & Charmaz, 2010). It was first developed by Glaser and Strauss (1967) in response to what they considered to be an overemphasis on hypothesis testing and the

verification of theory in sociological research. They argued that the work of theory generation could not be complete and that all human experience was unlikely to be captured and accounted for by the existing grand theories of the time. They put forth grounded theory as a systematic approach to qualitative data collection and analysis to be carried out with the explicit purpose of discovering new theory from data or building new theory from the ground up, rather than by logical deductions from a priori assumptions. Although grounded theory turned the process of scientific inquiry in the post-positivist tradition on its head by beginning with the collection of data to use to ultimately build theory rather than collecting data to prove or disprove existing theory, the foundational assumptions on which traditional grounded theory rests are largely rooted in post-positivism. That said, constructive approaches to grounded theory have also been articulated and widely implemented (e.g., Charmaz, 2006), and others have argued that grounded theory techniques can be implemented using a variety of philosophical approaches (Birks & Mills, 2011).

Traditional grounded theory “accepts that there is an external world that can be described, analyzed, explained and predicted: truth, but with a small *t*” (Charmaz, 2000, p. 524). Part of the intent of grounded theory was to codify qualitative research methods and put forth a systematic set of explicit strategies for carrying out the research process, with the assumption being that following a systematic set of methods would lead to the discovery of real phenomena and the development of verifiable “theories” of them (Strauss & Corbin, 1998). Such work, however, requires getting out into the field to collect rich data on which to build these theories. Some of the defining features of a grounded theory approach are (a) simultaneous data collection and analysis, (b) the development of codes from the data rather than from theory, (c) constant comparison of data at all levels of the data collection and analytic process, (d) theoretical sampling to serve the purpose of theory generation rather than representativeness of the sample, and (e) memo writing to define and elaborate on emerging categories and the relationships among them (Glaser & Strauss, 1967; Strauss & Corbin, 1998).

Social Constructionism

The tenets of the discipline throughout the twentieth century tended to place social constructionism at the opposite pole of experimental social psychology (Jost & Kruglanski 2002), with the idea

being that work in social psychology should fall on either end of the spectrum; you either do quantitative experiments or you engage in qualitative studies that are undergirded by a social constructionist paradigm. Although the two extremes have begun to meet in the middle in recent years, it is important to examine the role that the social constructionist perspective has played in shaping our thinking and work in the field of psychological research.

The notion of social construction first gained popularity in the United States after the publication of Berger and Luckmann’s (1966) seminal book, *The Social Construction of Reality*. Relying on the work of Schutz, Berger and Luckmann argued that all of our understandings and knowledge are socially constructed. The idea is that we create our own reality through social interactions, relationships, and experiences. From the ontological perspective, reality is context- and socially relative, and therefore many realities can exist simultaneously (Berger & Luckmann, 1966; Gergen, 1996). If our reality is constructed, then, too, our knowledge and meanings are derived from social interactions. Individuals hold them in their minds, but the epistemological notion of reality and meanings are not individual in nature but instead are constantly “negotiating meaning” (Gergen, 1996).

This has significant implications for both how we analyze the findings from past research in the field as well as how we shape future research projects. As Gergen (1996) states, “research findings don’t have any meaning until they are interpreted” and interpretations “result from a process of negotiating meaning in the community (119).” The data do not reveal anything in or of themselves; instead, it is the way that psychologists utilize and interpret the data that reveals meaning. But again, it is not a truth that is revealed, or rather it is *a truth*, the truth that the researcher, given his or her experiences and knowledge, *created* while interacting with the social environment. Diverse and influential work, such as Milgram’s (1974) experiment and Burr’s (1998) work on the social construction of gender, illustrates the power of social interactions to frame and influence our understandings and realities.

Perhaps most importantly for our purposes, social construction highlights the social creation of identity. Identity creation and maintenance is work that we are constantly engaged in as individuals; we use Goffmanian (1955) performances and props to test how others interpret our identities, which then impacts how we think of our identity. This is also true for the related notion of self-worth. In an

interesting study examining the social construction of identity among the homeless in Austin, Texas, Snow and Anderson (1987) found that there can be both a social identity (the identity that society gives you) and a personal identity (the identity you hold in your mind). Traditionally, these would be thought to align, but through a social construction approach Snow and Anderson (1987) argued that there are cases in which people cannot easily reconcile the public and personal. This has obvious implications for the field of social psychology and identity research.

Social construction, as defined by Berger and Luckmann (1966), suggests that reality is constantly in flux as it is negotiated and renegotiated through our experiences social worlds. From this core idea, other branches of social construction, such as symbolic interaction, phenomenology, and ethnomethodology, have evolved. Because they all fall under the social construction umbrella, it can be difficult at times to determine their differences. How does symbolic interactionism really differ from phenomenology, for example? The following sections lay out these three offshoots of social construction and attempt to present both their historical precedence as well as their current engagement with the discipline.

SYMBOLIC INTERACTIONISM

History

The symbolic interactionist approach was first developed in the early 1900s by George Herbert Mead (1913) at the University of Chicago. He was a member of the eminent group of sociologists (loosely termed at the time because he also taught philosophy) working as part of the Chicago School in the early to mid-1900s. The Chicago School came to be known in particular for the development of the symbolic interactionist approach to studying daily life. Mead argued that society and all its component parts—structures, interactions, and meanings—are developed through social interactions, thus macroanalyses can and should really be reduced to their smaller microlevel interactions. The theory was popular during the time of the Chicago School and was then expanded and adapted by Herbert Blumer in 1960s. Blumer did not like the emphasis placed on macrolevel structures that dominated most of the sociological research at the time and thought that symbolic interactionism offered an alternative theoretical framework. Blumer's work (1969) was resurrected as an empirical framework in the 1980s, and its popularity has ebbed and flowed since. One

of the most renowned sociologists utilizing symbolic interactionism today is Sheldon Stryker at Indiana University.

Foundational Assumptions

Although Mead did not refer to the theory as such, symbolic interactionism is based on the overarching premise that all aspects of society are socially constituted. From macrolevel power structures to microlevel daily interactions, all are created through social interactions at various levels. Embedded in this perspective is the notion that meanings (about these power structures, interactions, etc.) are derived from social interactions. For both Mead and Blumer, the unit of analysis is the individual, not society or institutions. They were both reacting against the notion that social structures (i.e., socioeconomic stats) explain outcomes. Structures, according to symbolic interactionists, are just groups of people repeatedly engaged in interaction.

Our social interactions lead us to develop “shared meanings” (Sandstrom, Martin, & Fine, 2006); through our interactions with others, we take on common definitions of emotions, experiences, and ways of acting. Thus, for example, gender norms may be taught, both consciously and unconsciously from early childhood; in this way, a female understands what it “means” to be a woman in her society without ever being explicitly told. A girl does not learn this in a bubble; rather, it is through her social interactions with others that she comes to understand what constitutes appropriate behavior, dress, appearance, and the like. She learns this through her experiences and the responses she gets from others.

Symbolic interactionism “stresses that people create, negotiate, and change social meanings through the process of interaction” (Sandstrom et al., 2006, p. 1). The key point here, for Blumer and others, is that meanings are constantly evolving. So, to follow the example just mentioned, our understanding of gender is not a fixed fact (because it might be different in different regions, religions, and time periods) but the result of previously experienced gendered interactions in our past. We take our previous interactions with us and apply them to the next interaction. Interactions, even with people we have just met, are not completely insulated events. Rather, each person brings to the interaction all of his or her previous interactions and meanings. Thus, a man and a woman in conversation will bring to this exchange all of their previously held ideas about femininity and masculinity, which they will use as

a guide for navigating this new interaction. And of ultimate importance is Goffman's (1959) notion of the feedback loop; you act based on your prior understandings, receive feedback from your new partner, and then take this new feedback with you into your future interactions. As this process continues, you may alter your meanings, and potentially your behavior, over time. It is a process, not a set plan.

Because behavior and meaning are social constituted, so, too, is the self. Most symbolic interactionists would argue that there is no core/true individual identity. Rather, we engage in identity work in which take on different identities to manage the diversity of our social interactions. So, for example, in the classroom setting, one takes on the role of either professor or student. Out of this context, we may take on an entirely different identity, such as mother. None of these identities represents our "true self," but rather they are all appropriate context-specific roles. We base these roles on what Goffman (1959) called "the generalized other" or the group/people we interact with. So, we base our mothering role on our interactions with our children, our experiences with our own parents, friends, and media/cultural influences. As the "generalized other" changes, so do our identities. As a result of the primacy of social interactions, Mead's original theory is a very fluid one. Meanings are iterative because they are informed by our ongoing interactions.

Implications for Research Methods

The legacy of symbolic interactionism for research in psychology is an important one. First, the notion that all behaviors, from internal thoughts to outward interactions, are socially constituted has an impact on the psychological discourse. For researchers, this means that the participant cannot be looked at simply as an individual but rather as an individual in the social context. Thus, a person's thoughts and judgments are not solely the product of his or her own mind, but rather of his or her understandings based on social interactions (Sandstrom et al., 2006). And, additionally, one of the byproducts of social interaction is feedback about ourselves; we internalize others' perceptions of us, which can in turn influence our self-concept (Cook & Douglas 1998). This has significant implications for any researchers studying mental health because it means that the mind is no longer a solely internal, individual unit of analysis. Our thoughts, ideas, hopes, and fears are all rooted in the social

world and therefore have both social causes and consequences. Therefore, the "social act" should be the unit of analysis (Sandstrom et al., 2006).

Symbolic interactionists also highlight individual agency to form and change the world around us. Individuals "designate meanings, define situations and plan lines of action. In so doing, they actively construct the reality of their environment and exercise a measure of control over it" (Sandstrom et al., 2006, p. 6). We do this through the process of interacting, reflecting on, and evaluating interactions, and acting. This process is dynamic and, at least to some degree, controlled by the individual. There is no right or set meaning or type of interaction. Instead, we each create our own realities based on our understandings and meanings. Thus, it is still possible for two people to react to the same interaction very differently because each will bring his or her own history of social interactions and meanings to this experience.

Rooting the theory in individual meanings and experiences has implications for the types of research methods symbolic interactionists will utilize. The most commonly utilized approaches are ethnography, grounded theory, and narrative analysis because these methods allow the themes to emerge from the data, thereby preserving the individual experiences and realities. These methods more readily address the question of how people make meaning out of experiences in their lives and do not allow the researcher's assumptions and own set of meanings to dictate the findings that emerge from the data.

Application

The border between social psychology and sociology is often blurred by researchers in both disciplines' use of symbolic interactionism. In particular, Stryker (1987) argued that the movement in psychology away from behaviorism and toward a value placed on subjective experience is the result of the use of symbolic interactionism as a lens through which to examine psychological research. Thus, it is fair to say that the scope of symbolic interactionism's influence is far reaching within the field. One interesting study that took a symbolic interactionist approach is Ponticelli's (1999) study of former lesbians who, due to religious involvement in a ministry that does not acknowledge homosexuality, must reframe their sexual identities to align with their newly acquired religious beliefs. Ponticelli's research method involved eight months of participant observations, interviews, and material analysis, and her

goal was to understand the ways that the ex-lesbians in her study construct a narrative of their sexuality. Symbolic interactionism lends itself well to this kind of study because it brings participants' own understandings and narratives to the study rather than the researcher's personal assessment of the participants' stories. Additionally, Ponticelli's study also incorporates a symbolic interactionist approach in its attempt to focus on the ways that meaning is created and adjusted over time.

PHENOMENOLOGY

History

Phenomenology was first established by Edmund Husserl in the early 1900s. It has subsequently been used as an approach within psychology as well as in other disciplines in the social sciences. Husserl's original goal was to find a way to conduct objective scientific analysis of subjective topics, such as emotion. Phenomenology, along with the ideologically similar symbolic interactionism, has been an important philosophical approach underpinning much of psychological research. In particular, phenomenology has influenced the Duquesne School as well as the experimental approaches utilized in psychological research. In spite of the influence of phenomenology within the field of psychology, over time, its theoretical premise has been challenged by some of the field's giants: James, Skinner, and Watson have at various times all challenged phenomenology and advocated a more scientific approach to the discipline of psychology. The debate continues today, and many researchers still question what constitutes phenomenological research as well as its merits as a philosophical framework.

Foundational Assumptions

Phenomenology is rooted in the notion that all of our knowledge and understanding of the world comes from our experiences (Hein & Austin, 2001). At their core, there are significant similarities between phenomenology and symbolic interactionism in that both focus on the ways our engagement with society affects our worldviews. However, whereas symbolic interactionism focuses on the ways that social interactions affect our meaning, phenomenology takes the broader aim of studying experiences (phenomena). But, like symbolic interactionism, the focus is not on the events themselves, but rather on the ways in which we experience things and the meanings these experiences create for us. As Kockelmans (1973) writes, it is "bringing to light the usually hidden meanings which

motivate the concrete modes of man's orientation toward the world" (p. 274). As such, those who utilize the phenomenological approach seek to make explicit the "taken-for-grantedness" assumptions that guide our experiences (Hein & Austin, 2001, p. 6). In essence, there is no objective reality, but rather it is our experiences and our perceptions of these experiences (i.e., our lived experiences) that are our reality. Given that the meanings we create from our experiences are largely based on the social context (Smith, 2011), there is a clear link to symbolic interactionism.

Additionally, phenomenologists believe that behavior is a reflection of our previous experiences; we act in response to our temporal and spatial memories of past experiences or, as Keen (1975) writes, "behavior is an expression of being in the world" (p. 27). Thinking about behavior as a product of our past experiences forces us to consider action and individual agency as embedded in a broader social context. Related to this question of behavior is the notion of intentionality; namely, the idea that every experience is in response to or connected to some past experience. Thus, attempting to examine the experience as "in the moment" is, from a phenomenological perspective, missing the unique understandings the individual brought to the current experience.

Implications for Research Methods

As a research method, phenomenology involves studying how we make sense of our experiences or "participant perspectives" (Bogdan & Biklen, 1998, p. 26). Therefore, as researchers, we cannot assume that we know what meanings people make of certain events. For example, even though we may think the standard response is to be sad after the death of a parent, we cannot presume that a participant in our study feels this or any other emotion. The job of the researcher is to uncover what it is people take for granted (i.e., what they might not even think to tell us in an interview and what we might not think to ask because we assume they think like we do). To do this, the researcher must first come to understand the assumptions and biases he or she brings to the research. Underlying phenomenological research is the notion of bracketing assumptions, which is the idea that, before we can conduct any analysis of our data, we must first explore our own biases or the "taken-for-grantedness" (Hein & Austin, 2001, p. 6) that make up our unique perspectives. Of course, there is no way we, as researchers, can operate outside of our assumptions and experiences. However, the self-reflection for which

phenomenologists advocate does at least charge the researcher with keeping these biases in mind when conducting analysis.

Approaching a research question with the assumption that experience forms the basis for behavior and understanding fundamentally lends itself to certain research methods. In particular, utilizing methods that emphasize gathering data on lived experience from the participant's perspective is essential. To that end, methods such as ethnomethodology, ethnography, and narrative analysis are particularly relevant for researchers utilizing the phenomenological approach because all of these methods focus on uncovering the meanings individuals give to their experiences.

Application

A great deal of the research in psychotherapy is rooted in the phenomenological approach because many scholars in this field see as their goal “discovering psychological meanings by identifying the essential psychological structure of an interviewee's description of an experience” (Camic et al., 2003, p. 8). A concrete example of this comes from Carl Rogers's client-centered therapy (1951). Rogers found that many of his patients struggled not with what actually happened—that is, the “in the moment” reality—but with their perceptions and feelings about what happened. As a result, therapy must be targeted to address the individual's set of perceptions and understandings. To follow up with the example of a person dealing with the death of a parent, a therapist cannot follow a preset protocol for helping the client because each patient's experiences and feelings about death will be different.

From the perspective of social psychology, the phenomenological approach has implications for how we conduct and think about research on identity. In its most general sense, phenomenology de-emphasizes the self as a unique individual, which has implications for the types of research questions we ask, as well as for the methods we utilize. A phenomenological study of identity allows for open-ended questions that allow participants to present, through the construction of a narrative for example, what identity means to them and how it functions in their lives. This is especially relevant for factors such as gender, race and ethnicity, and socioeconomic status, which, depending on our context, can constrain or enhance our experiences and interactions. One example of this type of work is Friedman, Friedlander, and Blustein's (2005) study

that used a phenomenological approach to develop an understanding of how Jews construct their collective religious and ethnic identity as a highly assimilated but still distinct population within the United States.

A well-defined method with some roots in phenomenology (among other approaches) is consensual qualitative research (CQR; Hill, Knox, Thompson, Williams, Hess, & Ladany, 2005; Hill, Thompson, & Williams, 1997). It is a method for interview research that has been used in numerous studies in psychology, especially within counseling psychology. Consensual qualitative research is actually constructivist in ontology, in that it assumes multiple realities, and in epistemology because the researcher experience matters and informs interview question development. However, it also has post-positivistic leanings, with its emphasis on consensus among a team of researchers in the construction of findings, close adherence to a systematic approach, and interest in generalization and (Hill et al., 2005).

In CQR, consistent data are collected across participants through semistructured interviews and then analyzed by multiple “judges” who must come to a consensus about the meaning of the data. At least one “auditor” also checks the “primary team of judges” to work against the potential for groupthink. Data analysis is carried out in three steps. First, participant responses to the open-ended interview questions are divided into domains or topic areas. Then, core ideas, which are abstracts or brief “summaries of the data that capture the essence of what was said in fewer words and with greater clarity” are constructed within each domain for each individual case (Hill et al., 2005, p. 200). Finally, cross-case analysis is carried out by developing categories that describe the common themes reflected in the core ideas within domains across cases.

Consensus is at the core of the CQR method, with the assumption being that consideration of multiple perspectives brings us closer in our approximation of the “truth” and reduces the influence of researcher bias (Hill, Thompson, & Williams, 1997). Using teams of three to five analysts, coders first look at the data independently and then come together to discuss their ideas until consensus about the single best representation of the data is reached. The goal here is not what is typically thought of as interrater reliability, wherein preagreement about how to code data is established and then carried out with the goal of achieving the highest levels of accuracy in

agreement in coding. Rather, it is expected, and even hoped, that team members will begin with different ideas about the data so that the final product reflects and integrates multiple perspectives and is less fraught with individual bias. The potential for groupthink is minimized through the use of one or two additional team members who serve as auditors to review and check the primary team's interpretations and judgments. The auditors review the work of the primary team once the core ideas for each domain have been established consensually and then again when the cross-case categories have been determined. At each of these stages, the auditors review the raw material and provide comments back to the primary team who must then carefully consider each comment and determine through discussion whether to accept or reject each one.

Critical Theory

Critical theory as an approach represents a key postmodern paradigm and offers alternatives to the postmodernist and constructivist lenses. In the context of research, the application of critical theory emphasizes the ways by which the values of the researcher and those studied impact the social world. This point of view contributes to a larger shift in research over the past several decades (Kidd & Kral, 2005), one that privileges meaning and requires a rethinking of knowledge (Goodman & Fisher, 1995).

HISTORY

Critical theory has had many distinct historical phases that cross several generations. The birth of this paradigm is considered to have taken place through the Institute for Social Research at the University of Frankfurt am Main during 1929–1930. During this time, the arrival of the “Frankfurt School” philosophers and social theorists (Creswell, 2007), including Max Horkheimer, Theodor Adorno, and Herbert Marcuse, marked an idealistic, utopian vision that stretched beyond the more customary “positivist” tradition of the time. This emergence offered criticism to the status, structure, and goal of the traditional social sciences (Adorno et al., 1969). The German philosophers and social theorists of the Frankfurt School were influenced by the barbarism of World War I and what was perceived as the inhumanity of post-war capitalism so widespread in Europe at the time. During World War II, several key contributors to the School moved to the United States in an effort to escape the war. Once in the

United States, these thinkers were struck by the gulf between the stated progressive agenda within the United States and the very real differences between races and social classes present, in large part due to discrimination (Ponterotto, 2005).

According to these theorists, “critical” theory may be distinguished from “traditional” theories to the extent that it seeks human emancipation and a disruption of the status quo. Ontologically, critical theory challenges the idea that reality is natural and objective because reality is shaped by social, political, cultural, economic, ethnic, and gender-based forces into social structures. Instead, critical theorists assume that reality can only come to be known through a subjective frame and as shaped by values and mediated by power relations that are socially and historically constituted.

More recently, Jurgen Habermas's (1988; 1990) work on communicative reason and linguistic intersubjectivity has represented iconic work in critical theory in the more modern era. Habermas's work has enabled strategies of community building and social movements based on his work in communication. This work has not taken place without scrutiny, however. Theorists such as Nikolas Kompridis have opposed some of Habermas's ideas (Kompridis, 2006), claiming that these recent approaches have undermined the original aims of social change espoused by critical theory, particularly in terms of the critique of modern capitalism.

FOUNDATIONAL ASSUMPTIONS

According to Horkheimer, a critical theory is adequate only if it is explanatory, practical, and normative (1972). In other words, it has to address what is wrong with current social reality, identify the actors to change it, provide clear norms for criticism, and identify practical goals for social transformation. The orientation of this theory is toward transformation, traditionally of capitalism into a “real democracy”.

Foundation ideals are based on a fundamental struggle for equality and social justice. Knowledge is used to emancipate the oppressed, and “validity is found when research creates action” (Lincoln, Lynham, & Guba, 2011, p. 114). Given this definition, a number of “critical theories” have been developed to demonstrate differences in power in the areas of gender, race and ethnicity, class, sexual orientation, and disabilities, many of which have emerged in connection with the social movements associated with these areas, particularly in the United States. In short, a critical theory provides the

basis and groundwork for research aimed at decreasing domination and increasing freedom.

Critical theory by and large rejects the assumption that a scientific or objective basis of criticism needs to be grounded in a grand theory. Rather, epistemologically, critical theory privileges agents' own knowledge and understandings, with an assumption that these understandings can be a basis for social criticism in themselves. In other words, theories can have "a relative legitimacy" (Habermas, 1988, p. 3). Habermas also argues that, relative to other existing theories, the role of critical theory is to unify these multiple theories, considering their varied methods and presuppositions (Habermas, 1988). Given this role, it stands to reason that any social scientific method or explanation-producing theory can be potentially critical.

Similarly, in critical theory, the relationship between researcher and participant is transactional, subjective, and dialectic. In other words, what can be known is inextricably tied to the interaction between an investigator and an object or group. Insofar as one can separate oneself from marginalized groups in an effort to remain "objective," one removes oneself from one's "share" of the social condition studied, likely perpetuating the inequalities that contribute to the adverse social conditions often of interest to social scientists.

Researchers who employ critical theory take values a step further than constructivists do in that they hope and expect their value biases to influence the research process and outcome. More specifically, because critical theory concerns itself with unequal distributions of power and the resultant oppression of subjugated groups, a preset goal of the research is to empower participants to transform the status quo and emancipate themselves from ongoing oppression. Thus, critical theorist researchers acknowledge at the outset that they expect results to document the high levels of stress or disadvantage of the group under study. Beyond this, such researchers aim to use the results and report of the study in some way to advocate for improvement of the examined group.

IMPLICATIONS FOR RESEARCH METHODS

Critical theorists, given their stance on the importance of researcher-participant interaction and the significance of understanding values as influencing the reality under study, more often use naturalistic designs in which the researcher is engaged in the daily life of participants. Critical theoretical approaches tend to rely on dialogic methods, which may combine data collection

methods (e.g., participant observation, in-depth interviewing, first-person written reports) with opportunity for reflection. This approach intentionally invites a questioning of the "natural" status quo and order and an exploration of the tensions that characterize the social issue under exploration. Inherently challenging, this approach values transparency and welcomes opportunities for alternative paradigms to be considered as part of the learning process itself.

Methodologically, contexts are not merely conceptualized as "variables," but as essential parts of subjectivity according to critical theory. In terms of the field of psychology, this approach invites us to consider the role of research in terms of how liberation might take shape across the lifespan. Qualitative approaches in which a researcher's social justice values help direct inquiry, such as PAR (Kidd & Kral, 2005), provide ample example of critical theory at work in the research context.

APPLICATION IN THE FIELD

Participatory action research is a form of action research anchored in the belief that the research process itself serves as a mechanism for social change. Participatory action research is an approach focused on critical theory because, at its core, PAR is geared toward empowerment of participants that leads to emancipation from oppression and enhanced quality of life. In laypersons' terms, "you get people affected by a problem together, figure out what is going on as a group, and then do something about it" (Kidd & Kral, 2005, p. 187).

According to Kemmis and McTaggart (2005), PAR often involves a cycle of self-reflection and action in addressing a community problem. Participants and researchers establish a collaborative relationship as they ask critical questions about the current life situation. This dialogue moves the group to action as they develop knowledge and further explore the problem and how it can be addressed. In this way, collaborators using PAR begin to set a stage of social action to instigate change.

The process of change emerges and shifts as part of the self-reflective cycles, but typically is not predetermined by a clear series of procedural and analytic steps. Instead, during the reflective and action spiral, PAR investigators rely on a wide variety of methods and procedures as they gradually better understand the needs of the community. As such, many studies that use PAR take on varied methods such as storytelling, sharing experiences, individual and focus group interviews, participant observation,

drawings, and even the more structured qualitative interview or quantitative survey as the need merits.

When engaged in a PAR process, study participants are expected to participate fully. However, the creation of such participatory contexts is very challenging and time-consuming, and is not the norm (Kidd & Kral, 2005). Disempowered groups are seldom given the opportunity and at times are discouraged from this type of action. Further compounding this problem is the tendency for established forums (e.g., academia) to claim exclusive ownership of methods of knowledge gathering and avenues for change. All of these challenges further lend the process of PAR to be informed by critical theory. As a specific example, Dentith, Measor, and O'Malley (2012) outline the practice of using critical theory across three separate research projects involving young people facing various life difficulties and vulnerabilities. In so doing, they highlight the dilemmas they face in doing so within the context of more traditional, positivist approaches frequently favored in academic research settings.

Participatory action research is somewhat new to the field of psychology and has not historically been utilized frequently in this field. This is likely at least in part due to the axiology of PAR as a critical theory method that advocates a value-directed (rather than value-neutral post-positivism or value-bracketed constructivism) stance. Traditionally trained psychologists may be made initially uncomfortable by research that is value mediated because psychological training often conceives of research as objective, in which participants are studied without changing them or the researchers.

Feminist Theories

Feminist theories are used to frame and understand research approaches across a range of disciplines and social problems. They developed in part in response to prevailing ideas that more traditional scientific inquiry tended to exclude women from inquiry and deny women epistemic authority (Anderson, 1995). They are often associated with critical theory, although they have been considered by some to be separate (Crotty, 1998), yet closely related, within the epistemological continuum.

HISTORY

Informed by the political ideologies of the 1970s women's movement, feminist scholars sought to reinterpret and modify concepts within the philosophy of science to create feminist approaches to

research. Originally fueled by activism, feminism as an academic focus has developed significantly from the 1980s until the present. According to feminist paradigms, the traditional philosophy of science has tended to produce theories that represent women (or their activities and interests) as inferior to their male counterparts. Further, "feminine" cognitive styles and modes of knowledge have been denigrated by traditional inquiry (Anderson, 1995), producing knowledge that is not relevant to people in subordinate positions and/or that reinforces unequal power dynamics, particularly as it relates to gender.

FOUNDATIONAL ASSUMPTIONS

Feminist theories "place gender at the center of inquiry," and yet "increasingly incorporate multiple...intersectionalities of identity," including sexuality, race, religion, and social class (Marshall & Rossman, 2010, p. 27). Similar to critical theory, the larger aim of feminist theories is to turn thought into action (Marshall & Rossman, 2010), in this case by focusing on the issues faced by women and other often marginalized groups.

Epistemologically, feminist theories focus on the accounts of women (and other historically marginalized groups) as legitimate and core sources of knowledge. Of note, feminist theories are not distinguished so much by their substantive topic (e.g., women's issues, gender, reproductive rights, etc.) or by the gender of the researcher (i.e., male or female) but rather by their orientation and guiding philosophy on epistemology and research creation (e.g., methodology).

IMPLICATIONS FOR RESEARCH METHODS

Over the past two decades, feminist scholars have developed alternative epistemologies to guide the process of doing research. Feminist methodologies attempt to eradicate sexist bias in research while capturing women's voices, particularly those consistent with feminist ideals. Epistemologically, feminist theories privilege women's experiences as not only legitimate, but also as important and revealing bases of knowledge. Work guided by feminist theories often aims to employ qualitative methodologies toward the exploration of power imbalances, starting with that between the researcher and researched (Marshall & Rossman, 2010), so as to engender trust and collect accurate data. Research informed by feminist theory, like critical theory more broadly, also challenges academia traditionally due to its value of application of research to lived experiences (Smart, 2009), particularly among those who are oppressed.

Thus, feminist theories mirror the core values of critical theory in emphasizing the mutual learning between the researcher and the researched, an exchange that is critical to the emancipation of disenfranchised or overlooked groups.

Feminist research has emphasized the importance of exploring the day-to-day experiences of marginalized groups, particularly women. Qualitative approaches are particularly well-suited to capturing the “messiness” of these daily experiences because these methods can account for emotions, as well as for other less tangible aspects of experience, in data collection. Often, feminist theories invite more traditional forms of qualitative data collection (e.g., interviewing, focus groups, ethnography) to be adapted to be more consistent with feminist ideology.

APPLICATION

As referenced earlier, a feminist approach to research can be employed across the social as well as physical sciences and beyond. For the most part, researchers employing this approach attempt to eradicate sexist bias in research while seeking to capture women’s voices, particularly as they apply to the day-to-day experiences of everyday life. This angle lends itself well to studies such as those examining the experiences of domestic workers and domestic violence. Core to the use of feminist theory is the understanding that ways of knowing, or epistemologies, are constantly evolving as knowledge grows and as the “knowers” expand in scope. Thus, bodies of research, as they make use of a feminist lens, may find that the social problem under study increases the complexity of the problem under study. This is characteristic of feminist methodologies. However, such an approach is also characterized by reducing the hierarchical relationship between researchers and their participants to facilitate trust and disclosure and recognizing and reflecting on the emotionality of women’s lives.

Queer Theory

HISTORY

With the rise of the gay liberation movement in the post-Stonewall era, gay and lesbian perspectives began to contribute to politics, philosophy, and social theory. Initially, these were often connected to feminist ideology. However, in the late 1980s and early 1990s, queer theory was developed as its own framework. The term “queer,” as opposed to “gay and lesbian,” also distinguished the theory from those that preceded it, specifically gay liberation

theories. Similar to feminist theories, queer theory was accompanied by social movements, and its emergence evolved in part as a reaction to the marginalization of the LGBTQ community and the ways by which “science” had historically been used against them (Minton, 1997).

Queer theory found a more natural home in qualitative research because this form had historically been less focused on objective reality and more on subjective experiences (Downing & Gillett, 2011). However, its emergence has occurred within an ongoing evolution in terms of how we consider sexuality and marginalization in research and in society at large. In the early 1900s, the scientific examination of those who were in same-sex relationships was perpetually challenged by the stigma and silence faced by this group. In short, this population was hard to identify and find, much less research. The second half of the twentieth century, however, shifted this as lesbian and gay studies expanded exponentially (Gamson, 2000), focusing explicitly on the lives of those who identify as gay or lesbian. Queer theory, a more recent arrival on the scene, has introduced a post-structuralist critique by suggesting that the self cannot and should not be identified by sexuality or sexual orientation by itself, thereby challenging the importance of studying sexuality as a “subject” of inquiry. Although the tension proposed by these shifts is often applauded within the qualitative research world (e.g., Gamson, 2000), it is this context in which queer theory has emerged.

FOUNDATIONAL ASSUMPTIONS

Queer theory was separate from gay liberation theories in several ways. First, queer theory defined itself as not specific only to sexuality. Instead, queer theory does not refer to a nature, be it sexual or otherwise, but rather as a relational construct. “Queer” refers specifically to being “outside the norm”; this norm can vary relative to context. In other words, “Queer is... *whatever* is at odds with the normal, the legitimate, the dominant. *There is nothing in particular to which it necessarily refers.* It is an identity without an essence” (Halperin, 1995, p. 62; emphasis in the original).

Because queer theory does not suggest a specific nature or essence, it therefore is inclusive of those who may express themselves outside *any* norm, including that of the gay and lesbian community. In other words, sadomasochism, perhaps marginalized by some constructs, is not so according to queer theory. Additionally, this lack of focus on a specific

essence allows gays and lesbians to identify by their sexuality *or* by any other aspect of their identity, thereby placing the focus on personal meaning, as opposed to societally ascribed labels.

IMPLICATIONS FOR RESEARCH METHODS

A central claim of queer theorists, which is that identity is understood exclusively as a social construct (rather than given by nature), significantly affects how research is carried out within this approach. Most immediately, it implies that research needs to be evaluated for biases that privilege heterosexuality (Butler, 1990; 1993), however subtle. Based on the concern that queer theory places on false dichotomies (e.g., “closeted” and “out,” etc.), this theory also is critical of other dichotomies implied in research, particularly as it relates to assumptions regarding what is natural or artificial and what is masculine versus feminine. Instead, queer theory emphasizes the importance of understanding categories more fluidly, an approach that lends itself more toward qualitative methods, which seek to explore social phenomena with an eye toward complexity rather than standardization.

APPLICATION

Queer theory has been applied to multiple social problems and developmental issues. However, it is most often applied to questions concerning empowerment, resistance to domination (e.g., heterosexism, homophobia), gender identity and marginalization due to gender, sexual orientation, or sexual behavior. Because queer theory is concerned with the nonessential nature of sexual identity, this theory pushes the field to consider identity from multiple perspectives, and invites cultural as well as race-related inquiry.

Conclusion and Future Directions

It is impossible to fully represent the richness of any one of these philosophical approaches in a chapter such as this one. We have instead tried to convey a sense of the breadth of the field and to illuminate at least some of the meaningful distinctions in the major approaches to qualitative research in psychology today. In this last section, we turn our attention to the future and identify three overarching, interrelated, and contested issues with which the field is being confronted and will be compelled to address as we move forward: retaining the rich diversity that has defined the field, the articulation of recognizable standards for qualitative research, and the commensurability of differing approaches.

The contested nature of these issues stems in part from the very diversity of philosophical approaches that has defined the field. Here, again, we cannot possibly represent the considerable thought behind and debate around each of these matters. Rather, we simply raise and mark them at this time.

The diversity of approaches represented in the field of qualitative research today speaks to the strength of the movement and bodes well for our efforts to both advance and deepen our understanding of the psychological world. As Ann Hartman (1990) wrote many years ago, “each way of knowing deepens our understanding and adds another dimension to our view of the world” (p. 3). Just as no single research design or data collection method can adequately capture the multidimensional nature of human psychology, no one philosophical approach can suitably guide our efforts to address the full range of questions that need to be pursued to develop the knowledge needed “to benefit society and improve people’s lives” (American Psychological Association, 2013).

However, this diversity in approaches to qualitative research also creates significant tensions and makes attempts to “define” the field quite challenging. Despite the substantial work done by many scholars (c.f. Denzin & Lincoln, 2013) to delineate these contrasting perspectives and approaches, a lack of awareness remains, especially (but not exclusively) among those not well-versed in qualitative methods. The predictable misunderstandings and strong differences in beliefs about what is “credible” research that can result continue to plague those of us who practice qualitative research as we strive to get our work funded and published more widely. Peer reviews of our work can often be riddled with contradictory assessments of its rigor and even of its basic value or contribution. (c.f. Ceglowski, Bacigalupa, & Peck, 2011).

Continued efforts to make clear the diversity of approaches, the philosophical assumptions guiding these, and the particular contributions the differing approaches make to our understanding of psychology are critical. We must be cautious about making general claims about rigor and the “right” way to do qualitative research that are actually framed within our own narrower terms or experience with certain approaches. Keeping the richness of the field alive will require discipline on all of our parts to respond to questions about how best to go about engaging in high quality qualitative research or evaluating the quality of the work of others by first acknowledging “it depends” and then inquiring about the philosophical approach, aims, and context of the work.

One of the biggest challenges before us is the continued articulation of recognizable standards for qualitative research that represent, and which ideally can be applied to, the full range of approaches. The very differences in purpose and aims and in philosophical approaches that comprise the rich field of qualitative research today makes such efforts seem impossible. However, ignoring this task in the era of what has been called the scientifically based research movement (National Research Council, 2002; Torrance, 2008), defined largely in terms of experimental design and methods and with randomized controlled trials heralded as the “gold standard,” leaves the array of approaches that do not readily fit this mold highly vulnerable. But what is the best way to address these complex and high-stakes issues?

Researchers taking a more post-positivistic approach have argued that there are separate but parallel sets of standards for validity and reliability in qualitative and quantitative research (e.g., Hammersley, 1992; Kuzel & Engel, 2001). Some constructivists have put forth that a common set of standards can be established but because the foundational philosophical approaches between post-positivism and constructivism are so different, a separate and distinct set of criteria need to be applied. Models using concepts such as trustworthiness, transferability, and authenticity have been developed (e.g., Guba & Lincoln, 1989), and it is estimated that more than 100 quality appraisal forms have been put forth (Saini & Shlonsky, 2012). Unfortunately, most do not make clear the philosophical assumptions that undergird them (Saini & Shlonsky, 2012), which unfortunately further muddies the water. Moreover, other adherents to constructivist approaches hold that the contextual and relational nature of knowledge construction precludes the possibility of establishing such standards (e.g., Lincoln, 1995; Schwandt, 1996). Finally, many working from within critical theory and related approaches suggest that such standards are inevitably formed by the power structures in which they are housed, thereby potentially further perpetuating the inequalities the research aims to address or study (e.g., Garrett & Hodkinson, 1998). Furthermore, they assert that the quality of the research should be based on an assessment of whether it empowered participants to effect meaningful and lasting changes (Correa, 2013).

Some have tried to resolve these tensions by suggesting guidelines they believe account for and are applicable across the diversity of approaches to qualitative research (e.g., Drisko, 1997; Saini

& Shlonsky, 2012; Tracy, 2010). These guidelines focus on the different components of the research process, such as clear identification of philosophical approach and aims of the research, specification of methods and congruence between these and the stated philosophical approach and aims, and transparency and clarity in sampling, data collection, and data analytic procedures. Although the imperative to tackle these issues is clear, the way forward to doing this is less so. Should we push further toward agreeing on a shared set of standards that can be applied across traditions, or invest in more localized ones tailored specifically to particular approaches (e.g., narrative analysis) and developed by scholars practicing these (Preissle, 2013), or both? How might the myriad elements of research, including the many gatekeeping activities in the research and scholarship enterprise from funding through publication of research findings, address and accommodate these standards in their expectations and processes? What is clear is that the diversity of approaches to qualitative research must be fully represented in any efforts to further define and move the field forward on this front.

Embracing and fully representing the diversity of approaches and coming to terms with standards for them stills leaves unaddressed a third concern for the field moving forward, namely what has been referred to as the *commensurability of approaches*. That is, whether approaches rooted in the differing philosophical approaches can be “retrofitted to each other in ways that make the simultaneously practice of both possible” (Lincoln et al., 2013, p. 238). Some, such as critical and feminist theorists, have argued that epistemological differences between methods can render research paradigms incompatible (Lincoln et al., 2013). Others have dismissed assertions about irreconcilable differences between philosophical approaches and research paradigms and argue for what they call a “pragmatic” approach, particularly in the service of carrying out mixed-methods research (e.g., Creswell, 2009; Creswell & Clark, 2007; Maxcy, 2003). Lincoln, Lynham, and Guba (2013) take a middle position and offer a “cautious” endorsement of the commensurability of approaches. They assert that some approaches share some elements that are similar or strongly related and therefore can be effectively and meaningfully combined, whereas others are more “contradictory and mutually exclusive” (p. 239). Preissle (2013), in her consideration of the future of the field, makes a pragmatic argument of a different sort for commensurability. Citing the work of her

students that has combined approaches in unconventional yet highly productive ways, she observes that the novice scholars of today are “challenging, even transgressing, epistemological and theoretical boundaries” that will ultimately move research forward in unexpected ways” (p. 536).

There is nothing new about these questions. They have been debated for decades now, and clarity seems no nearer. What has changed is the climate. It is at once more open to qualitative methods than ever before and less accommodating of the rich diversity among the approaches taken to this work. Increasing numbers of graduate students are being trained in multiple methodologies. Although, unfortunately, there does not yet appear to be a cry for purely qualitative studies on the horizon, most major funding sources are at least indicating a preference for the use of multiple methods, in some cases even quite strongly so. Qualitative studies can be found in journals of differing ilk, not just within the confines of those dedicated to publishing qualitative research. However, what is deemed acceptable or “credible” qualitative research is narrowing. In the parlance of the old expression “a little knowledge is a dangerous thing,” the widening exposure and reach of qualitative work means that many more scholars are encountering and engaging with it in some way; these scholars often do not realize that what they know is but a small slice of a now large and longstanding field. Researchers outside the field of qualitative research who participate in setting the standards for research more broadly may be friendly to particular kinds of approaches, such as seeing a place for qualitative work only in the exploration of new areas of inquiry to offer “thick description” and examples or to complement or round out the quantitative findings, but much less so to stand-alone work or work aimed at explicating processes and mechanisms at work in human psychology. Scholars from within who are joining in the work of setting the standards of research can sometimes allow certain kinds of qualitative research to stand for the field, which can serve to belie and even shut out other, often more transgressive forms. These perhaps seemingly old and familiar questions about philosophies of science, rigor, and commensurability are alive and well, taking new forms, and they are, in some ways, more important now than ever before.

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Applied Interpretive Approaches

Sally E. Thorne

Abstract

In the applied world of the practice disciplines, the recognized limitations of conventional science have stimulated a lively and enthusiastic uptake of many of the qualitative research approaches generated over decades of social science scholarship. However, due to significant differences in the nature and motivation between the more theoretical and more applied fields, many applied scholars have been departing from established method to articulate approaches better suited to the questions of the applied world. This chapter considers the evolving relationship between the methods and their disciplinary origins and current trends in the direction of the applied interpretive qualitative research project. *Interpretive description* is used as a methodological case in point to illustrate the kinds of departures that applied approaches are taking from their ancestral roots as they begin to advance knowledge development within the practical and contextualized realities of their applied contexts.

Key Words: Applied research, interpretation, methodology, social science, applied science, disciplinary thinking, interpretive description, qualitative research

Many of the qualitative methods we have come to consider as conventional approaches over the past generation of scholarship were handed down to the applied world from the intellectual projects of decidedly theoretical academic disciplines, especially anthropology, sociology, and philosophy. They were wonderful methods, carefully thought through and tested over time by enthusiastic students of human nature and societal experience and taken up by applied communities who saw them as the light at the end of a tunnel that had resulted from a dominant quantitatively constructed science. In this chapter, we reflect on the motivations that drove applied scholars to embrace qualitative methodology, and we deconstruct some of the inevitable challenges that they confronted in trying to bend it toward their distinct purposes. On that basis, we trace the evolution of new approaches to applied interpretive inquiry that are informed

but not constricted by the history and tradition of qualitative science. We see the exciting and innovative new approaches that are emerging to help scholars and practitioners in various health, education, social policy, and humanitarian fields take advantage of the rich heritage that exists within the body of qualitative research tradition and apply it usefully toward the social mandate that each of their applied disciplines represents.

To begin this journey through the evolution into applied interpretive methodology, a few words of location may be in order. I am a nurse by profession, with a curiosity about the human interface within which the healthcare system shapes the options available to people with chronic illness or cancer as they learn to live with the lot that life has handed them. The questions I pose in my program of research have to do with making sense of experiential challenges, making meaning

out of despair, learning to handle the frailties of the body, and finding ways to live well despite debility, discomfort, and impending mortality. I am particularly interested in how those of us who fulfill professional roles in the healthcare system engage with patients and their families in ways that can range from empowering and healing to terrifying and soul-destroying. These are naturally complex, dynamic, fluid, and messy concepts to be studying. We can know things on the basis of behavioral or attributional patterns that defy measurement. We can believe things about how to “be” with patients during these most difficult of times without being able to “prove” the distinct impact that our moment of engagement has produced. And yet we all hold a professional (legal, ethical, and moral) mandate to act in such a manner as to do no harm and to support the processes that stand our patients the best chance of leading toward health. Thus, in that context of wanting to build knowledge that will help health practitioners toward being of use, I have spent the past thirty years in the world of qualitative health research.

The story of how my discipline embraced qualitative methods is instructive in understanding why it came to a point at which it had to generate its own modifications to existing methodology. Nursing has played a rather active role in the evolution of qualitative approaches to health research because its core business exists within a realm of complex and messy matters (Dzurek, 1989; Reed, 1995; Sidani, Epstein, & Moritz, 2003; Thompson, 1985; Watson, 1995). We work closely and intimately with individuals, families, and communities, and the nature of our work engages deeply with the minutiae of their bodies, minds, and souls; their realities and aspirations; and their despairs and triumphs. Nursing also works in close proximity to medicine, for which the increasingly powerful mandate of formally constructed and scientifically rigorous evidence as the basis for practice has been dominant in recent decades. So, nursing needed ways of working with the questions that arose from its core business, and it also needed to justify the kinds of work it was doing within a rather hardcore, scientific, and ideological landscape of what counted in healthcare (Johnson & Ratner, 1977; Liaschenko, 1997; Maxwell, 1997). Thus, the methods that had been created by social scientists for the very different kinds of things they were studying seemed to create a wonderful legitimacy for an enterprise that could consider itself as rigorous and credible even

as it departed quite significantly from what science looked like in the traditional biomedical context.

In the early years of what became a time of explosive growth in qualitatively derived health knowledge, nursing often led the way, practicing a kind of meticulous compliance with the methodological dictates that had been established by and for the social science disciplines. Although these nonquantitative pieces of research started to find legitimacy in grant reviews, conference presentations, and journal publications, it was well recognized that the genre’s credibility depended on accurate alignment with legitimate and credible social science methodological sources as the basis for its scholarly work (Bartolomé, 1994; Sandelowski, 1986). However, not long into the journey of the qualitative health enterprise, this obsession with methodological precision started to become a liability (Thorne, 1991). Debates within the literature ensued with respect to whether methodological slippage and sloppiness ought to be tolerated in the context of the kinds of rigorous and rigid expectations that the evolving quantitative science demanded (Baker, Wuest, & Stern, 1992). This methodological rigidity came to constitute a crisis characterized by deep tensions between those interested in promoting technically correct methodological applications, even if the findings were rather “bloodless,” and those interested in making the most meaningful discoveries, even if that required methodological departures (Janesick, 1994; Sandelowski, 1993*a*; Thorne, 2011).

It was within this tense methodological context that my graduate students and I began to consider the possibilities of articulating applied interpretive methods as a distinct approach within our qualitative enterprises and to imagine what would be required to legitimize methodological alternatives that might not only work within the evidence-based science context our discipline resides in but also meet the need of producing truly usable knowledge. The ideas and approaches that we now advocate have arisen explicitly from and in response to the ideas and agendas of the historical times in which we find ourselves. We believe that they offer important insights for optimizing our work today, even as they will inevitably evolve into new and different opportunities for tomorrow. Thus, this chapter encourages applied researchers in their quest to conduct studies that have meaning for informing engagement with the present-day realities they face while also exposing and illuminating new interpretive possibilities that might serve us even better in the future.

The Historical Grounding of Traditional Qualitative Methods *The Quest for Objective Truths About the Social World*

Auguste Comte (1798–1857) was among the first social theorists to have understood that authentic knowledge derives from personal experience and not simply from metaphysical or theological foundations (Pascale, 2011). The positivism he advocated represented a search for the laws of social life that might parallel the natural laws of the physical sciences (p. 13). Early in the 1900s, the physical sciences model of social research became the subject of considerable critique. A leading voice in this was Antonio Gramsci (1995), whose “Prison Notebooks,” written between 1926 and 1934, argued that the methods used for an inquiry had to be congruent with its own purpose (Pascale, 2011). The increasing rejection of hypothetico-deductive reasoning as the appropriate foundation for certain kinds of knowledge positioned a new kind of method as counter to the constraint of an objective world about which one verifies data through the processes of empiricism (Bohman, Hiley, & Shusterman, 1991). Thus, methods of rigorously working with nonobjective data started to emerge within the social sciences as a way of studying human behavior and understanding the reasons that govern it (Jovanović, 2011). These historical tensions help us appreciate why, despite subsets of their members who consider themselves to do “applied” work, those in the mainstream social sciences have generally remained quite skeptical of methodological limitations that seem bound to the discourses of science and scientific notions of evidence (Pascale, 2011).

The Emergence of Application

As the qualitative approaches to social science theorizing evolved and career opportunities for social scientists expanded beyond the academic institutions in the mid-century (Gordon, 1991), scholars began to apply their social research methods to questions arising within the health field. Some of the earliest contributions of this type came from Howard Becker and his colleagues’ *Boys in White* (Becker, Geer, Hughes, & Strauss, 1961) and Erving Goffman’s classic, *Asylums* (Goffman, 1961). By the 1970s and 1980s, health researchers within the professional disciplines had begun to pay close attention to this brand of research (Anderson, 1981). Cross-fertilization took place as increasing numbers of health professionals undertook doctoral

studies in social science disciplines and began to experiment with some of these methods in their own clinical investigations (Morse, 2012). By the end of the 1970s, the occasional qualitative piece could be found within leading scholarly journals (Loseke & Cahil, 2007), and, in the following decade, new journals started to emerge with a focus on qualitative approaches (Denzin & Lincoln, 2005).

The Interpretive Turn

Although these initial applications were clearly distinct from their positivist forbearers in their methods of generating and testing truth claims, in several respects they remained quite aligned with the objective realism of the social science traditions in their attachment to theorizing as the primary product of good inquiry. Thus, much of the early grounded theory of scholars such as Anselm Strauss and Barney Glaser (Glaser, 1978; 2002; Glaser & Strauss, 1966; Glaser & Strauss, 1967; Strauss, 1995) actually furthered confusion by virtue of its attachment to the aspirations of grand theorizing it had inherited from its roots in the Chicago School of symbolic interactionist analysis (Layder, 2007). Although these early methodological developers fully acknowledged that the findings one created on the basis of their approaches were generalizable and reproducible only to the extent that one could replicate original conditions of context, they also advocated “theoretical saturation” as the legitimate point at which an inquiry concluded (Glaser, 1978; Schmuttermaier & Schmitt, 2001) and rejected the possibility that social reality might be best depicted by a multiplicity of seemingly irreconcilable theoretical perspectives (Layder, 2007). Thus, they left behind a rich collection of techniques bound within some fairly problematic theoretical architecture.

Beyond grounded theory, the other major methods taken up by applied scholars during this period also came with considerable layers of theoretical “baggage.” Ethnographic methods, such as those advanced by James Spradley (1979) and Rosalie Wax (1971), which were marvelous in their depth and detail with respect to certain aspects of investigative engagement, provided little direction for generating coherent conclusions about human experience outside of the context of full considerations of culture (Aamodt, 1989). Furthermore, they were guided by rather foundational assumptions about universalities in human nature that sometimes overshadowed the individual variations that a health researcher might want to exploit, such as the notion that an individual’s understanding of

his or her situation might be actually more relevant to the problem at hand than was a more generalized and comprehensive portrait of why people within certain intact cultural contexts think and behave as they do (LeCompte & Goetz, 1982; Lipson, 1989).

Applied phenomenological researchers such as Max van Manen (1984), working in the traditions of Martin Heidegger (1962) and Hans-Georg Gadamer (1960/1989), similarly offered excellent options for digging deep into the subjective experience that had proved so difficult to account for in more traditional studies of health experience. However, the techniques associated with this tradition also posed obstacles to the applied researcher (Anderson, 1989; Benner, 1994; Lopez & Willis, 2004). What did it mean, for example, to genuinely bracket preconceptions when those preconceptions justified the health inquiry in the first place (LeVasseur, 2003; Morse, 1994)? And what kinds of subjective realities might one want to try to understand beyond those aspects considered essential structures of human experience (Anderson, 1989; Thorne, 1997*a*; Thorne, Reimer Kirkham, & MacDonald-Emes, 1997)?

The expression “interpretive turn” therefore became a signifier for work that was considerably less theoretical and philosophical than the traditions from which it had arisen and inherently implied both the applied and the practical (Bohman, et al., 1991). It referenced a fundamental recognition that human interpretation is the appropriate starting point for the study of the social world (Pascale, 2011) and also the point toward which research findings are ultimately directed. Thus, it became clear that the analytic induction that had arisen from many of the earlier qualitative approaches had never been interpretively neutral (Pearce, 1971); rather, it had inevitably relied on interpretation in order to be put to use in the world of applied practice (Thorne, 2001).

The Nature of Applied Interpretive Methodologies

An argument can be made that applied interpretive work differs from nonapplied interpretive work in the degree to which it accepts the existence of some form of reality and the relationship it assumes to various truth claims. Shusterman captures the essence of the kind of interpretive work that sits firmly within the antifoundationalist and antinaturalist realm: “Having abandoned the ideal of reaching a naked, rock-bottom, unmediated God’s-eye-view of reality, we seem impelled to

embrace the opposite position—that we see everything through an interpretive veil or from an interpretive angle” (1991, p. 103). From this perspective, what we come to understand about a phenomenon depends on who we are rather than by virtue of any immutable properties it possesses, and who we are is unconstrained by such conventional modernistic limitations as reason or logic. Thus, competing theoretical positions become intellectual standpoints from which to consider or debate a thing, with no pretense toward a truth claim because the “real world” upon which a truth claim must be based is itself simply an idea.

This kind of nonapplied positioning makes for marvelous theorizing, endless debate, and rather seductive intellectual entertainment. It takes one out of the mundane and ordinary everyday into a world of limitless standpoints and subjectivities. Ideas become the mechanisms through which engagement in the human world is navigated, and the theoretical projects that evolve from this kind of work take on a direction that is firmly located within thinking rather than action. Considering nonapplied work in this way (and of course I am overgeneralizing here to make a point), it becomes understandable why purists within the social science tradition would be somewhat horrified at the thought that their ideas might actually be put to use in the practical and material world.

Variations on the Interpretive Lens

Applied interpretive work therefore departs from what convention within the social sciences might consider genuinely interpretive in that it must always keep at least one foot firmly planted on the ground. It accepts that the ground exists and possesses a nature that constitutes a form of reality apart from human perception, even as it recognizes that the perceptions we humans make of it are powerfully shaped by our historical and cultural positionings upon it (Crotty, 1998). Thus, applied interpretive work sits in a somewhat complex philosophical space in which the polarities of subjective and objective truth are not incommensurate or mutually exclusive, and strands of both realism/positivism and idealism/relativism can potentially inform knowledge development (Stajduhar, Balneaves, & Thorne, 2001).

There is, therefore, room for considerable confusion with regard to what is meant when a scholar positions his or her work as “interpretive” (Guignon, 1991). For some scholars and traditions, it implies an explicit reliance on the ideas of

a certain favored set of established thinkers, such as Heidegger. Conversely, for others, it references the more general notion that research never occurs in a vacuum and, in the applied fields in particular, it is highly problematic to ever pretend that it does (Bohman, 1991). Rather, educators study learning problems because they hope to resolve them, health practitioners study disease experiences because they hope to reduce suffering, and so on. The disciplinary lens that comes along with the credential inevitably and fundamentally paints the colors and defines the contours that a qualitative researcher will see in the field, no matter how compelling the theoretical invitation to imagine that field as something else. Thus, qualitative research by anyone whose legitimacy in conducting research derives from membership in an applied practice discipline is perhaps most usefully understood as an inherently interpretive endeavor.

Contradictions Arising in the Applied Context

As the qualitative methods and approaches that had been generated within the social sciences for the purpose of advancing theorizing were taken up by an increasing spectrum of scholars in the applied disciplines, this tension between theoretical and applied interpretation led to considerable slippage and confusion. From where I sit, I believe that this confusion may be most strongly represented in the health field, where there has been a stronger tendency than in some other disciplines to try hard to adhere to conventional social science method.

Qualitative health researchers seem to have been slower to develop alternative methods than have their cousins in such fields as education. Lincoln and Guba's *Naturalistic Inquiry* (1985) of the mid-1980s was an unselfconscious adaptation of conventional grounded theory principles into a highly pragmatic approach for the study of complex educational systems. Although one might have expected their explicitly applied methodological approach to have had considerable appeal within the health disciplines, it attracted considerable criticism for being theoretically lacking and was not as well received beyond the educational application (Dixon-Woods, Shaw, Agarwal, & Smith, 2004). For the most part, despite these available options, qualitative health researchers continued to position their studies within the same small set of social scientific traditions and rely on adherence to same conventional rule sets for determining whether a qualitative research product had merit (Cohen & Crabtree, 2008). The powerful arm of borrowed

credibility that social science methodology conveyed seemed to have the qualitative health research field in a chokehold.

Despite the timidity within the qualitative health research sector to depart too far from the rules and traditions inherited from social science, some scholars were clearly recognizing the inherent limits of social science approaches within the applied clinical context (Johnson, Long, & White, 2001) and pointing out that uncritical acceptance of conventional social science methodological tenets was leading to some rather weak applied products (Thorne & Darbyshire, 2005). For example, some health researchers drawing on phenomenological methods were claiming to have maintained *tabula rasa* (blank slate) by failing to read extant literature in advance of their study (LeVasseur, 2003). While bracketing preconceptions in order to delve below superficial understandings to discern the deeper structure of essential human experiences makes good sense for the pure phenomenologist, it fails to ring true in the study of a human health experience when clinical familiarity with a phenomenon has led one to the conclusion that there are gaps in existing knowledge (Morse, 1994).

Another misapplication prominent in the body of health research using grounded theory was the artificial claim that "theoretical saturation" had been reached as a justification for concluding data collection (Smaling, 2003). Although the idea that one had exhausted all possible configurations of a theoretical proposition might make sense in the generation of basic social theory (Morse, 1995), it runs counter to the disciplinary mindset required of the practicing health professions, in which the clinical gaze must go beyond population patterns to detect the infinite variation that occurs within each individual case (Thorne & Sawatzky, 2014).

Among the many other problematic ideas that had crept into the qualitative health research domain because of this uncritical adherence to method were member checks as a primary means of determining credibility. In the health domain, we are often studying phenomena for which patient perceptions can be the source of a problem. Thus, seeking their confirmation that we "got it right" may actually impede epistemological integrity (Thorne & Darbyshire, 2005). The idea that qualitative research becomes the voice for the voiceless has led some researchers to believe that interpretation was somehow unethical and that the data should "speak for themselves," thus effectively sidestepping the obligation for rigorous analysis and

relying on (supposedly uninterpreted) a selection of transcribed speech excerpts as a reasonable way of displaying findings (Ceci, Limacher, & McLeod, 2002; Sandelowski, 2004). Similarly, believing the requirement that all studies must be conducted within a theoretical framework, many authors were almost predetermining their findings by virtue of structuring their studies within a perspective that actually limited their capacity to see all that they might have seen of relevance to the question at hand (Carter & Little, 2007; Sandelowski, 1993*b*).

Thus, the evolving body of qualitative health research was fraught with these kinds of contradictions and complications that exposed it to credibility challenges and weakened the potential impact of the evolving science. The allegiance with social science methodologies had certainly brought it well beyond the confines of the quantitative methodological paradigm, but had left it with some rather worrisome unintended consequences. New options were therefore required to challenge researchers working in applied fields for either making hollow claims or defeating their stated purpose.

Characteristics of the Evolving Genre

Researchers also needed ways of building on the creative modifications they had worked out in order to render them coherent and credible. The applied interpretive methodologies that are evolving over time derive from a philosophical positioning that visits the world of theorizing without taking citizenship. That positioning reflects an intrigue with the possibilities inherent in the universe of technique generated for the purposes of the social sciences without taking on the mantle of coherence that determines the integrity of the methods when they are used in their entirety. They therefore require a different kind of conceptual organization and order, so that the steps one takes are consistent with an interior logic that will get you to a recognizable and worthwhile goal. They take as a foundational principle that a disciplinary mandate underpins the decision to do the research in the first place and all of the consequent steps that will be taken in bringing it to a meaningful conclusion. They also understand there to be a particular audience for the eventual findings that will require certain kinds of transparency and auditability maneuvers to attain credibility and coherence.

It is in the nature of the applied disciplines that knowledge exists for some purpose (Malterud, 2001). Thus, the qualitative tradition that simply

describes a thing has relatively little relevance within the applied world. Despite careful attempts to adhere to rules that limit the generalizability of findings—and so much of the qualitative research literature reflects these disclaimers—every clinician knows that an idea that captures the imagination in relation to a clinical problem that requires further understanding cannot really be suspended until it achieves some measure of truth value. Rather, since all knowledge generated in the applied fields may actually influence someone's thinking in the practice world and therefore affect those individuals they serve, we actually need responsible implications and estimates more than we require some theoretical calculation of the conditions under which our claim might have population relevance (Sellman, 2011).

So it is these kinds of problems that applied interpretive methodologists are concerned with as they propose various ways of approaching the problem of trying to do rigorous and useful qualitative research in a manner that addresses the needs of the disciplines and fields from which their questions derive (Angen, 2000). They are necessarily concerned with credibility, assuring scholars that their inquiries can pass the muster of funding body panels and journal editorial reviews. Thus, they must be mindful of the context within which the qualitative scholarly tradition has developed within their field, finding ways to demonstrate a respect for that tradition at the same time as they push back against some of its more problematic artifacts in their efforts to produce authentic knowledge products. This makes for a delightfully contested arena in which various methodological positions are being promoted, debated, and challenged (Thorne, 2011). And it is quite understandable that newer scholars entering the field are excited about the possibilities of not only stepping outside of convention but also ensuring sufficient respectability within it to navigate the treacherous waters of the scholarly assessment establishment.

The Terminological Land Mines

On the basis of these common difficulties with conventional method, scholars in various applied fields have put forward alternative options for framing qualitative research outside of the conventional social science traditions.

In 2000, Margarete Sandelowski raised a challenge to health researchers to consider that much of what they were doing was, in fact, quite different from the kind of work toward which the named social science methods were directed, and she asked

why we couldn't simply call this kind of work *qualitative description* (Sandelowski, 2000). Others similarly proposed sidestepping the methodological battles by using language such as *generic qualitative research* (Caelli, Ray, & Mill, 2003). Ten years after that initial paper, Sandelowski expressed dismay that her argument had been misinterpreted by many as a new methodological approach for which she had inadvertently assumed the role of authority (Sandelowski, 2010). She also decried the possibility that referencing one's work as "qualitative description" might be a convenient excuse for poorly conceived or inadequately conducted studies, providing would-be researchers with a quick and easy way to sidestep thoughtful interpretation.

In furthering this debate, Sandelowski aimed our attention directly at the problem of naming the kind of research that does not fit the methodological names in our conventional repertoire. From her perspective, the boundaries between the named methods have been much more semipermeable in the applied context than most researchers recognized. She further pointed out that, "Complicating the borderlands between methods (and the policing of these borderlands that too often passes for methodological rigor and expertise) is that in qualitative research, methodological procedures function more to trigger analytic insights than to determine or constitute them" (2010, p. 81). Thus, she advocated for reserving the use of the term "qualitative description" as a "distributed residual category" rather than as a coherent methodological option, making those porous lines more visible, reducing erosion, and avoiding the need to continually reinvent method. For her, what ought to be center stage in the empirical research of the practice disciplines was technique rather than method.

The term *applied phenomenology* has been widely used to reference the kind of inquiry that seeks to draw on phenomenological thinking to enact social change. Cheryl Mattingly recognized that the practice of clinical reasoning in her profession, occupational therapy, was itself a form of applied phenomenology (1991). In a similar vein, Patricia Benner referenced *interpretive phenomenology* to embrace a range of applied approaches toward engaged reasoning within nursing research (1994). Richard Addison, an applied researcher from the discipline of family medicine, further advocated interpretive approaches that allowed for a range of technique to bring phenomena from "unintelligibility to understanding" (1992, p. 110). Explicitly referencing the kind of commitment to

meaning-making that a hermeneutic approach invited, albeit applied in the context of the kind of grounded participant observation work that seemed relevant to his profession, Addison called his kind of applied work *grounded hermeneutic research*. Norman Denzin (1989), another leader in interpretive methodology, coined the term *interpretive interactionism* to reference a self-reflexive action research approach that has been taken up by various health researchers for applied purposes (Mohr, 1997). Borrowing from scholars who use this kind of language to reference creatively applied studies that address the core mandate of their discipline, others have taken up this kind of language to help distinguish their applications from the original traditions.

Jonathan Smith and colleagues in the UK health psychology field have been working with an applied and interpretive methodological tradition called *interpretative phenomenological analysis* (IPA) (Smith, Jarman, & Osborn, 1999; Smith & Osborn, 2003). It explicitly draws on phenomenological notions of the hermeneutic circle inherent in the researcher's attempt to try to "make sense of the [study] participant trying to make sense of their personal and social world" (Smith, 2004, p. 40). In so doing, it unambiguously positions its design recommendations as having to maintain relevance and credibility within the corpus of mainstream psychology. It therefore generally aligns its approaches to qualitative inquiry with the distinctive subdiscipline of cognitive psychology in its recognition of the centrality of mentation (p. 41), thereby serving as an adjunct to the more mainstream scholarly contributions arising from quantitative and experimental methodology. At the same time, Smith clearly understands his method as having appeal to a range of applied disciplines in which ideographic case examination becomes the launching point from which inductive analyses may evolve. He further sees IPA as interrogative in its capacity to engage with the ideas arising from existing research within a field.

What I have observed, at least in the health research world, is that there exists a very strong preference for work that explicitly and credibly locates its methodological origins within something with the capacity to convey the legitimacy of an accepted authority or tradition. The deeply held convictions among those who grew up in the science tradition as to what constitutes methodological integrity cannot be easily discarded. Thus, it makes for a much more persuasive claim to locate and justify your design choices within one or more of the philosophically

compatible traditions that have already met the test of scholarly review by virtue of being published in the appropriate manner than it does to try to convince a panel of scholars that your distinctive and idiosyncratic approach will ultimately make sense.

Interpretive Description

It was this awareness of the imperative to cite appropriate references for methodological choices, especially when one veered off the beaten path (Thorne, 1991), that led my graduate students and me to publish our first manuscript on interpretive description as method in *Research in Nursing & Health*, a journal recognized at that time as among the most highly respected in our discipline (Thorne et al., 1997). In our strategic selection of venue and in the terminological choice we made in naming the method, our conscious intent was to render credible the kinds of design modifications that we saw very good qualitative health researchers making without acknowledging what they were doing or, as Jan Morse put it, doing qualitative research “for which there was no name” (Morse, 1989, p. 6). We characterized it as “noncategorical” in an (awkward) attempt to explicitly distinguish it from the named categories of methodology that were in favor at that time.

Following that initial publication, in response to calls for further elaboration, we expanded on options for the analytic process, which is generally the most difficult aspect of constructing a high-quality research product (Thorne, Reimer Kirkham, & O’Flynn-Magee, 2004). Subsequently we ventured a longer treatment of interpretive description in book form (Thorne, 2008); this text was intended not as prescriptive method but as a companion to support the interior logic of each design decision that a researcher might be called on to make throughout the applied interpretive inquiry process. Interpretive description is explicitly designed for researchers, such as those in my profession of nursing, whose disciplinary framework and mandate is sufficiently comprehensive to frame inquiries and, one might argue, ought to be driving those inquiries. In this way, it can be thought of as either a method in and of itself or as a guide to the use of method, depending on which one needs it to be. Positioning it in that manner recognizes that each disciplinary scholar will be best placed to discern the distinctive conditions and contexts within which the research will be conducted. These might include, for example, such elements as the state of the science (both empirically and philosophically),

the prevailing opinion (including tensions and debates), the breadth and depth of a phenomenon that would need to be taken into consideration if the results of an inquiry are to be meaningful, and the ideological and theoretical proclivities of the target audience toward which the study will be directed. Interpretive description thus becomes a decisional model within which all of those elements can be reconciled into a coherent and logical plan that can meet the kinds of quality criteria that we refer to when we reflect on what really constitutes excellent applied interpretive work (Engel & Kuzel, 1992; Hunt, 2009; Kuzel & Engel, 2001; Morse, Barrett, Mayan, Olson, & Spiers, 2002; Oliver, 2011; Popay, Rogers, & Williams, 1998).

Because it is the approach in which I have been immersed, and not implying that it is the only viable approach from which to mount a well-constructed applied interpretive study within a disciplinary framework, I will expand on the idea of how interpretive description works to serve the needs of the applied researcher across a range of contexts using my own discipline as a case in point. By reflecting on the nature of the design options that the applied researcher will face and must justify along the way, unpacking this particular exemplar may be instructive across the spectrum of using method in such a manner that serves, rather than enslaves, its rightful master.

Understanding the Disciplinary Lens

The manner in which nursing’s conceptual structure shapes aspects of research design illustrates what I mean by a disciplinary interior logic. Nursing knowledge inherently and explicitly capitalizes on a dialectic interface between the general and the particular (Reed, 2006; Rolfe, 2011). Nurses rely on general knowledge not as prescriptive or superordinate truths but rather to expand their repertoire of options for informing the complex considerations that will inevitably be applied in the uniquely individual context of each patient (Thorne & Sawatzky, 2014). Thus, the structure of nursing thinking uses such mechanisms as categorization and description as a tool toward reasoning rather than as an answer to a question. That reasoning takes the form of a cyclical process of engaging, assessing, planning, acting, and evaluating. By virtue of their professional accountabilities, nurse researchers are held to certain standards with regard to anticipating the potentially untoward effects that uncritical implementation of some of their

findings and interpretations might have in certain cases (Cheek, 2000). So, for example, in rendering responsible and useable findings, they would take into consideration the possible misinterpretations that might arise at any phase within that cyclical reasoning process, as well as the universe of clinical and contextual variables that the practitioner might confront in applying the new idea in the real world.

The manner in which this disciplinary lens shapes research design, therefore, is to ensure that even in the search for commonalities, the applied interpretive researcher is always and inevitably curious about difference. And although various theoretical positionings such as those offered within postmodern/poststructural traditions of scholarship can help uncover the implications of the way a discipline thinks about certain phenomena (Kagan, Smith, Cowling, & Chinn, 2009), these are typically understood as only temporary standpoints because staying there too long tends to make it difficult to justify the action that is the inherent *raison d'être* of the profession (Pesut & Johnson, 2013). It is this intimate knowledge of how a discipline's thought structure works, and not merely the substantive content of it, that guides a scholar in the kinds of methodological design options that are consistent with and informative to disciplinary knowledge. And, for this reason, I personally would have considerable hesitation with advocating an approach as flexible as interpretive description for a researcher without a strong grasp on an applied disciplinary perspective.

Articulating the Question

Because nursing's practice mandate would preclude its assuming that all patients might experience a health or illness phenomenon in a similar manner, research questions framed in the style of conventional phenomenology—such as “What is the lived experience of...?”—don't quite fit. The discipline tends to reject notions of essential experience in favor of the principle that infinite variations on almost any theme are to be expected. A fundamentally human commonality, such as the ability to experience pain, for example, does not lead nursing toward the search for the essential nature of pain, but rather for an understanding of the kinds of variations in perception and expression that may be meaningful for the work of reducing unnecessary suffering.

Similarly, the typical forms of grounded theory questions that orient one toward basic social processes at play—such as “What is the process

of...?”—suggest an assumption that the tacitly held dimensions of a phenomenon may be more influential than the patient's perspectives about it. Thus, recognizing the inherent tension between the kind of research that assumes a primacy of patient perspectives and the kind that would see them as a distractor would reveal the inconsistencies in embarking on that kind of inquiry process in most of the contexts in which nursing inquiry occurs.

Instead, in keeping with a more authentic understanding of why nursing might need to obtain a certain understanding of a phenomenon, an interpretive description question might be articulated in less theoretically loaded terms. One might ask, for example, in what ways do patients explain their experiences with this issue? Or what kinds of experiences do they describe as most worrisome and why? Such framings clearly locate not only the manner in which subjective material will be considered in the analytic process but also the role that the available data will play in informing interpretations with regard to the wider context within which that phenomenon appears in practice.

Framing the Theoretical Scaffolding

Interpretive description frees the nurse researcher from the convention of having to select an extant theory within which to locate the study. Because nursing exists within the dominant culture of the (Western biomedical) health science tradition, the notion of theoretical positioning as a hallmark of good science has been a deeply held expectation. In the early years of qualitative nursing research, positioning within a particular social science tradition fulfilled that function. However, once one recognizes the problematic of that posture for the applied fields, then such theoretical positioning seems a hollow exercise at best and, at worst, an abdication of authentic disciplinary inquiry. As Sandelowski has pointed out, although they might claim one as a denominational credential to justify legitimacy, for the most part, nurse scholars were rarely engaging with those theories in the manner in which their social science colleagues intended (Sandelowski, 1993b).

A further complication arises when one understands the convoluted history of nursing's efforts to theorize itself. In its early attempts to justify its scientific base, the discipline devoted considerable efforts to articulate a set of theoretical structures that might best capture the nature of nursing. In that this exercise predated such conceptual tools as complexity science or a recognition that philosophy

had anything much to do with scientific thinking, these entities referred to as theories were in fact competing philosophical configurations within which to try to capture something that was by its very nature dynamic, messy, and complicated (Thorne, 2009). Thus, aligning one's study with one or another of the theories of nursing would position one within a rather meaningless and divisive discourse.

What interpretive description therefore offers is the invitation to reclaim the foundational intellectual character of nursing thought—those essential commonalities among and between all of those individual attempts to theorize. And, perhaps because of our complicated intellectual history, much of the work that we now draw on to guide us can be found more accessibly in the world of philosophy of nursing rather than in anything that considers itself to be theory. So, what scaffolds a study is the explicit articulation of the elements of disciplinary structure that will be brought to bear in shaping and guiding the design elements and applications of each study, and it is these that will ultimately afford it credibility and legitimacy.

Sampling and Data Collection

Sampling approaches using interpretive description may be convenient, theoretical, or purposive. The key is for the researcher to not only name what they represent, but also to hold to an integrity of interpretation informed by the nature of a sample (Kuzel, 1999). Applied researchers must always suspend the notion of representation in some kind of tension, recognizing that although elements of two cases may have similarities, each case also holds distinctive uniqueness at some level (Sandelowski, 2006). Thus, the challenge to the researcher using interpretive description would be a clear and credible contextualizing of the sample size and nature within the context of the kinds of populations or patients the findings are meant to inform the discipline about.

A study may well combine various sampling forms, beginning with convenient recruiting to launch a study, moving into a purposive stance as the dynamics of the recruiting process unfold, and then targeting recruitment for particular instances of certain configurations of a phenomenon in the later phases of data collection. In addition, in recognition of the representation challenge, the researcher may well include reference to a more theoretical consideration of possible variations beyond the scope of the actual study to ensure that the inherent limits of sampling are not overly influential in shaping the

study findings and interpretations (McPherson & Thorne, 2006). For example, a clinician might well recognize that a qualitative study sample cannot normally include data reflective of all of the population subgroups that might participate in a particular clinic, but that it might draw on personal or expert knowledge of that clinic context to hypothetically test claims as they emerge from the data analysis. This “what if?” aspect to making sense of what you have and don't have in the study sample can be especially beneficial to the process of articulating findings in such a manner that they “ring true” to the intended clinical audience by virtue of attending to the range of experience that it entails.

Data collection using interpretive description can appropriately draw on multiple and diverse approaches. My discipline has been especially enthusiastic about individual interviewing as a primary data collection approach, and an overreliance on this has been the focus of critical debate as to the limitations this may have on the nature of the evolving qualitatively derived knowledge base available to those working in the field (Nunkoosing, 2005; Sandelowski, 2002; Silverman, 1985). Interpretive description is compatible with a range of alternatives, including focus groups, participant observation, and documentary analysis, but, perhaps most importantly, it encourages the researcher to think about appropriate combinations of approaches so as to enhance a comprehensive understanding without being overly dependent on the inherent limits of any singular approach. For example, beyond interviewing a group of patients who may have had experience with a particular health or healthcare phenomenon, one might additionally seek out perspectives from thoughtful clinicians who could contribute a much broader experiential range of diversities and variations that they have seen over time. One perspective need not trump the other, but rather the triangulation of perspective increases the likelihood that the findings will be reflective of a broader context than one can reasonably capture in a sample of voluntary study participants.

Data Analysis and Interpretation

Interpretive description sits within an inductive analytic tradition that would not favor the kind of thematic processes that we might think of as qualitative content analysis (Elo & Kyngäs, 2008; Hsieh & Shannon, 2005). Instead, it seeks ways of thinking about and organizing insights that become emergent as one works iteratively with data, such that new insights and possibilities for understanding can

be illuminated, considered, and further developed. Even when a study is explicitly designed to expand on and develop ideas that have already been derived from a rigorous inductive analytic process, the interpretive description approach encourages the investigator to remain open to new ways of seeing and understanding that might advance our capacity to know a phenomenon in a manner that is, in one respect or another, better than we did before. Thus, the idea of replication to enhance credibility doesn't really make sense, nor does the prior assumption that one will necessarily recreate the precise conceptual structure proposed by another researcher when investigating a similar kind of dataset. Interpretive description always starts with what is already known, believed, or accepted within a discipline about the phenomenon in question, and it seeks some expansion on that prior knowledge for some defensible purpose. And it would be that purpose that shapes much of the ongoing analysis and interpretation in a dialectic of inquiry along the lines of: What else might be happening here? What might we be missing? How else might we be thinking about this phenomenon? What other interpretive lenses might add value (or depth, or perspective) to what I am able to discern to this point?

This philosophical stance to interpretive descriptive analytic process clearly steers researchers away from the presumption that they are discovering truths and toward processes that will better and more effectively illuminate possibilities for thought and action. A universe of technique drawn from the body of qualitative methodology may be helpful in advancing the analytic process, as long as the researcher thoughtfully sustains the capacity to understand the nature of the technique and its limitations. For example, if you code, you need to understand what you are coding for and what you have set aside, as well as what that might mean for your eventual conclusions. If you rely on excerpts of verbatim interview text, you are privileging that which is rendered articulable in overt speech over that which may have been communicated nonverbally but quite clearly in the interactional moment. So, the challenge becomes one of immersing oneself in data, capitalizing on a strategic sequence of objective and subjective engagements with the data, and knowing the data well enough to be able to propose several different options in ordering and organizing them such that the final presentation portrays the best representation of the important meanings they contain.

The analytic process typically moves from pieces to patterns, from patterns to relationships and,

sometimes (but not always), into a new coherent whole. Interpretive description assumes that the researchers would not really know, until fully engaged with and reflective about a set of findings, whether the eventual form of the interpretive claims would be best represented by an overarching metaphor, a set of conceptualizations, a thematic summary of sequences, or a typology of processes. A skilled researcher would typically be capable of considering multiple viable options on how to craft and display a set of findings such that it was true to the rationale for the study and the conditions on which it has been built, as well as relevant and credible to the eventual intended audience. As Sandelowski might explain it, you are deciding whether the optimal organizing structure is the one that emphasizes "character, scene or plot" (Sandelowski, 1998, p. 377). Thus, analysis stays true to the data without losing sight of the rationale and conditions under which it has been created, and it aims toward discernment of the best possible options for bringing the newly generated insights to the attention of those who might benefit from them.

Data display follows the logic of analysis, such that the analytic structure shapes and organizes that which will constitute findings. The aim within interpretive description is for a reader within the applied discipline to understand and easily follow the logic with which the elements of the findings are sequenced and presented. Since new knowledge within an applied discipline presumes a certain kind of fit within existing disciplinary understandings, interpretation is integrally interrelated with the presentation of analyzed findings. In my discipline, it may not be useful or appropriate to expound on a litany of theoretical options for which some vague "fit" with the findings may apply, but rather to exploit similarities and differences in relation to currently popular conceptualizations that may be influencing practice within the field. At a bare minimum, given the ethos of the discipline, one would expect a new conceptualization that has been derived from a qualitative inquiry process to theorize what kinds of patients, contexts, or circumstances might be less well served if we thought about this phenomenon in a new way. The interpretive process therefore refers us back to disciplinary logic to determine how best to situate new ideas or claims within prevailing options in an interpretive manner. Thus, the explicit literature to which one would refer would be that which is most likely to be familiar to the discipline in terms of accepted wisdom, as well

as that which might extend the credibility of any new directions or considerations being proposed by the new findings. The aim here is to be able to generate a set of conclusions that both follows logically from a coherent study design process and also speaks to the discipline in a language that is internally consistent, logically accessible, and credible in the eyes of that theoretical “thoughtful clinician.”

Credibility

Although all qualitative research approaches wrestle with the complex challenge of how to authentically and reasonably evaluate the credibility of a qualitatively derived research product, there are some additional challenges inherent in the applied methods that interpretive description considers. Clearly, in the applied world, a researcher ought not to get away with claims that credibility determinations rest entirely with the individual reader or that the study has no credibility beyond its immediate time and location. In the applied world, research is not simply an intellectual fancy of the individual scholar but rather becomes a strategic and meaningful activity to be conducted on the part of the discipline. Although all qualitative research presumably strives for *epistemological integrity* and *analytic logic*, inquiry in the applied world must also consider both *representative credibility* and *interpretive authority* as key quality measures (Thorne, 1997b, 2008). These two angles of critique demonstrate respect for the complex contexts within which disciplinary readers deserve to make sense of and understand the expected limits of the conceptualizations being proposed, as well as judge the intellectual foundational claims on which the new interpretations infer both commonalities and variations.

Beyond these fundamental principles that constitute the standard for quality evaluation in applied qualitative work, the interpretive description approach explicitly requires *disciplinary relevance* as an important consideration. The competently theorized study report that might most easily find favor with a social science-oriented audience may seem to an applied audience to be engaged in quite a different conversation. As many applied scholars have found, it is often impossible to satisfy both masters, and, by succeeding in the theorizing world, they may have lost their grip on the world they sought to inform. Similarly, since the applied disciplines operate from the perspective of a definable social mandate, their research products can be judged by virtue of *moral defensibility*. By this, I mean a level

of responsibility and accountability that extends well beyond the matter of ethical behavior in relation to research subjects and thoughtfully considers how the findings of our research might be used or abused in society (Lipson, 1994; Sieber, 1993). Another consideration for the applied researcher using interpretive description is a *pragmatic obligation* deriving from the knowledge that, if they seem meaningful, findings may well be applied in the practice world whether or not we claim them to be sufficiently developed to warrant knowledge translation. Similarly, a *contextual awareness* must be apparent in the report of study findings so that they reflect credibility (Herzlich & Pierret, 1985). This appreciation for the world of practice, with its inherent hunger for better ways to think through the problems with which it is confronted, ensures a mindfulness for the appropriateness of rhetoric, persuasive language, or emotionality in our (naturally) enthusiastic claims about our scholarship.

A critical element of the contextual world to which practitioners of qualitative health research should always take into consideration is the complex world of “evidence.” Although the evidence debate in the health world is itself fraught with complexity, and the qualitative research community remains divided on the degree to which it ought to attend to this debate, complain about it, or ignore it (Ray & Mayan, 2001), one might argue that if evidence is the conceptual term by which the decision-making and policy world references what it might draw on to make intelligent decisions, then we have no choice but to position our work such that it optimally speaks evidence language (Madjar & Walton, 2001). By this I am not suggesting a competition between the qualitatively and quantitatively derived truth claim, but rather a strategic positioning of both our research questions and the manner in which we frame, display, and interpret our findings such that they add something of recognizable value to the more deeply philosophical question of how we know what we know (Tarlier, 2005). Indeed, discovering the skill sets required to build applied qualitative inquiry on a sophisticated understanding of what is detected and obscured through measurement and how decision-making processes take up knowledge within society seems the next frontier to be conquered in this project of methodological advancement (Thorne & Sawatzky, 2014).

Implications

In the applied research world, the “so what” is always a particularly important element of a

qualitative report. This is the point where the investigator turns back to face the discipline to make explicit what can and cannot be taken from the findings to inform practice, as well as what requires further investigation. As would be apparent from reflection on the discussion of interpretive description to this point, the obligation associated with articulation of a study's implications derives strongly from the disciplinary logic from which the research question arose. Further, it ought to reflect a thoughtful appreciation for the evolving advancement of the field into which the current findings seek to make some sort of contribution.

An interpretive description approach takes issue with some of the (unfortunately) common kinds of claims one might find within the available body of qualitatively derived knowledge. Qualitative studies rarely generate the kinds of findings that would justify a radical departure from the manner in which good practitioners deliver care, for example, but might steer them toward important and meaningful fine-tunings of awareness and insight in their actions. Effectively presented findings and implications invite the discipline to consider shifts in direction and guide them in determining the nature and scope of knowledge development that might help them feel justified in defending those shifts. In keeping with the epistemological integrity that every applied qualitative study ought to aspire to, they should clearly delineate an auditable logic with regard to any directions in which the new insights might take the discipline. Thus, such discussions would studiously avoid the kinds of assumptive leaps that are too often seen in published qualitative research reports that, on the basis of one small study, public policy or legislative changes may be warranted. Wishful thinking absent data has little place within the scholarly agenda of the applied fields.

Leaders within the applied fields fully recognize that it is the ongoing and iterative dialectic of watching where the full body of science is heading and considering that in the light of directional trends in the policy environments that will best ensure forward progress within the fundamental social mandates of their disciplines (Kagan et al., 2009). And it is into this larger world of ideas and action that interpretive description seeks to insert the kinds of ideas that qualitative inquiry can produce to enrich and inspire a better world.

Conclusion

Interpretive description, as explained here in some detail, illustrates but one example of the many

creative and strategic ways that scholars in applied disciplines have been working within the qualitative research tradition to generate coherent, strategic, and comprehensive methodology that will speak to the intellectual projects of their disciplines and generate knowledge that has the potential to be put to use. The proliferation and uptake of these newer applied interpretive approaches over the past decade confirm the profound need that has been felt for inquiry approaches that respect the integrity of the knowledge structures the applied disciplines entail, as well as the pragmatic contexts within which these disciplines require knowledge.

Fortunately, we seem well past the era in which it was presumed in the health research world that qualitative and quantitative research were paradigmatically incommensurate to the extent that a single researcher could not possibly appreciate or contribute to both (Coulehan, 2009). That remnant of Kuhnian thought, an idea that artificially separated the worlds of objectivities and subjectivities, has little place in the real world of applied scholarship, in which human processes and experiences are being shaped by that which we claim as an outcome of our science (Newman & Hitchcock, 2011; Walsh, 2011).

The applied qualitative research of the future will be informed by knowledge that derives from whatever knowledge sources are available, interpreted and integrated according to an accessible disciplinary logic, and rendered credible by the policy and practice worlds in which it seeks legitimacy (Mitcham, 2007). In the complex and messy world of real-life practice challenges, it will necessarily reflect a wealth of techniques and tools, options and approaches, all held together within a coherently logical framework that allows readers and knowledge users to discern its integrity and understand how to use it. We have moved far beyond being the "poor cousin" of our social theorizing colleagues or the "soft and fuzzy" thinkers of the applied scientific community. This intriguing juncture in our collective methodological history offers a rich and evolving compendium of options capable of guiding us toward wisdom and intelligence as we move perceptively closer to solving the problems of the inherently fascinating and invariably complex world that is our reason for being.

Future Directions

A consideration of the current state of applied interpretive methodology in the qualitative research tradition brings to light several important directions

that will be of interest to the field in the coming years. Among them are these:

1. What role will interdisciplinarity have within programs of research designed with a disciplinary agenda in mind?

2. What might be the risks of orienting applied qualitative research along the lines of disciplinary logic? Can an approach such as interpretive description inform our understanding of the gaps that might potentially derive from a disciplinary lens on knowledge, or might it blind us to implications of disciplinary agenda?

3. What might be the role of the “generic” researcher in the study of applied problems, such as health, outside of the perspective of a disciplinary framework?

4. Are there certain research tools, techniques, and strategies designed for the purposes of theoretical disciplines that ought to have no place within applied research? Are there certain combinations of techniques that should be considered inherently incompatible? Or is it useful to consider all available techniques potentially appropriate to an applied qualitative inquiry?

5. How might we design studies that effectively triangulate interpretation such that multiple angles of vision are considered in a coherent and thoughtful manner?

6. How would we educate a next generation of applied researchers such that their grasp of the full scope of available knowledge informs their insight as to the most compelling questions to be asked and the most convincing approaches to be used toward building studies with optimal impact?

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The Grounded Theory Method

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Abstract

The term “grounded theory” was introduced to the research lexicon by Barney Glaser and Anselm Strauss in the 1960s, particularly with the publication of *The Discovery of Grounded Theory* in 1967. The term itself is somewhat misleading since it actually refers to a method that facilitates the development of new theoretical insights—grounded theories. In this chapter the method is outlined, together with some background to its appearance and subsequent developments. Later sections describe the main features, procedures, outputs, and evaluation criteria.

Key Words: grounded theory, grounded theory method, coding, theoretical sensitivity, pragmatism

The term “grounded theory” first came to prominence with the publication of *The Discovery of Grounded Theory* (hereafter *Discovery*) by Barney Glaser and Anselm Strauss in 1967. Since that time, the term itself has come to encompass a family of related approaches to research that reaches across many disciplines, including the social sciences, psychology, medicine, and many others. Strictly speaking, the term “grounded theory” refers to the outcome of a research process that has used the grounded theory method, but it is quite common for researchers and others to refer to the method simply as “grounded theory,” with the context clarifying the meaning. For instance, when Kathy Charmaz and I were compiling and editing a Handbook on the topic (Bryant & Charmaz, 2007a/2010), I suggested that the title should be *The Sage Handbook of the Grounded Theory Method*, a suggestion that was immediately and justifiably rejected by our editor on the grounds that, as far as publishers, librarians, and researchers were concerned, *The Sage Handbook of Grounded Theory* was far more recognizable and perfectly self-explanatory. For the purposes of what follows, however, the term “grounded theory method”—hereafter GTM—will be used to refer to

the method, with the term “grounded theory” referring to the outcome.

Prior to the appearance of *Discovery*, Glaser and Strauss had published several papers and also a book-length study using the GTM, entitled *Awareness of Dying* (Glaser & Strauss, 1965; hereafter *Awareness*). This early work developed from deeply personal experiences for both of them, Glaser and Strauss having each recently suffered the loss of a parent. It is crucial to understand that these deeply personal experiences of key lifecycle events were an important facet of the development of the method. Moreover, similar issues continue to form a key feature of a good deal of research using GTM, with the individual researcher or research team being motivated in their work by personal experiences or specific interests in the area. This is evidenced in many papers and accounts centered on GTM-oriented research, and several of the contributors to chapters in the handbook stress this aspect (e.g., Covan [2007], Star [2007], and Stern [2007]).

Glaser and Strauss were joined in their early research by Jeanne Quint (later Jeanne Quint Benoliel), a nursing specialist who transformed the

practice of care for the terminally and chronically ill in the course of her professional career, eventually being admitted to the Nursing Academy of Fame (Quint Benoliel, 1967, 1982, 1996). Some of the earliest papers on GTM were co-authored not only by Glaser and Strauss, but also included Quint (Strauss et al., 1964). Indeed, the acknowledgments at the beginning of *Discovery* include reference to a Public Health Service Research Grant, the funding for which provided the basis for the work leading to publication not only of *Awareness* and *Discovery*—and the later book *Time for Dying* (Glaser & Strauss 1968)—but also of Quint’s own book *The Nurse and the Dying Patient* (1967). Moreover, Quint’s interest in the outcomes of the work would almost certainly have been centered on the ways in which the research on dying—“awareness” and “time”—afforded a basis for more effective practice, something that has always been a central feature and concern of those developing GTM.

Apart from their own personal experiences of bereavement, the personal trajectories of both Glaser and Strauss are critical in understanding their contributions, joint efforts, and later divergent trajectories with regard to GTM. Anselm Strauss had studied at the University of Chicago as a postgraduate and thereafter held posts at various colleges and universities, until he returned to Chicago in the 1950s. At this stage, he worked with and was influenced by Howard Becker (1963) and Erving Goffman (1959), continuing the ideas of the earlier Chicago luminaries such as Herbert Blumer (1969), and George Herbert Mead (1934, 1938). Blumer is credited with coining the term *symbolic interactionism*, in the 1930s, although its origins are usually linked to the work of Mead. This basis provided Strauss with a background in social sciences that stressed the importance of naturalistic forms of inquiry, and his writings include standard and influential works on social psychology, many of which went through several revisions and reprints. In 1960, Strauss moved to the University of California, San Francisco (UCSF). There, he was given the responsibility of establishing the teaching of research methods in the new doctoral program in nursing, itself something of a key innovation. By 1968, he had developed his own doctoral program in sociology, with a specific focus on health, illness, and care, and with a clear predilection for qualitative research. As explained later, his early background was critical in the initial articulation of GTM and its later developments, but not always in the ways that might have been expected.

Barney Glaser studied at Columbia University, New York, where the key influences and luminaries were Paul Lazarsfeld and Robert K. Merton; Merton being ostensibly the supervisor for Glaser’s PhD. The influence of Lazarsfeld was significant, and, to some extent, Glaser might be considered as one of the key adherents and developers of Lazarsfeld’s methodological ideas. Glaser himself makes this clear in his book on *Doing Quantitative GT* (2008), in which he clarifies the ways in which Lazarsfeld’s ideas influenced and presaged many key aspects of GTM itself.

In a more recent account of his time at Columbia (Holton, 2011), however, Glaser places far more emphasis on the direct influence of Hans Zetterberg in his intellectual and methodological trajectory. The overall impact of his time at Columbia was to imbue Glaser with an agenda that included confidence in pursuing his own research ideas, a suspicion of grand conceptualizations and the grand conceptualizers, and the importance of publishing one’s work—if necessary, self-publishing. In the development of GTM, the influence of Lazarsfeld was particularly important, as will be explained.

In the early 1960s, Glaser moved from New York to California, and, by the mid-1960s, he and Strauss had started to collaborate, producing *Awareness* in 1965, as well as various earlier papers that can be seen as precursors of GTM. *Awareness* included a brief appendix entitled “Methods of Collection and Analysis of Data.” This is an important early statement of GTM. It notes that both Strauss and Glaser had experienced bereavements in the years prior to their research. Strauss’s experience in the death of his mother had led him to understand the importance of people’s expectations of the “certainty and timing of dying” (1965, p. 287). He had set up a preliminary study and was later joined in this by Barney Glaser, whose father had recently died. The appendix then offers a succinct summary of the approach that had been used to produce the foregoing chapters, with mention being made of the importance of developing the confidence to plunge into the fieldwork from the outset, generating hypotheses in subsequent stages as the research progresses, and the “blurring and intertwining of coding, data collection and data analysis” (p. 288). Anyone looking for a starting point in reading about GTM would do well to start with this appendix.

The doctoral program at UCSF, founded in 1968, was very much a proving ground for GTM. Those among the first groups undertaking this program were presented with the new research approach, and many of them subsequently became key propagators

and developers of the method. Given the settings and context of Glaser and Strauss's early research, and also that the focus of UCSF was on developing professionals in the areas of medicine, nursing, and what might be termed health support, it was not surprising that much of the work emanating from these GTM pioneers focused on hospital- and health-oriented issues.

Marking the fortieth anniversary of the doctoral program in 2008, a member of its first intake made the following comment:

"I like to refer to this program as The Mouse That Roared," says Virginia Olesen, professor emerita in the Department of Social and Behavioral Sciences at the UCSF School of Nursing. "This has always been a tiny program—never more than six or seven faculty. But, my gosh, the contributions..." (quoted in Schwartz, 2009)

Strauss can be seen as a pioneer of what would now be termed the sociology of medicine and healthcare. Moreover, this initial anchoring in the healthcare context, combined with the methodological innovations, resulted in a rich and varied series of outputs that have had a significant and continuing influence on social research methods, nursing practices, and palliative care. Schwartz (2009) does not exaggerate in summarizing the contributions as including, "legitimizing the concept of nursing research, establishing today's most prominent qualitative research methodology and, supplying much of the ammunition informing the most significant public discussions about health and health care over the past half century, from women's health and health disparities to aging and the impact of science and technology."

With regard to GTM itself, many of the students from these early years of the program went on to develop and enhance the method, including Kathy Charmaz, Juliet Corbin, and Adele Clarke.

Background and Early Development

Although *Discovery* is rightly regarded as the founding text of GTM, its role was very much one of a manifesto, rather than an instructional overview or manual. In the opening pages of the book, Glaser and Strauss argue that the book "is directed toward improving social scientists' capacities for generating theories" (1967, p. vii). They recognize that not everyone can develop this capacity, but this does not mean that it should be seen as something restricted to a few geniuses. Generating "useful theories" requires "a different perspective on the canons

derived from vigorous quantitative verification on such issues as sampling, coding, reliability, validity, indicators, frequency distributions, conceptual formulation, construction of hypotheses, and presentation of evidence. *We need to develop canons more suited to the discovery of theory*" (p. viii; emphasis added).

Glaser and Strauss contended that research in the social sciences in the United States in the 1960s was largely centered on the grand theorists and their grand theories. Thus, doctoral students in particular were all too often expected to develop proposals that emanated from one or other well-founded, "grand" theoretical position, deriving hypotheses and then concomitant procedures and tests for validating these latter deductions. They saw this as a highly unequal relationship between "theoretical capitalists" and "proletarian testers." Moreover, this emphasis on verification prevented new and useful theories from being developed. Whether this was quite as widespread as Glaser and Strauss claim is not clear; indeed, Strauss himself had come from a contending orientation—the Chicago School—that had produced significant work from a fairly wide range of different researchers. But whatever the truth of the matter, GTM developed as a reaction against a view of research—quantitative and hypothesis-oriented—which was prevalent among the social science research community in the United States at the time. Conversely, it is important to understand that the method was, from the first, marked far more by its innovative claims and contribution to research practice than it was by its critical position with regard to standard approaches.

Kathy Charmaz (2006) has pointed to the distinctive features of GTM that challenged many of the core assumptions prevalent among US social science researchers in the 1960s:

the "arbitrary divisions between theory and research"; viewing qualitative studies as preparatory for more rigorous quantitative work; viewing qualitative research as illegitimate and devoid of rigour; viewing qualitative studies as impressionistic and unsystematic; the separation of data collection from its analysis; seeing the only possible outcome of qualitative research as "descriptive case studies rather than theory development."

It is worth dwelling on these since further consideration will be of particular benefit in preparing a GTM-oriented research proposal that often requires engagement with the still conventional hypothesis-oriented "quantitative canon."

Research Versus Theory

What Charmaz terms the “arbitrary division between theory and research” emanates from Glaser and Strauss’s argument that the social sciences in the 1960s in the United States had become “frozen” theoretically. The work of the European founding fathers of social science—Marx, Weber, Durkheim—had been supplemented by the work of homegrown theorists such as Parsons and Merton. This body of work had then come to be seen as a rich basis for further research, particularly for doctoral students and other, relatively inexperienced researchers, who would enhance existing work through the “canon of verification” to which Glaser and Strauss alluded in the opening section of *Discovery*.

Whatever the merits might have been for this orthodoxy, Glaser and Strauss individually had taken issue with it, both conceptually and as part of their own intellectual trajectories. Strauss had developed ideas in the field of social psychology and was heavily and directly influenced by the work of relatively unconventional social scientists associated with the various generations of the Chicago School, particularly those linked to symbolic interactionism. Glaser, conversely, had direct experience of the ways in which doctoral research could become a process of “proletarian testing” under the guidance of “theoretical capitalists”: Merton was his doctoral supervisor. In the recent work in which Holton (2011) reports on a series of interviews with Glaser, he makes it clear that although he learned a great deal from Merton and Lazarsfeld, he also consciously trod his own path, with encouragement from Zetterberg, who was only his senior by a few years.

In their early statements on GTM, such as *Awareness* and *Discovery*, Glaser and Strauss not only wanted to demonstrate the power of their method, but also to encourage others to follow their example. In particular, they wanted to encourage early-career researchers to branch out on their own, confident that they could and should aim to contribute new theoretical insights. The grounded theory method, with its emphasis on research founded on directly gathered data, rather than initial hypotheses, offered a route whereby researchers could aim to produce novel theoretical insights in the form of *substantive* theories—that is, conceptual statements or models that provided deep and practical insights into specific contexts, but that required further work if they were to provide the basis for more general purposes (see later discussion).

The overall impact of this means that there are firm justifications for the preparation of research proposals that can indeed eschew hypothesis testing

as the starting point of research and instead specify objectives based on developing new conceptual models, framework, or theories. These outcomes can be evaluated using Glaser and Strauss’ criteria of *fit*, *grab*, *work*, and *modifiability*. Thus, the view that research is something based on existing theories can be challenged, offering the alternative proposition whereby theories and hypotheses can be the results of a research project. This is not to suggest that the latter viewpoint eclipses the former, but rather that the sequence of “theory then hypotheses then research” can be supplemented or replaced by the sequence “research then theory and hypotheses.”

The Status of Qualitative Research

For many researchers and, perhaps more importantly, for many disciplinary and research domain gatekeepers, valid research ought to be quantitative. The epigram of Lord Kelvin (Sir William Thompson) is often (mis)quoted in this regard: “If you cannot measure it, you cannot (control) improve it.” A more extended version runs as follows

In physical science the first essential step in the direction of learning any subject is to find principles of numerical reckoning and practicable methods for measuring some quality connected with it. I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the state of *Science*, whatever the matter may be. [PLA, vol. 1, “Electrical Units of Measurement,” 1883-05- 03] available at <http://zapatopi.net/kelvin/quotes/>. Accessed July 26, 2012

Kelvin also argued, however, that “radio has no future,” “X-rays will prove to be a hoax,” warned the Niagara Falls Power Company that I “trust you will avoid the gigantic mistake of alternating current”; and stated in his address to the British Association for the Advancement of Science, in 1900, that “There is nothing new to be discovered in physics now, All that remains is more and more precise measurement.” (This last statement is somewhat disputed, since the original source cannot be confirmed.) So much for Lord Kelvin’s prognostications!

All too often, researchers have made the mistake of measuring what can be measured, rather than attending to investigating the key issues—whether or not they are amenable to simple, or not-so-simple, quantification. Glaser and Strauss could have

counted the number of patients who died in the various hospital wards they investigated; they could also have looked at the number of days or hours that elapsed between admission to hospital and eventual demise. These might have produced some meaningful outcomes, but the concepts of “awareness” and “time” would not have emanated from such studies.

Kelvin’s longer quote expresses the view that nonquantitative studies are “at best” a preliminary to true knowledge (which must always be quantitative), but the results of the burgeoning of qualitative research that has developed at least since the 1960s indicate something very different. The outcomes of qualitative research can indeed be poor, ill-defined, lacking in rigor, and of little practical use; but so too can the outcomes of quantitative research. Moreover, thanks to the efforts of Glaser and Strauss—as well as many others who have contributed to innovation in research practice in many disciplines—qualitative research can be carried out in accord with clear and coherent criteria, laying a foundation for rigorous claims to knowledge and conceptual and theoretical innovation.

As will be seen in the sections that follow, there is an issue with regard to the distinction between conceptual innovation and impressionistic (re)description, but this is no more problematic for qualitative research than issues around statistical significance and meaningless or ambiguous measurement are for quantitative research. The key point is that Glaser and Strauss’ work in the 1960s and beyond needs to be recognized as forming a significant contribution to the knowledge claims of qualitative research methods and outcomes—many of which are now far more widely accepted if not widely taken for granted.

Data Collection and Analysis

One of Glaser’s teachers at Columbia was Paul Lazarsfeld, now considered to be one of the key influences in the development of investigative and experimental methods in sociology. Many of the existing taken-for-granted methods in applied social research were, in fact, developed by Lazarsfeld and his colleagues, and one of his key concerns was to combine quantitative and qualitative approaches. Before immigrating to the United States, Lazarsfeld lived and worked in Vienna. During this period, he was one of the key researchers and authors of the Marienthal study (Lazarsfeld et al., 1933/1971), which has since become a classic in the sociological canon. The study was an investigation of one Austrian village—Marienthal—and was pioneering in its

in-depth analysis, combining both quantitative and qualitative approaches. In his later work, Lazarsfeld developed the methodological insights gained from this and other studies (1972), publishing several key texts on methods (most notably Lazarsfeld and Rosenberg [1955]—and many editions thereafter); and, in these, he warned researchers about the dangers of simple coding and classification techniques, often stressing the need for researchers to analyze their data as it was in the process of being collected and categorized.

Much of this resonates with Glaser and Strauss’ characterization of GTM, albeit in a far less amenable and articulated form. Although there are now several variants of the method, one of the key aspects of any truly grounded method study is the way in which the processes of gathering, sorting, and analyzing the data continue simultaneously and iteratively. At later stages of the research, data will be sorted into or compared against categories or codes, but these will themselves be products of the earlier stages of the research, rather than delineations and distinctions preconceived prior to the start of the study itself.

This intertwining might be thought of as a spiral, with foundations in the early data, gathered in a wide and encompassing manner, then moving upward and inward toward a more focused and directed view of some key aspect or aspects of the research domain. As Glaser and Strauss demonstrated in their early studies, and as many have since demonstrated, this approach can result in detailed models or theories that combine conceptual cogency with relevance and utility.

The Results and Value of Qualitative Research

In some cases, qualitative research can produce outcomes that can be criticized as failing to offer more than impressionistic (re)description—that is, simply taking various accounts or observations of some domain of interest and weaving them into a narrative with little or no conceptual depth or practical relevance. As stated earlier, however, an equivalent failing also haunts the world of quantitative methods: results that are based on incorrect or inaccurate use of statistical methods and meaningless or ambiguous hypotheses (see Goldacre’s vivid and readable account of “Bad Science,” 2009; also his blog at <http://www.badscience.net/>). Research is a process fraught with a variety of pitfalls and problems requiring a combination of skill, experience, serendipity, and, sometimes, plain dumb luck. This applies equally to all forms of research, whether

predominantly quantitative, qualitative, or a combination of several methods and approaches.

Glaser and Strauss, from the very beginning of their work together, stressed that the outcomes of a grounded theory study—that is, the grounded theory itself—had to adhere to some specific criteria, but ones that were distinct from those often held up as necessary for hypothesis-based, deductive research. They termed these *grab*, *fit*, *work*, and *modifiability*. At first sight, these might appear to be somewhat vague, but the terms are explained in some detail in the latter chapters of *Awareness* and sections of *Discovery*.

As I have explained elsewhere (Bryant, 2009), the use of these terms can best be understood in the light of the work and ideas of the pragmatists, specifically John Dewey (1999) and William James (1904). Dewey, in particular, promoted the idea of theories as tools—to be judged by their usefulness, rather than their truthfulness. This link between pragmatism and GTM was rarely mentioned by Glaser and Strauss in their joint publications in the 1960s, and Glaser never makes any reference to it in his later, solo writings. Strauss, for his part, does refer to pragmatism as “a red thread running through my work” (1993, p. 22) in his last book, *Continual Permutations of Action*, which is not regarded as part of his output on GTM and qualitative methods. Strauss was heavily influenced by pragmatism via his contact with G. H. Mead and others associated with the early Chicago School. In *Awareness*, chapter 14 is entitled “The Practical Use of Awareness Theory” (p. 259), and the footnote on that page does make specific reference to Dewey’s concept of a theory as something that is instrumental. But this is perhaps the only indication in Glaser and Strauss’s work—in concert or individually—of any relationship between GTM and pragmatism. Whatever the actual and acknowledged links between pragmatism and GTM might be, situating these four criteria against pragmatist ideas does shed light on each of the terms, enhancing the ways in which they can be understood as guidelines for evaluating the outcomes of research as follows:

- *Grab*: This is a characteristic of a substantive grounded theory. It relates to Dewey’s idea of a theory being judged in terms of its usefulness, rather than on any abstract principle of veracity. If a grounded theory has *grab*, this might be demonstrated in the way in which the actors from the research setting respond when it is explained to them—they will understand and engage with

it, using it in their activities and practices. Jeanne Quint’s development of innovative nursing practices and the ways in which these were taken up by colleagues and fellow professionals are prime examples of this feature.

- *Fit*: This term refers to the need for theoretical insights to adhere to the substantive context, rather than to the predilections or biases (conscious or unwitting) of the researcher(s). Glaser offers further thoughts on this issue in *Theoretical Sensitivity* (1978), stressing that the categories resulting from a GTM study should fit the data. How this is accomplished, and the cogency with which it is demonstrated and argued, will depend on the researcher(s) and the relevant published outputs. It should be thought of as an overarching aim to be striven toward in any GTM-oriented research.

- *Work*: This again builds on the idea of a theory as a tool. Tools are useful within specific contexts or for specific tasks. There are no general-purpose tools suited to all and every situation and job. The anticipated outcome of a GTM-oriented research project ought to be a *substantive* grounded theory—that is, one that is of use in the context from which it has been drawn and within which it has been grounded. Thus, any such theory ought to be able to offer explanations and insights that perhaps previously were unrecognized or implicit and also provide a basis for consideration of future actions and directions. If such a substantive theory is then enhanced and developed to a wider class of contexts, it can claim *formal* status. One of the earliest examples of this was Strauss’s work on *negotiated orders* (Strauss, 1978), which extended some of the aspects of the research that led to Glaser and Strauss’s early writings.

- *Modifiability*: One of Glaser and Strauss’s criticisms of hypothesis-based research was that, far too often, by the time a research project had been completed—passing from derivation and proposal, through investigation, to eventual proof or disproof—things had moved on and, as a consequence, the finding and conclusions proved to be of little or no relevance. Furthermore, the process of conceptual discovery is not to be thought of as a once-and-for-all activity, but rather as a continuing and continuous dialogue. Thus, grounded theories have to be understood as modifiable, rather than as fixed, definitive statements for all time.

Epistemological and Ontological Issues

Practical men, who believe themselves to be quite exempt from any intellectual influence,

are usually the slaves of some defunct economist.
– (John Maynard Keynes, 1964, p. 383)

The 1960s witnessed various other challenges to academic orthodoxy, although these seem not to have been of any real concern to Glaser or Strauss, since neither one makes extended reference to them in their writings on GTM and associated methodological matters. One of the key challenges emanated from a variety of critiques of what was perceived as the dominant model of social science research and theorizing in the United States at the time, most notably the structural-functionalist approach exemplified in the work of Talcott Parsons (1949, 1951). Apart from being seen as inherently conservative in its orientation, this stance was also criticized for placing far more emphasis on social structures and stability at the expense of social actors and agency. Part of the reaction to this view came from the work of the Chicago School of sociology, which stressed the importance of social actors' views in creating and sustaining social contexts and institutions, including, in the 1950s and early 1960s, the work of Strauss himself, as well as others such as Erving Goffman and Howard Becker (Becker, 1963; Becker, Geer, Hughes, & Strauss, 1961; Goffman, 1959).

With hindsight one can see the continuity between this facet of the Chicago School and the development of GTM. A significant aspect of the grounded nature of GTM arises from its focus on direct participation in the research context by the researcher(s), often including observation of and interviews with those involved. As will be explained later, the derivation of initial codes that encapsulate key features of the research context can themselves originate with the outcomes of these early interviews, based on the actual words and phrases used by the interviewees.

As has already been argued, GTM was presented by Glaser and Strauss as a challenge to the orthodoxy of research practice at the time. Moreover, it appears reasonable to argue that another aspect of their challenge drew on the ideas Strauss in particular had encountered, and contributed to, during his time in Chicago. Similarly Glaser had himself taken on, and significantly enhanced, some of the methodological insights on offer from familiarity with Lazarsfeld and colleagues at Columbia. So there is a case to be made for the influence of these lineages in the development of GTM, although this is in no way to detract from the innovative nature of GTM itself.

What is surprising, however, is the lack of any engagement with a further aspect of the range of

challenges to academic orthodoxy at the time, as embodied in the work of Thomas Kuhn. Kuhn's book *The Structure of Scientific Revolutions* (1962) created a major stir in the 1960s and is now regarded by many as one of the key works of the twentieth century. Apart from anything else, he challenged widely accepted views of science, scientific research, and the ways in which our knowledge of the world has developed and might be thought of as progressing in the future. His use of the term "paradigm" undermined the view that one could observe the world from a completely neutral position. At around the same time, Peter Berger and Thomas Luckman encapsulated a similar set of arguments in their book *The Social Construction of Reality* (1966), and both books contributed to what can be termed a *constructivist* or *interpretivist* model of knowledge—that is, that our understanding of reality is apprehended and sustained through social processes and interactions.

This position was articulated specifically to challenge various forms of *positivism* that, broadly understood, assumes the possibility of some neutral form of observation as a basis for discovery, testing of theories, hypotheses, and other claims to knowledge. The 1960s was marked by a variety of attacks on various forms of "conventional wisdom," and Glaser and Strauss's work can be seen as one component of this. What is surprising, however, is that neither Glaser nor Strauss makes any extended reference to any of these other, contemporary developments. Kuhn's argument incorporated what was seen by many as a highly unflattering characterization of science in nonrevolutionary periods—which he termed "normal science"—as "puzzle solving," rather than what might be termed discovery of new knowledge.

This resonates to a large extent with Glaser and Strauss's criticism of social science research as "proletarian testing" of the grand conceptions of the "theoretical capitalists." Conversely, one of the main thrusts of Kuhn's argument was that scientific revolutions amounted to a paradigm shift, which was not simply an enhancement of previous knowledge but a completely different way of seeing the world. For instance, the shift from a geocentric view of the universe to a heliocentric one involves studying common aspects of the natural world but seeing them in totally different ways. Likewise, someone with a grounding in natural sciences from the late seventeenth or early eighteenth centuries would, quite literally, see things very differently from someone with a grounding in natural sciences from the late eighteenth century onward—something illustrated by Kuhn in his description of the work undertaken

by Joseph Priestley in the late eighteenth century. Priestley is now accredited with discovering oxygen, but Kuhn argues that Priestley's own account of his experimental findings indicates that he continued to adhere to accepted wisdom rather than accept what we would now understand as the idea of air and other materials being composed of basic elements such as oxygen. (Priestley argued to his dying days that his observations were of something called "de-phlogisticated air," whereas Lavoisier, who heard of and repeated Priestley's experiments, wrote about his observations of the properties of oxygen.)

One of the key consequences of the ideas of Kuhn and others was that there was no such thing as a neutral standpoint from which to observe and explain the world. Taken further, this leads on to the argument that the ways in which we describe the world, using language, are not neutral or transparent; language is not simply a way of describing reality, it is actually a crucial part of how we constitute reality. Taken as a whole, these developments—many of which actually predate the twentieth century in one form or another—culminated in the 1960s in a concerted attack on simple and straightforward ideas about data and observation. But neither Glaser nor Strauss ever took these up in any way. On the contrary, Glaser and Strauss, whether in their collaborative or separate contributions, consistently treat "data" as an uncomplicated concept. Moreover, in using the term "emergence" in a passive and unembodied sense—as in "the theory emerges from the data"—they cannot help but oversimplify the nature of data and the process of "discovery," also obscuring the active role of researchers in shaping the development of codes, categories, and concepts.

By the late 1980s and early 1990s, GTM had grown in popularity, particularly following the publication of Strauss's solo work *Qualitative Analysis for Social Scientists* (Strauss, 1987) and his collaborative work with Juliet Corbin, *Basics of Qualitative Research* (Strauss & Corbin, 1990, 1998)—now in its third edition (Corbin & Strauss, 2008). Many doctoral researchers and others more advanced in their academic and research careers were taking up GTM, presenting proposals and findings that drew on *Discovery* and *Basics* in particular. Reviewers and research advisers found themselves presented with proposals that did not emanate from clearly formulated research questions or present hypotheses to be tested but that rather outlined generic areas of concern or specific contexts to be explored prior to articulation of clear objectives or issues. Moreover, research papers reported findings in which categories

were derived from the intertwining of simultaneous and iterative processes of data gathering and analysis, with the outcomes often presented as having "emerged from the data."

This presented evaluators, reviewers, and assessors in general with a number of problems and concerns. Some of these emanated from the innovations in the method itself, others from the ways in which researchers reported their findings and the details of the processes they followed.

Innovations

For those used to assessing research proposals in terms of the hypotheses presented or the clarity of the objectives articulated at the outset, GTM-oriented examples were something of a conundrum. Often, such proposals gave only a very generic and ill-defined account of the nature of the planned research, with little if any overview of the relevant literature, and only the slightest indication of the detailed instruments and methods to be used. This led to GTM proposals being treated as lacking in sufficient detail for any assessments to be made, and the method itself was seen as apparently providing the researchers—particularly doctoral and masters students—with a justification for only a limited amount of preparation prior to embarking on various, often ill-defined, research activities. Thus, the strengths of the method had come to be seen as its inherent weaknesses. In part, this was based on a misunderstanding of GTM by those in positions of authority claiming knowledge of methods, but it was also due to the ways in which the method was described in various texts and the manner in which it was then taken up by enthusiastic but inexperienced researchers keen to use alternative approaches.

Reporting of Findings

Although there may have been misgivings with regard to use of GTM and, as a consequence, some basis for limiting its growth, in many areas—particularly those associated with the pioneering work that emanated from UCSF in the 1960s and early 1970s—a significant proportion of research publications claimed use of GTM. It rapidly became the most widely claimed of any qualitative method, and, in some areas, it eclipsed all other methods—qualitative and quantitative—taken together. Editors and reviewers, however, were often perplexed by some of the GTM-oriented papers that they received. In many cases, these papers seemed to indicate that GTM amounted to nothing much more than stages of data gathering—usually in the

form of open-ended interviews—followed by analysis of this data to produce codes or categories, which then mysteriously led to the “emergence” of some end result. This result itself was sometimes termed a “grounded theory,” but often its conceptual or theoretical claims seemed at best weak and often nonexistent. Moreover, the writers of such accounts often stated that they deliberately ignored any literature that might have shed light on the generic research area and had set off on their research “without any preconceptions” or had somehow discounted any potentially relevant experiences, ideas, or preexisting knowledge that might influence their investigations. Terms such as “theoretical sensitivity,” “emergence,” “theoretical sampling,” and “theoretical saturation”—sometimes accompanied by fleeting references to “grab,” “fit,” and “work”—were perhaps mentioned (often merely in passing) to provide some indicators of rigor and substantiation, but the overall effect on many reviewers and their ilk was one of bewilderment and suspicion.

Constructivist GTM

The overall result of these shortcomings was that GTM came to be regarded as methodologically frivolous or near vacuous. Those with positivist inclinations, particularly if they adhered to Lord Kelvin’s assumptions concerning measurement and quantitative techniques, saw GTM as lacking in any firm foundation (no hypotheses at the outset) and deficient in terms of rigor (no measurement or quantitative verification). Conversely, those with interpretivist predispositions regarded the method as naïve and simplistic, given the characterizations offered by its progenitors—and then parroted by users—of terms such as “data,” “emergence,” and “induction.” Lois Wacquant (2002, p. 1481) encapsulated this when he described the method as one founded on “an epistemological fairy-tale.”

From the 1960s until the mid-1990s, neither Glaser nor Strauss ever engaged with the ways in which the work of Kuhn, Berger and Luckman, and others of a similar ilk undermined conventional ideas about data, observation, and knowledge claims. Given the central role played by “data,” particularly in Glaser’s writings, this seems somewhat strange; after all, Glaser and Strauss had set out to challenge the research orthodoxy, including those who acted as the gatekeepers and evaluators of theoretical legitimacy and authority. Kuhn’s ideas similarly sought to question the basis on which claims to knowledge were based; a critical enterprise that continues to this day. As I have argued elsewhere (Bryant, 2009), this

omission was particularly perplexing with regard to Strauss, given his background, steeped in the work of G. H. Mead and pragmatism.

Whatever the rationales behind both Glaser’s and Strauss’s specific failures to engage with these issues and ideas, there was no way that GTM could remain remote from or indifferent to them. By the mid-1990s, Kathy Charmaz had begun to articulate what she termed a “constructivist” form of GTM, and, in the second edition of the *Handbook of Qualitative Research* (Charmaz, 2000), she developed her argument, contrasting “constructivist” GTM with “objectivist” GTM, as espoused by Glaser.

For Charmaz, GTM had to take account of the active role of the researcher in moving from data collection through analysis to coding, then iterating through further stages of collection and analysis and coding. Thus, codes and categories did not “emerge” but were the product of deliberate interpretation by the researcher(s). She contrasted this view of GTM with what she termed Glaser’s “objectivist one,” which treats data as something uncovered by the research process, leading to the unearthing of codes and categories, and virtually effacing the researcher as an active participant. Thus, in her later book, Charmaz (2006) used the title *Constructing Grounded Theory*, rather than Glaser and Strauss’s *Discovery*.

Soon after this, in the late 1990s and quite independently, I had begun to develop a similar view. I had been presented with several research proposals that alluded to GTM, and, in many cases, this was no more than a thin veneer, hiding the student’s inability to state any clear ideas regarding specific objectives, lack of familiarity with the literature, or aversion to rigorous methods, particularly quantitative ones—sometimes all three. In most cases, when challenged, the student would agree to revise the proposal, remedying the deficiencies and opting to use some other, more prescriptive method. One student, however, persevered with GTM and was able to respond to the criticisms in a manner that indicated the strengths of the method. My own further examination of texts and sources such as *Awareness*, *Discovery*, and *Basics*, indicated that there were indeed valuable and important features of GTM, but that these needed to be separated from the language within which much of the GTM-oriented literature was based—what I termed “the GTM mantra.”

Writers claiming use of GTM often resort to variations or verbatim quotes of one or more of what might best be termed “the mantras of grounded theorists”—for example, “entering the research domain with an open mind,” “allowing the theory to emerge

from the data,” “letting the data speak for themselves/itself.” Invocation of any or all of these should not be seen as inevitably leading to inadequate research, although, as has already been pointed out, such statements inevitably lead many reviewers and evaluators to be suspicious of or discount whatever follows.

In the wake of the work undertaken by Charmaz, myself, and others to develop the method in the light of the critiques of positivism or objectivism—particularly those emanating from a constructivist or interpretivist position—two issues come to the fore for anyone using or evaluating GTM:

- Data now becomes a problematic concept and cannot simply be incorporated into research without further consideration. Glaser’s admonition against “immaculate conceptualization” is an indispensable part of the researcher’s mindset, but equally essential is an understanding that although the original meaning of “datum” (plural “data”) is something that is “given”—i.e., obvious and apparent and ready-to-hand—our processes of cognition are not as mechanistic and simple as this.

- Developing from this is the argument that participants in research settings will encompass multiple standpoints and conceptions of the specific context. Early statements of GTM clearly incorporate this to some extent; for instance, the work on awareness describes the ways in which different people develop and communicate their awareness across different settings. But this range of viewpoints must also include the researcher or research team—something that is missing in early GTM writings and was not really attended to in any systematic manner until Charmaz’s work from the late 1990s onward.

In 2006, Kathy Charmaz published an extended statement of constructivist GTM—*Constructing Grounded Theory*, thus contrasting this approach with one oriented around “discovery.” Charmaz argues that taking an explicitly constructivist standpoint does impact on the research itself, since data collection will necessarily involve researchers taking account of people’s meanings, intentions, actions, and interpretations both in terms of actually engaging with participants—using interviews—or for other forms of data collection, such as observation. Moreover, this leads to a specifically reflective position on the part of the researcher who now has to consider his or her own participation and interaction in the research setting.

Since the 1990s, researchers have been faced with a number of possible forms of GTM. Initially,

the fundamental distinction was that between Glaser’s work and Strauss’s later writings, particularly his joint work with Corbin. This distinction centers on a number of issues around the process of the method itself, particularly ideas about coding and the use of various frameworks or guidelines for developing concepts. The distinction between Glaser’s “orthodox” or “traditional” or “objectivist” GTM and constructivist GTM relates to the ways in which researchers seek to couch the form of justification for their ideas—constructed or discovered. Although there has been a good deal of debate around this issue, when it comes to carrying out research itself, one’s epistemological stance is often only of passing interest. The most important feature of research is its outcome, and it seems to make little or no difference whether the researcher conducted the research from a positivist/objectivist viewpoint or an interpretivist/constructivist one. Glaser and Strauss were correct to see the criteria of a research outcome—concept, theory, framework, or model—in terms of grab and fit, thereby offering alternative criteria for evaluating research outcomes.

The conclusion with regard to GTM and epistemology is that, although it may be useful for researchers to clarify their own disposition, ultimately, this may not really be a factor of any great import. In which case Wacquant’s jibe evaporates, and the true value of GTM lies in its application and impact on the research contexts in which it has been used.

GTM in Practice

The Grounded Theory Method (GTM) comprises a systematic, inductive, and comparative approach for conducting inquiry for the purpose of constructing theory (Charmaz, 2006; Charmaz & Henwood, 2007). The method is designed to encourage researchers’ persistent interaction with their data, while remaining constantly involved with their emerging analyses. Data collection and analysis proceed simultaneously and each informs and streamlines the other. The GTM builds empirical checks into the analytic process and leads researchers to examine all possible theoretical explanations for their empirical findings. The iterative process of moving back and forth between empirical data and emerging analysis makes the collected data progressively more focused and the analysis successively more theoretical. GTM is currently the most widely used and popular qualitative research method across a wide range of disciplines and subject areas. Innumerable doctoral students have successfully completed their degrees using GTM. (Bryant & Charmaz, 2007b, p. 1)

GTM is a method for qualitative research.¹ It offers an alternative to hypothesis-based research, stipulating that, at the outset, the researcher(s) should not seek to articulate concepts or hypotheses to be tested, but rather that the initial aim should be to gather data as the basis for developing the research project in its initial stages. This can appear perplexing both to researchers and assessors, since there seems to be little in the way of guidance with regard to the research topic itself. In practice, however, researchers always do have some idea of their topics of interest and should be able to offer some initial characterization of the contexts that they are keen to study. This may be a specific location, a set of practices, or specific issues that have engaged the researcher's interest.

Glaser and Strauss were keen for researchers to approach their study without having formulated ideas about the nature of the "problem" or the specific research question to be asked. In this way, they wanted researchers to be ready to be surprised by their findings, rather than looking for things based on their preconceived ideas. In some cases, researchers have misunderstood this admonition and have made mysterious and frankly laughable claims along the lines of "ignoring" or somehow disconnecting from their own existing knowledge of potentially relevant ideas, concepts, and other materials. (It is this claim, together with the magical invocation of "theory emerging from the data," that lies at the heart of accusations of GTM being founded on an epistemological fairytale.) Ian Dey (2007) has provided a pithy corrective to this, which should be remembered by all researchers, whether or not they use GTM: "an open mind is not the same as an empty head."

Bearing this in mind, a grounded theory study should begin with some characterization of the research context and can then continue with the posing of some open-ended and wide-ranging questions. Glaser and Strauss suggested the following high-level GTM questions:

- What is happening here? (Glaser, 1978)
- What is this data a study of? (Glaser, 1978, p. 57, Glaser & Strauss, 1967)
- What theoretical category does this datum indicate? (Glaser, 1978) ("What Is Grounded Theory," PowerPoint presentation, Kathy Charmaz, 2008 http://eprints.ncrm.ac.uk/208/1/What_is_Grounded_Theory.ppt)

If researchers are concerned or confused about the term "data," Glaser has clearly and consistently affirmed that "All is data." This means that

researchers can and should plunge into their research context and start looking for data. This may be in the form of initial, open-ended interviews, but it can also be in the form of observations, texts, documents, and anything else that might be relevant.

One of the developments emanating from the constructivist account of GTM can be seen in the range of basic questions that a researcher should be prepared to pose at the outset of a research project. This is not to say that, prior to this, GTM researchers failed to consider such issues; rather, that the constructivist position necessarily prompts researchers toward such considerations. Thus, Charmaz (2006) offers several further questions that develop GTM in a more specifically constructivist manner than is evident in Glaser's and Strauss's work. She stresses that articulations of answers to the "what is happening here?" question lead to consideration of "basic social processes" and/or "basic psychological processes," which Glaser mentions in *Theoretical Sensitivity* (1978). Unlike Glaser, however, who remains silent on such matters, Charmaz stresses that such consideration depends on the assessments and judgments made by the researcher(s) reflecting on the findings, and such reflection may encompass analysis of the data using further questions such as:

- From whose viewpoint is a given process fundamental?
- How do participants' actions construct [observed social processes]?
- Who exerts control over these processes?
- What meanings do different participants attribute to the process? (Charmaz, 2006, p. 20)

Taken together, all of this gives some guidance to researchers who are faced with the inevitable and awkward issue of how and where to start the research. But it provides a very different starting point from more traditional methods, particularly those developing from hypotheses. This latter approach has been described as *deductive*, since the hypotheses are often derived—deduced—from existing theoretical frameworks or models. This allows researchers to frame a specific research question, which then guides later activities such as the initial engagement with the research context, sampling, method, and analysis. Researchers following GTM eschew this strategy in favor of a far more open-ended one that many have described as *inductive*, since it relies on gathering data from which more generic patterns or conceptualizations can be ascertained.

In an age of formal evaluations and institutional review boards or committees, this can be problematic, since researchers will usually be expected to offer clear and concise research questions or hypotheses at the outset, accompanied by a critical review of the relevant literature, in order to sustain the argument that the proposed research offers some value and validity in terms of novelty or affirmation of existing claims. GTM-based research needs to provide other criteria at these early stages, and this can be problematic. Glaser's position has always been that GTM researchers should avoid the relevant literature at the outset, but, in practice, this often proves impossible and inadvisable. Review committees expect that researchers can position their proposals against existing work, and this can only be done on the basis of a critical review of the literature. Moreover, GTM researchers themselves often point out that they need to explore existing work in order to have confidence in their own studies and ideas.

In recent years, there has been a burgeoning literature offering guidelines and justifications for many qualitative research methods specifically aimed at assisting reviewers and evaluators, as well as researchers, in assessing proposals oriented around methods such as GTM (see Bryant, 2012). This should provide a more supportive basis for consideration of such proposals, particularly GTM, where the initial stages provide such a crucial aspect in guiding the later ones.

Coding, Memoing, Theoretical Sampling, Theoretical Saturation

For many people, GTM is regarded as a method that relies on "coding"; indeed, for some, this is the be-all and end-all of the method. Thus, some research papers claiming use of GTM offer nothing further than reference to interview data, together with some codes that have been developed from that material. The outcome is then presented in the form of a diagrammatic model linking these together in some manner. Partly as a consequence of this, many editors and reviewers have something of a low regard for GTM. Many researchers, however particularly those in the early stages of their careers and undertaking doctoral research, start to use GTM and find themselves overwhelmed by the outcome of early coding exercises on their data. It is not unusual for such researchers to produce several hundred codes from one or two initial interviews and then to double this number for subsequent ones—not so much "saturation" as inundation.

As was pointed out earlier, coding was not unique to Glaser and Strauss's conception of GTM,

although the way in which it is incorporated into the method certainly was, in that codes are developed subsequent to the start of data gathering. For many researchers, GTM relies on interview data, and this forms the source material for coding. But it is worth recalling Glaser's dictum of "all is data" and understanding this as encompassing many other types of source material, for example, documents, articles, web pages, tweets, and so on.

To illustrate some of the issues around coding and the way in which the method progresses, it is best to use some examples, even if they are somewhat constrained. To start with, Table 7.1 shows an extract from a paper on GTM (Giske & Artinian, 2007); the text on the left-hand side is taken verbatim from an interview, the comments on the right-hand side are the researchers' initial codes.

These initial codes can be thought of as ways in which the researcher has sought to highlight some key aspects of the "data." For those writing from a basis in "traditional" GTM, as claimed and exemplified by Glaser's work, this is seen and described in terms of the initial stages in the process of emergence. But the use of a phrase such as "the theory emerges from the data" is problematic, since it obliterates the active roles of the researcher(s). Different researchers may well look at the same data and produce a range of codes; some may well be common to several or all co-researchers, others may only have been developed by one researcher. The example in Table 7.1 is the work of more than one researcher and so may well have come about in its published form only after discussion and revision among the research team. This is grist to the mill for those working within a constructivist orientation; different people will construct or develop codes as the result of complex interactions between themselves and the "data." This goes on in a far less formal manner all the time and is readily exemplified by the comments section appended to articles on the web; these often result in such disparate comments from readers that one wonders if they have all read the same article.

In GTM, the coding process is far more rigorous and develops through use of the method, as will be described later. But, to demonstrate the initial stages, readers are invited to look at the brief extract—Table 7.2—from an article published in the UK newspaper *The Guardian* in late March 2012 as this essay was first being drafted. The column on the right-hand side has been left blank; in a manner similar to that shown in the earlier extract, try to come up with some initial codes of your own. Details of the full article are given as Doctorow (2012).

Table 7.1 Open coding example extracted from Giske & Artinian

Data	Open coding
Sometimes you think about the worst, you know, but they have informed me that they have taken so many tests; I have been to gynaecological examination, they have taken lots of blood samples, and my liver is OK, and they find nothing. But even though it lies there smouldering. (Interview 3)	Thinks about the worst Uncertain despite many samples and no findings Smoulders
It is important for me to get to know, to be able to move on, either with treatment, that I am well, or that I have to live with this. If they can tell me; OK, this is nothing dangerous, you can come to controls, so can I manage to live with the pain. But I have to know the reason why it is so. (Interview 9)	Wants to know to move on Can live with it if She knows why
I read a book I brought and I listen to music to possess another world while I am here. I need to overcome a threshold to get rid of what my head is full of.	Try to think of other things

Source: A personal experience of working with classical grounded theory: From beginner to experienced grounded theorist³

Table 7.3 shows the codes that I have made on the basis of my reading of the “data.” Some of the codes you have produced may be similar to those on the right-hand side, others may well be different. The constructivist orientation clarifies the interactive process that underlies the production—construction—of these codes. Those you have produced will depend not only on the extract itself, but also on a host of other factors bearing on your own experiences, interests, and way of understanding and interpreting the extract itself.

One possible set of codes, differing markedly from those in Table 7.3, might have come from someone deciding to focus on the extract from a journalistic perspective, one responding to the question “what is

happening here?” in the sense of contextualizing the article as something published by a British newspaper generally regarded as taking a liberal, or left-of-center stance on many aspects, particularly those concerning citizens’ privacy and rights. There is no right or wrong set of codes to be derived from this initial process; only codes that might prove to be useful in developing an explanation, a model, a theory of some aspect of social life. Glaser and Strauss exemplified this in their early work, with their first extended GTM publication focusing on “awareness” and their subsequent one focusing on “time.”

There are several ways in which initial codes can be developed, and researchers can and should try several of them when first starting to use GTM. The coder in

Table 7.2 Coding exercise: open/initial coding

Data	
Many big firms use “lawful interception” appliances that monitor all employee communications, including logins to banks, health providers, family members, and other personal sites. Even firms that don’t require self-signed certificates in their employees’ computers may use keyloggers, screenloggers, and other spying tools to watch what you do and capture your passwords. If your employer, school, or institution gets to control the software on your computer, you can’t know that it’s not snooping on you at all times. Just ask the kids in the Lower Merion School District, whose school-issued laptops were loaded with software that let school administrators covertly watch students at home and at school through the computers’ webcams.	Try to produce some open codes in a manner similar to that for the extract from Giske et al. in Table 7.1—do this <u>before</u> you turn to the next page!

Table 7.3 Coding exercise: ideas for initial codes

Data	Open codes
<p>Many big firms use “lawful interception” appliances that monitor all employee communications, including logins to banks, health providers, family members, and other personal sites. Even firms that don’t require self-signed certificates in their employees’ computers may use keyloggers, screenloggers, and other spying tools to watch what you do and capture your passwords. If your employer, school, or institution gets to control the software on your computer, you can’t know that it’s not snooping on you at all times. Just ask the kids in the Lower Merion School District, whose school-issued laptops were loaded with software that let school administrators covertly watch students at home and at school through the computers’ webcams.</p>	<p>Use of IT by companies/employers</p> <p>Interception and monitoring</p> <p>Employees communicating practices</p> <p>Recording and capturing</p> <p>Control of software/computer hardware</p> <p>Snooping and watching</p>

Table 7.1 broke down the data into smaller units and then summarized each part using terms similar or identical to those used in the original. You may have adopted a similar strategy in developing codes for Table 7.2. The important point to note is that there is no one, correct way of coding; GTM research is oriented toward the development of a model or theory that is “grounded” in the data in some substantive fashion, so that it has “grab,” “fit,” and the like.

I have deliberately used the plural form—researchers—in order to stress that, although much of the GTM literature implies that research is carried out by a single person, in practice, this not usually the case. Carolyn Wiener, in her chapter on teamwork and GTM, offers some important observations on this issue, illustrating her account with observations from her experience as a member of the team that Strauss set up for a GTM research project in the 1970s (Wiener, 2007). Moreover, even when there is a lone researcher—as in the case of most PhD research—this person should be encouraged to discuss codes and coding with their research advisors and their peers. This is common to all strands of GTM, with Glaser continuing to offer GTM workshops where issues such as coding can be discussed with others.

In these early stages, as well as coding, GTM researchers must record their ideas in the form of memos. Memos are a critical part of GTM, and memoing is an activity that often proves extremely valuable to other forms of research. In the earliest stages, memos may be created in the form of fairly unstructured notes and comments about the developing research, focusing on the researcher’s

experiences in using the method, as well as on the early results themselves. Thus, an early memo might be in the form of a researcher, new to GTM, reflecting on the experience of coding. Alternatively, an early memo, related to the extract in Table 7.1, might add some detail to the context of the two interviews used in the coding—interviews 3 and 9—which then might be used in later stages.

As the research develops, memos become more formal in the sense that they should be written with an eye on a wider readership and perhaps eventual publication and dissemination. Glaser has suggested that researchers should aim to develop a set of memos that can then provide the basis for publications. This may not always be possible, but GTM researchers should certainly bear in mind that memoing is an important component of the method, one that should be undertaken in a serious and consistent fashion throughout the research itself. (Further examples of memos can be found in Charmaz, 2006, chapter 4.)

All coding in GTM should start with “open coding.” Charmaz defines coding as

the process of defining what the data is about. Unlike quantitative data which applies *preconceived* categories or codes to the data, a grounded theorist creates qualitative codes by defining what he or she sees in the data. Thus, the codes are emergent—they develop as the researcher studies his or her data. The coding process may take the researcher to unforeseen areas and research questions. Grounded theory proponents follow such leads; they do not pursue previously designed research problems that lead to dead-ends.

Open coding is the first stage of coding and usually involves close scrutiny of data. If the data are in the form of written documentation or verbatim or near-verbatim interview transcripts, then this may be done line-by-line or even word-by-word. The examples given in the Tables 7.1–3 demonstrate this level of analysis. The idea is to capture certain key aspects of the data, reducing the complexity by providing a smaller number of more abstract terms.

Subsequent strategies will depend on what has transpired from these initial efforts and also on the choices made by the researcher or research team. But what all strategies have in common are ways in which they facilitate the move from a large number of codes, often anchored in the actual terms or phrases used in the source data, to a narrower set of high-level codes that encompass the richness of the source materials in some manner. This may involve the researcher choosing one specific aspect of the research context for further development, as exemplified in Glaser and Strauss’s first GTM study that focused on the concept of “awareness.” Only later did they develop a second concept of “time” (Glaser & Strauss, 1968).

If we return to the first example in Table 7.1, the right-hand side of the table now includes these later codes (Table 7.4)—classified by these authors as “selective coding.” Note that these codes can be seen to encompass the earlier codes but work at a higher level of abstraction. Again, it is not a case of them being correct or incorrect, but being judged in terms of whether or not they move the process of conceptualization

forward in the articulation of a useful, grounded theory.

Glaser has consistently advocated that researchers seek to develop codes based on gerunds, and Charmaz strongly supports this. Gerunds are the verb forms of nouns, so, in English, the gerund form of the noun “interception” is “intercepting.” Using gerunds should focus the attention of the research on the processes and actions that, in part, constitute the social context under investigation. Taking this into account, the more focused codes for the extract from *The Guardian* might now be revised along the lines shown in Table 7.5 —although several of the original codes were themselves in gerund form.

At this stage, it might be useful to create a memo for “Employer intercepting and monitoring”:

Employer intercepting and monitoring
Employer intercepting and monitoring

A wide range of employers seek to monitor the use of IT and related technologies by their employees. Increasingly, this monitoring extends to a wide range of communication practices, and the monitoring itself has been taken up by other groups, including school administrators checking up on students’ use of school-issued laptops.

Consider the growth of mobile technologies and the extent to which employers might claim justified monitoring of employees using their work-supplied mobile devices such as smart phones, tablet PCs, etc.

Table 7.4 Open and selective coding example extracted from Giske & Artinian

Data	Open coding	Selective coding
Sometimes you think about the worst, you know, but they have informed me that they have taken so many tests;	Thinks about the worst	Ambivalence
I have been to gynaecological examination, they have taken lots of blood samples, and my liver is OK, and they find nothing.	Uncertain despite many samples and no findings	Uncertainty
But even though it lies there smouldering. (Interview 3)	Smoulders	
It is important for me to get to know, to be able to move on, either with treatment, that I am well, or that I have to live with this. If they can tell me;	Wants to know to move on	To receive information
OK, this is nothing dangerous, you can come to controls, so can I manage to live with the pain. But I have to know the reason why it is so. (Interview 9)	Can live with it if she knows why	
I read a book I brought and I listen to music to possess another world while I am here. I need to overcome a threshold to get rid of what my head is full of.	Try to think of other things	Create a room of rest

Table 7.5 Coding exercise: open and selective codes

Data	Open coding	Selective coding
<p>Many big firms use “lawful interception” appliances that monitor all employee communications, including logins to banks, health providers, family members, and other personal sites. Even firms that don’t require self-signed certificates in their employees’ computers may use keyloggers, screenloggers, and other spying tools to watch what you do and capture your passwords. If your employer, school, or institution gets to control the software on your computer, you can’t know that it’s not snooping on you at all times. Just ask the kids in the Lower Merion School District, whose school-issued laptops were loaded with software that let school administrators covertly watch students at home and at school through the computers’ webcams.</p>	<p>Use of IT by companies/ employers Interception and monitoring Employees communicating practices Recording and capturing Control of software/computer hardware Snooping and watching</p>	<p>Employer intercepting and monitoring Employee communicating Recording and capturing Controlling Snooping and watching</p>

Once a researcher has developed his or her ideas to something akin to this level of conceptualization, there is a basis for “theoretical sampling,” a GTM practice that Glaser and Strauss defined as “the process of data collection for generating theory whereby the analyst jointly collects, codes, and analyses his data and decides what data to collect next and where to find them, in order to develop his theory as it emerges” (Glaser & Strauss, 1967, p. 45).

And Charmaz notes that “when engaging in theoretical sampling, the researcher seeks people, events, or information to illuminate and define the boundaries and relevance of the categories. Because the purpose of theoretical sampling is to sample to develop the theoretical categories, conducting it can take the researcher across substantive areas.”

In effect, this amounts to a more directed and focused search for evidence that might uphold, enhance, or undermine the initial ideas generated from the earlier findings. Researchers using GTM need to make this move clear in reporting the progress of their work, so that there is no misunderstanding about the strategy employed to identify the sample used.

The issue arises of how large a sample is required for the research to provide the basis for any reasonable and justifiable conclusions. GTM deals with this under the heading of “theoretical saturation”: “the point at which gathering more data about a theoretical category reveals no new properties nor yields any further theoretical insights about the emerging grounded theory” (Charmaz, 2006).

This has proved to be an elusive concept in the literature, and many researchers and reviewers, among

others, have wondered not only what the term actually means, but how a researcher might know that he or she has reached this position. In straightforward terms, the response to this is that, for instance, in research based on interviews, saturation is reached when responses given in later stages of the interviewing process yield confirmation of earlier findings, but nothing significant or new. In such cases, the researcher can decide that no further interviews are necessary, and the research itself can be moved on to its final stages.

Some commentators have argued that this decision point appears to be somewhat arbitrary and that, all too often in the literature, the researcher simply reports that saturation was reached, with little or no evidence for this. With regard to the former point, the decision to stop further gathering of evidence based on some criterion of sufficiency applies to all forms of research: when does one have enough data to start to draw some conclusions? In quantitative research, this usually takes the form of statements regarding the size and nature of the sample and its relationship to a wider population. In qualitative research, this is less clear cut, but amounts to the same thing. The key is for researchers to clarify the basis on which they made this decision, so that readers and assessors can decide whether this was indeed justified, and subsequent researchers can then ascertain if there might be a basis for developing this research in other areas or with other respondents. In all cases, there is always the possibility of what might be termed the “black swan research event”; that is, a research finding that completely undermines the pattern that seems to

have been developing from findings to date. But that is an inescapable aspect of all forms of investigation.

Using the Literature

Researchers are usually expected to have reviewed the literature relevant to their research topic early in the process. In this way, they can justify their proposal in terms of existing research, current issues and concerns, and the like. When Glaser and Strauss introduced the idea of GTM, they were keen to ensure that researchers, particularly early-career doctoral students, were presented with an alternative to the literature-derived form of research that was predominant at the time, in which doctoral students studied the works of the great theorists and developed their research on some aspect of this.

The outcome was that GTM was seen as advocating that researchers should not engage with the literature in the early stages of their work. Glaser, in particular, has constantly advocated that researchers stay away from the relevant literature until much later in their research, although he has also stressed that researchers should not take this as a reason to stop reading; on the contrary, one should read avidly and widely.

There are a number of problems with this position. The main one is that researchers need to have some familiarity with the current status of work that has been carried out in the general area in which they are interested; otherwise, they have no basis on which they can claim novelty or justification for their plans. Indeed, one of the reasons they plan to do their research may well be that they have knowledge and even practical experience of the area and its key issues. Keeping an open mind is certainly important, but either pretending to have an empty head or deliberately making it so by avoiding the literature is not a feasible option, particularly if one has to present one's proposal to a review board.

The result is that there is no way of avoiding some form of literature review in the early stages of one's research. But, in the context of GTM, there are a number of issues to take into account. One of these is that the literature itself can be treated as "data," with the researcher pointing to key issues and concerns and using these as the basis for some initial coding. This may well help in developing a proposal that, although devoid of specific research questions and hypotheses, still provides readers and assessors with an understanding of the general research area, as well as with the basis for some confidence that the research will develop and lead to appropriate outcomes.

In subsequent stages of the research, it may well prove to be the case that the findings lead away

from the initial ideas, often quite markedly. Even if they do not, once the researcher has developed the basis for a new model or theory, there is a need to go back to the literature in a far more focused manner, in order to hold up one's concepts against those most closely related to the eventual findings. So, the response to anyone who criticizes GTM for ignoring the literature is to point out that, on the contrary, the method requires at least two stages of engagement: one at the start and a potentially more rigorous one near the end of the process.

Results, Theories, and Publications

This chapter is designed to give you a brief overview of GTM, rather than a detailed account. The stages from initial coding through to more focused coding can take a great deal of time, effort, and ingenuity, but that is common to all forms of research. The extent to which research can be supported by methodological recommendations is a controversial one. Glaser and Strauss parted company on precisely this point in the 1990s, with Glaser accusing Strauss of undermining their concept of GTM with what Glaser saw as a far too prescriptive account of coding and generation of theories. (Various accounts of this can be found in Glaser, 1992; Bryant & Charmaz, 2007*b*, 2007*c*)

One of the key issues for GTM, however, must be the outcome and its dissemination. Whatever the differences might be between the various approaches to GTM—Glaser and Strauss's, Strauss and Corbin's, Glaser's, Charmaz and Bryant's—they all share the aim of providing researchers with a series of pointers to guide them from early ideas and insights toward substantive theories or models that have "grab" and "fit" and that "work" in some manner. The way in which these criteria might be assessed will depend on others having access to the account of the research itself, either in the form of published papers or perhaps more directly as a presentation by the researcher to the other participants (Turner, 1983).

Some of these issues can be illustrated using the examples presented earlier. The full table of codes from Giske et al. (2007) is shown in Table 7.6, with all three stages of coding. There are now three "final concepts," all in gerund form. If readers refer to the full paper, they will find a very clear and succinct account of the way in which the researchers moved from this to a grounded theory of "preparative waiting."

Giske et al. (2007) present their results not only in diagrammatic form, but also with textual explanation. This combination is a practice to be strongly

Table 7.6 Open and selective codes, and final concepts example extracted from Giske & Artinian

Data	Open coding	Selective coding	Final concepts
Sometimes you think about the worst, you know, but they have informed me that they have taken so many tests; I have been to gynaecological examination, they have taken lots of blood samples, and my liver is OK, and they find nothing. But even though it lies there smouldering. (Interview 3)	Thinks about the worst Uncertain despite many samples and no findings Smoulders	Ambivalence Uncertainty	Balancing between hope and despair Seeking and giving information Seeking respite
It is important for me to get to know, to be able to move on, either with treatment, that I am well, or that I have to live with this. If they can tell me;	Wants to know to move on	To receive information	
OK, this is nothing dangerous, you can come to controls, so can I manage to live with the pain. But I have to know the reason why it is so. (Interview 9)	Can live with it if she knows why		
I read a book I brought and I listen to music to possess another world while I am here. I need to overcome a threshold to get rid of what my head is full of.	Try to think of other things	Create a room of rest	

encouraged because diagrams are often useful in summarizing lengthy expositions and also in guiding readers in the development of research accounts; however, they rarely, if ever, serve as satisfactory explanations on their own. A picture may well be worth a thousand words, but researchers need to ensure that the thousand words conjured for the reader bear some resemblance to those intended by the writer.

Theoretical Sensitivity

This is in many ways the holy grail of GTM and, indeed, of research in general. Kelle summarizes it as follows: “In developing categories the sociologist should employ *theoretical sensitivity*, which means the ability to ‘see relevant data’ and to reflect upon empirical data material with the help of theoretical terms.” Glaser’s book of this title (1978) is a “must read” for those interested in GTM, and it should also be on the reading lists for all courses on research methods and research design.

The concept is very much a case of what might be termed “IKIWISI” rather than “WYSIWYG”; that is, *I’ll Know It When I See It*, rather than *What You See Is What You Get*. This is not particularly helpful as a response to novice researchers who ask for more information about the term and perhaps even expect some clear and concise guidelines for ensuring this aspect. The term “grab” is relevant here, since it can also be applied to the way in which

one’s research findings “grab” the imagination of one’s peers and colleagues in the relevant research community. Moreover, it brings into consideration the ways in which researchers actively participate in shaping or constructing their studies and eventual findings; that is what Kelle (2007) meant by a researcher’s ability to “see relevant data.”

Perhaps it is best to think of theoretical sensitivity as a research horizon; something that is always in front of us, but which inevitably recedes as we approach it. In any case, it will usually be presumptive of a researcher to claim that he or she has this sensitivity; far better to present one’s findings and assess the ways in which one’s colleagues respond, using this as a guide to the extent to which theoretical sensitivity has been demonstrated.

Alternative Approaches

The various exchanges between Glaser and Strauss in the light of their individual accounts of GTM, and the more recent ones focused on “objectivist” and “constructivist” approaches, might lead researchers to believe that there is some fairly strict gatekeeping going on with GTM. To some extent, this is correct, since there are many instances in which use of the method has been claimed in research proposals and publications but amounts to no more than a cursory incorporation of some aspect of GTM—usually the coding of data after some initial phase of collection.

However, there are many cases in which researchers have used GTM in unorthodox ways, but with good reason and producing results with “grab” and “fit.”² For instance, one of my PhD students had set out to administer a fairly structured questionnaire among a group of potential respondents but found that their background stories were far more interesting and did not fit into her initial research strategy. Rather than “forcing” these responses into her initial framework or simply ignoring the rich information that she had unearthed, she changed tack and started to analyze her data using GTM techniques. Since she had already gathered her data, I advised her to code one or two of her interviews and then see what transpired. Eventually, she managed to develop a set of codes and applied this to her other interviews and observations, resulting in a model that certainly had grab and fit.

Future Directions: What Is a (Grounded) Theory Anyway?

I have deliberately used terms such as “model,” “framework,” “theory” almost interchangeably in the earlier sections. Some writers make specific distinctions between these terms, but I have chosen not to do so. One of the issues with regard to use of GTM is the expectation that the outcome of any such research should result in a theory—but what exactly is a theory, whether of the grounded variety or any other type?

There is currently a good deal of discussion about the status of the term “theory.” Those arguing in favor of some form of “creationism” or “intelligent design” often make statements to the effect that “evolution is *only a theory*,” that it is not fully proven and therefore alternative claims to knowledge, however tenuous or problematic, must be granted equal status. This is to confuse the meanings of the term. In cases such as the theory of gravity, or relativity, or evolution, the term refers to a body of knowledge and concepts that have stood both the test of time and an extended time of testing and various forms of rigorous investigation. In more colloquial use, people talk about their own particular “theories” of anything from the origin of the universe, the economic crash of the last decade, or how to pick winners in horse races—in this sense, a theory is no more than a guess or a hunch.

In an earlier paper (Bryant, 2009), I noted that, for pragmatists such as John Dewey and William James (particularly Dewey), a theory was something to be judged in terms of its usefulness rather than its truthfulness. Consequently, a theory should be

regarded as a tool, and a tool is only useful for certain tasks. This, in fact, characterizes what Glaser and Strauss mean by the term “substantive theory” as opposed to “formal theory.”

By substantive theory we mean theory developed for a substantive or empirical area of sociological inquiry, such as patient care, geriatric life styles etc.... By formal theory we mean theory developed for a formal or conceptual area of sociological area such as status passage, stigma, deviant behavior, etc. (Glaser & Strauss, 1967)

So, terms such as “grab,” “fit,” and “work” can then be seen as ways in which research outcomes can be judged, whether these results are regarded as theories, models, frameworks, or something else. In all cases, the outcome can be evaluated in terms of whether it has some use within the context from which it was derived. These criteria should not be restricted to GTM-oriented research, but if this form of research is to be assessed in terms of its “theoretical” outputs, then it is important that the nature of such results is understood.

GTM has developed into a mature family of methods and now provides researchers with a host of possible strategies, techniques, and guidelines. It is important that the intricacies and rich potential of GTM are understood, both by researchers and by those who judge and evaluate research proposals, funding applications, and articles submitted for publication. Use of the method continues to grow and so, too, does the supporting literature on the method itself. The extent to which researchers now have to articulate their methodological strategies is to be welcomed, but not if it starts to obscure the actual research itself. It is important that those involved in research, particularly those in positions of authority whose decisions can encourage or deter research projects, understand the intricacies of the plethora of research methods; and also that researchers themselves clarify and justify their research approaches so that their various audiences can assess the ways in which their efforts have achieved fruition.

Locating GTM within the pragmatist tradition, as I have argued elsewhere (Bryant, 2009), implies an understanding of the process of research as a continuing dialogue. All outcomes must be seen as, at best, provisional, affording the basis for further research and investigation. In the light of this, I conclude by offering some issues for readers to ponder and also a list of sources, to some of which I have added a brief indication or comment.

- To what extent is a researcher’s epistemological position important in guiding their research? Has it been an issue in your own research or in the way in which you have framed research proposals with which you have been involved?

- There is now a wide variety of software tools available, either specifically aimed at GTM or supporting qualitative research in more general ways. To what extent do such tools impact on the research process, either positively or negatively?

- Try to read several articles in which the researchers indicate that they have used GTM. How do these differ from each other? What do they have in common?

- GTM-based research does not start out with specific hypotheses; indeed, hypotheses can be the result of this form of research. How should such hypotheses be taken up and used in further research? Can you find any examples in the literature in your field of expertise?

Suggestions for Further Reading

Although the three books published by Glaser and Strauss in the 1960s are rightly regarded as the founding texts for GTM, the best introduction to the method itself—together with clearly worked examples of coding, memo-writing, and other key features—is to be found in Charmaz’s *Constructing Grounded Theory*. Glaser’s *Theoretical Sensitivity* should be read thoroughly, as should the Appendix to Glaser and Strauss’s *Awareness*. The *Handbook of Grounded Theory* provides a valuable overview of many aspects of GTM in recent years, with contributions from Glaser, as well as from many of those who were part of the UCSF doctoral program in the 1960s. There are also chapters from German-speaking contributors who were influenced directly or indirectly by Strauss as he lectured on the method in Germany.

If you contemplate using GTM in your own research, you should use keywords or other searches to review recent journals in your area of study to find examples of the ways in which others have used the method. This seems to go against Glaser’s line that you should not look at the relevant literature until you reach the later stages of your research. But this seems far less feasible with the burgeoning of research and the demand by reviewers and evaluators that a case be made for a research proposal to demonstrate awareness of existing work, together with critical insights regarding prior work and the methods employed. It is worth reiterating Dey’s point about “an open mind not being the same as

an empty head”—something that should apply to all forms of research.

Notes

1. This section offers only a brief account of the method—a more detailed exposition will appear in my forthcoming book on GTM (Bryant, 2014).
2. Several examples of this will be described in my forthcoming book.
3. Tove Giske, Bergen Deaconess University College Bergen, Norway; Barbara Artinian, School of Nursing Azusa Pacific University Azusa, California © 2007 Giske et al. This is an Open Access article distributed under the terms of the Creative Commons Attribution License (<http://creativecommons.org/licenses/by/2.0>), which permits unrestricted use, distribution, and reproduction in any medium, provided the original work is properly cited.

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Feminist Qualitative Research: Toward Transformation of Science and Society

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Abstract

Feminist research is described in terms of its purposes of knowledge about women's lives, advocacy for women, analysis of gender oppression, and transformation of society. Feminist critiques of social science research are reviewed in relation to the development of methodological and epistemological positions. Feminist research is viewed as contributing to the transformation of science from empiricism to postmodernism. Reflexivity, collaboration, power analysis, and advocacy are discussed as common practices of feminist qualitative research. Several qualitative approaches to research are described in relation to feminist research goals, with illustrations of feminist research included. Validity and voice are identified as particular challenges in the conduct of feminist qualitative research. Intersectionality and double consciousness are reviewed as feminist contributions to transformation of science. Some emerging and innovative forms of feminist qualitative research are highlighted in relation to potential future directions.

Key Words: Feminism, standpoint theory, social construction, postmodern, methods, methodology, epistemology, essentialism, sex differences, qualitative, objectivity, validity, voice, consciousness

What Is Feminist Research?

A starting principle of feminist research is that psychology should, at minimum, be nonsexist. Feminist scholars have identified numerous sexist biases in the existing psychological literature; psychological research is sexist to the extent that it incorporates stereotypic thinking about women or gender (McHugh, Koeske, & Frieze, 1986). Sexist bias also refers to theories or research that do not have equal relevance to individuals of both sexes and to research in which greater attention or value is given to the life experiences of one sex (McHugh et al., 1986). Research practices and methods that produce, promote, or privilege sex/gender inequalities are sexist and unacceptable.

Feminist research is research that is not only nonsexist, but also works actively for the benefit and advancement of women (McHugh et al., 1986)

and puts gender at the center of one's inquiry. Specifically, feminist research examines the gendered context of women's lives, exposes gender inequalities, empowers women, advocates for social change, and/or improves the status or material reality of women's lives (McHugh & Cosgrove, 1998; 2002). According to Letherby (2003), feminist researchers have a "political commitment to produce useful knowledge that will make a difference in women's lives through social and individual change" (p. 4). Feminist research is not research *about* women, but research *for* women; it is knowledge to be used in the transformation of sexist society (Cook & Fonow, 1990; McHugh & Cosgrove, 1998).

Feminist research cannot be fully identified by its focus on women or its focus on gender disparity, as sexist research may entail a similar focus. Furthermore, feminist research cannot be specified by any single

approach to the discovery or creation of knowledge, and feminist research is not defined by any orthodox substantive position (Jaggar, 2008a; McHugh & Cosgrove, 2002). However, feminist researchers share common perspectives. Hesse-Biber and Leavy (2008) identified three shared concerns: giving voice to women's lives and experiences, overcoming gender inequities at the personal and social level, and improving women's opportunities and the quality of women's lives. Hawkesworth (2006) argues for three similar commitments of feminist research: "to struggle against coercive hierarchies linked to gender (and other statuses); to revolt against practices, values and knowledge systems that subordinate and denigrate women; and to promote women's freedom and empowerment" (p. 7). Jaggar (2008a) described feminist research as distinguished by its dedication to the value of gender justice and its "commitment to producing knowledge useful in opposing the many varieties of gender injustice" (p. ix). According to Jaggar (2008a), feminist research can be uniquely identified by its dedication to the value of gender justice in knowledge and in the world. And the feminist commitment to women's emancipation requires knowing the situations and circumstances of women's lives; to determine what needs to be "criticized, challenged or changed," feminists need valid knowledge of the oppressions and marginalization of women (Code, 1995, p. 20). Feminist research is an approach to research that seeks knowledge for the liberation and equality of women.

To what extent can research, qualitative or otherwise, contribute to feminist goals of transforming society toward gender equality? Some feminists have questioned the liberation potential of research and especially the possibility of traditional (i.e., experimental, quantitative, and objective) research to produce knowledge that will alleviate gender inequity and oppression (e.g., Hollway, 1989). Keller (1982) viewed feminism and science as in conflict, but argued that the exploration of the conflict between feminism and science could be both productive and transformative. Some feminists have specifically called for the transformation of science to incorporate feminist values (e.g., Wiley Okrulik, Thielen-Wilson, & Morton, 1989). Feminist researchers, in their quest to transform society, have argued for and contributed to the transformation of (social) science research. In this chapter, I identify the dimensions and characteristics of feminist research and examine research practices and methodological and epistemological positions in relation to feminist tenets. Feminist research is not viewed

as a static entity, but as a transforming and transformative practice.

(Trained as a social psychologist, I identify as a feminist psychologist. I studied at the University of Pittsburgh, working with Dr. Irene Frieze. My first research study, conducted as an undergraduate student at Chatham College, a woman's college in Pittsburgh, examined problem-solving performance of women students as impacted by context; students completed a series of mathematical word problems in an all-female or a mixed-sex group. Women students performed better in a single-sex context in what today might be considered a study of stereotype threat. I pursued an interest in sex differences in graduate school, and my doctoral dissertation examined the intrinsic motivation of women and men as a function of task feedback. Over the course of my career, I became increasingly critical of both the experimental method of research and the study of sex differences. My own epistemological and methodological path parallels the progression of feminist research as described here.)

Feminist Research as Corrective

Feminists challenged the neglect of women's lives and experiences in existing social science research (e.g., Wallston, 1981; Weisstein, 2006). Feminists have criticized psychology (and other disciplines) both for not studying the lives and experiences of women and for the development of sexist research theory and practice (McHugh et al., 1986). One contribution of feminist research has been to offer a corrective to traditional research that either neglected women or presented a stereotypic or biased view of women. For example, early feminist research identified experiences of women including widespread gender discrimination and violence against women (Chrisler & McHugh, 2011; Jaggar, 2008a). As a corrective to research that neglects the study of women's lives, feminist research has transformed the content of research in most disciplines. The expansion of feminist research over the past four decades has transformed knowledge in the humanities, social sciences, and natural sciences (Hawkesworth, 2006). The transformation of psychological science was examined by a task force of the Society of Women in Psychology (Eagly, Eaton, Rose, Riger, & McHugh, 2012). Eagly and the task force members documented the growth of published research on women and gender in the psychological literature and its movement from the periphery of the discipline toward its center. They concluded that research on women is now situated

as a methodologically and theoretically diverse content area within contemporary psychological science. Yet, by their broad definition, psychology of women and gender articles accounted for few (4.0 percent from 1960 to 2009 and 4.3 percent from 2000 to 2009) of the articles in the prominent journals of psychology. And for most of the research that Eagly and her colleagues documented, researchers did not label their research as feminist nor did the research explicitly address feminist goals of gender equality or advocacy for women.

A second important contribution of feminist researchers and theorists has been their critical analysis of research and the production of knowledge. Feminists have criticized research that characterizes women as having deficits and critically examined asymmetrical and inequitable constructions of the cultural masculine over the cultural feminine (Jaggar, 2008a). Similarly, Geiger (1990) characterizes feminist research as challenging the androcentric (male-centered) construction of women's lives, and Wiley (2000) notes that feminists question androcentric or sexist frameworks or assumptions that had been unchallenged. Pushing against that which is taken for granted, feminist inquiry probes absences, silences, omissions, and distortions and challenges commonsense understandings that are based on inadequate research. For example, feminists challenge conclusions about human behavior based on evidence taken from narrow (e.g., male, European-American, educated, and middle-class) samples of human populations (Hawkesworth, 2006). Furthermore, feminists exposed the (gender) power dynamics that operate in many aspects of women's lives, including in research, and have challenged existing explanatory accounts of women's experiences (Hawkesworth, 2006). One goal of feminist research then is to attend to the power dynamics in the conduct of research, to expose invisible or concealed power dynamics. The demonstration that gender and other contextual variables can create bias in the scientific research of individuals, and that such bias exists in the science accepted as valid by scientific community, is an important contribution of feminism to science (Rosser, 2008). Thus, one function of feminist research has been to call for the transformation/correction of science as a series of sexist and stereotypic depictions of women and of research that devalues women. Hawkesworth (2006) acknowledges the transformational character of feminist research as "interrogating accepted beliefs, challenging shared assumptions and reframing research questions" (p. 4).

(In 1975, I began teaching Psychology of Women, and I was keenly aware that there was very little research published on the experiences or concerns of women. As a member of Alice Eagly's Task Force on the Feminist Transformation of Psychology, I agreed that there has been an explosion of research on women and gender over the past four decades, which Eagly et al. effectively documented. However, I am ambivalent about the degree to which most of that research has improved the status or lives of women.)

Challenging Traditional Methods

The experimental approach has been critiqued as inauthentic, reductionistic, and removed from the social context in which behavior is embedded (Bohan, 1993; Sherif, 1979). Others have exposed the laboratory experiment as a social context in which the (male) experimenter controls the situation, manipulates the independent variable, observes women as the "objects" of study, and evaluates and interprets their behavior based on his own perspective (McHugh et al., 1986). From this critical perspective, the traditional psychological experiment is a replication of the power dynamics that operate in other social and institutional settings. The interests and concerns of the research subjects are subordinated to the interests of those of the researcher and theorist (Unger, 1983). Feminists have argued that the controlled and artificial research situation may elicit more conventional behavior from participants, may inhibit self-disclosure, and may make the situation "unreal" to the participants (McHugh et al., 1986). From this perspective, the experiment is not the preferred method of research.

Feminists challenged the pervasive androcentrism evidenced in empirical research. For example, in the 1980s, a task force of the Society for Women in Psychology examined the ways in which psychological research could be conducted in a nonsexist way (McHugh et al., 1986). The task force's guidelines (McHugh et al., 1986) challenged traditional empirical psychology by examining the role that the values, biases, and assumptions of researchers have on all aspects of the research process. There is always a relationship of some kind between the scientist and the "object" of study since the scientist cannot absent himself from the world (Hubbard, 1988). Selection of topics and questions, choice of methods, recruitment of participants, selection of audience, and the potential uses of research results all occur within a sociohistorical context that

ultimately influences what we “know” about a topic or a group of people (McHugh & Cosgrove, 2004). The realization of the operation of sexist bias in science/psychology led some feminist researchers to question the value of the scientific method and to more carefully consider issues of methods, methodology, and epistemology. The study of gender raised the issue of how context and values challenge traditional conceptions of objectivity (Rosser, 2008). The feminist challenge to the possibility of impartial knowledge and the recognition of the operation of values in science impacted the research conducted in some of the sciences (Rosser, 2008; Schiebinger, 1999).

Feminists, including Hollway (1989) and Hubbard (1988), provided a critique of the “context-stripping” and alleged objectivity of scientific research. According to Hubbard, the illusion that the scientist can observe the “object” of his inquiry as if in a vacuum gives the scientist the authority to “make facts.” She observed that science is made by a self-perpetuating group of chosen people; scientists obtain the education and credentials required and then follow established procedures to “make” science. The illusion of objectivity gives the scientist the power to name, describe, and structure reality and experience. The pretense that science is objective obscures the politics of research and its role in supporting a certain construction of reality. By pretending to be neutral, scientists often support the status quo. “By claiming to be objective and neutral, scientists align themselves with the powerful against the powerless” (Hubbard, 1988, p. 13). In terms of gender, male scientists’ alleged objectivity has given scientific validity to their mistaken contentions about women’s inferiority.

Feminist Epistemology

Prior to conducting research designed to address feminist goals, Harding (1987) advised feminists to understand the distinctions among methods, methodology, and epistemology. Others have similarly called for feminists to be aware of their epistemological positions and biases (e.g., Cosgrove & McHugh, 2002; Unger, 1988). Methods are the concrete techniques for gathering evidence or data such as experiments, interviews, or surveys. Methodology is the study of methods, the philosophical position on how research should proceed. Epistemology is the most central issue for feminist research according to Harding (1987), Stanley and Wise (1993), and others. Epistemology involves the study of answers to the question: How can we

know? Epistemology is a framework for specifying what constitutes knowledge and how we know it. An epistemological framework specifies not only what knowledge is and how to recognize it, but who are the knowers and by what means someone becomes a knower or expert (McHugh & Cosgrove, 2002). Epistemological frameworks also outline the means by which competing knowledge claims are adjudicated (Stanley & Wise, 1993). Harding (1986) identified three distinct feminist epistemological perspectives: empiricism, standpoint, and social construction. These epistemological perspectives are briefly reviewed here prior to a description of feminist qualitative research.

Feminist Empiricists

Feminist empiricism adopts the scientific method as the way to understand or know the world. Feminist empiricists believe in the scientific method for discovering reality; they assert that science is an approach that can provide value-neutral data and objective findings (Chrisler & McHugh, 2011; McHugh & Cosgrove, 2004). Their position is consistent with the modernist perspective. The modernist perspective endorses adherence to a positivist-empiricist model, a model that privileges the scientific method of the natural sciences as the only valid route to knowledge (Cosgrove & McHugh, 2008). From this perspective, there is a single reality that can be known through the application of the methods of science, including repeated objective observations. Objectivity refers to a dispassionate, impartial, and disengaged position and is valued. Bias is acknowledged as impacting scientific research but is viewed as a distortion that can be eliminated or corrected (McHugh & Cosgrove, 2004). The Guidelines for Nonsexist Research provide examples of errors and biases in research that should be eliminated (McHugh et al, 1986). Feminist empiricists attempt to produce a feminist science that, without androcentric bias, more accurately reflects the world (McHugh et al., 1986). To varying degrees, many feminists continue to conduct empirical research based on approved scientific methods.

(As a graduate student, I co-chaired (with Irene Frieze) the Task Force to Establish Guidelines for Nonsexist Research in Psychology for Division 35 of the American Psychological Association (APA). We started the project as empiricists hoping to help eliminate sexist bias from psychological research, especially research on sex difference. This experience introduced me to the diverse positions taken

by feminist scientists, and, in the process of addressing sexist bias in research, my own understanding of the limits of empirical research developed. I became increasingly critical of the scientific method even as I conducted a social psychological experiment involving some deception for my degree.)

Feminists have refuted “scientific” evidence that women are inherently different from and inferior to men. Feminist empiricists have employed the experimental methods of science to provide evidence for gender equality (Deaux, 1984; McHugh & Cosgrove, 2002). However, there is debate over the success of using science to refute sexism in science. Shields (1975) contended that research comparing men and women has never been value-free or neutral but rather has typically been used to justify the subordination of women. Alternatively, Deaux (1984) concluded that empirical evidence has been used to effectively change belief that differences between men and women are universal, stable, and significant, and Hyde (1986) endorsed the use of scientific and quantitative measures to debunk gender stereotypes. Eagly and her colleagues (2012) concluded that research on women and gender has transformed psychology over the past fifty years and has influenced public policy. However, McHugh and Cosgrove (2002), among others, have questioned whether the tools of science are adequate for the feminist study of women and gender. Burman (1997) argued that by employing empirical methods, feminist empiricists help to maintain a commitment to existing methods that neglect, distort, or stereotype women.

The study of sex differences is central to feminist psychology (McHugh & Cosgrove, 2002); arguments for the inclusion of women in social science research are based, in part, on the recognition that women have different experiences and perspectives. Critics, however, contend that research on sex differences typically leads to the devaluation and discrimination of women and confirms stereotypes (through biased methods) (e.g., Hare-Mustin & Maracek, 1990; 1994a). MacKinnon (1990) argued that “A discourse of difference serves as ideology to neutralize, rationalize, and cover up disparities of power” (p. 213). Feminists have argued that interest in sex differences involves interest in justifying differential treatment of women and men and that there is a confirmation bias operating. Research that “finds” a sex difference is more likely to be published, publicized, and cited than is research refuting the existence of a difference between men and women (e.g., Epstein, 1988; Hyde, 1994; Kimball,

1995; Unger, 1998). Furthermore, research is often constructed to produce sex differences (McHugh et al., 1986). For example, Kimball (1995) demonstrates how the research on sex differences in math ability has been carefully constructed to produce differences (i.e., the use of standardized tests administered to very large samples) and related research not demonstrating difference (i.e., classroom tests and research using smaller, more heterogeneous groups) is ignored.

Through the debate on the study of sex differences, feminists continued to recognize the politics of research. Increasingly, feminists recognized that research that supports the status quo and the view of women as less than men is more likely to be funded, conducted, published, and widely cited (Epstein, 1988; McHugh & Cosgrove, 2002; Unger, 1998). Sexist bias not only impacts the design and conduct of research but is apparent in the interpretation and distribution of the research results. Differences between women and men were typically labeled “sex differences.” This label implies that the demonstrated differences are essential (i.e., reside inside men and women) and are related to biology. Feminists argued that differences that were found were frequently due to prior experiences, gender roles, and/or the context and not to biology (Deaux, 1984; Hyde, 1986; Unger, 1998). Others argued that the behavior seen as characteristic of women is actually the behavior evidenced by people with low power and status (Hare-Mustin & Maracek, 1994a). Unger (1979) recommended that we use the term “gender” to avoid the biological connotation of the term “sex.” Despite this increasing sophistication in our understanding of gender as a function of context, roles, and power, gender differences continue to be constructed as essentialist (Cosgrove, 2003; McHugh & Cosgrove, 2002). Also, the research findings, even when they were published, did not impact the beliefs held by professionals or the general public about women and men and their performance on tasks. For example, despite the pattern of results across many studies (Frieze, McHugh & Hanusa, 1982; Frieze, Whitley, Hanusa, & McHugh, 1982), people continued to believe that women attributed their failures to lack of ability and their success as due to luck.

(Early in my career, I studied sex differences in response to task performance success and failure. I gave subjects ambiguous tasks that had no right or wrong answers and gave them false feedback about their performance. Some subjects were given success feedback; others were told that they had failed.

I then asked them how they explained their performance and about their expectancies for future performance. I abandoned this line of research when I realized that the debriefing I gave might not have been successful in erasing their emotional response to failing the experimental task. Others documented that women's response to novel tasks revealed low expectancies for success, thus biasing our understanding of women's (lack of) confidence. I did not want to contribute to individuals' feelings of failure, or to stereotypic and invalid characterizations of women.)

The realization that the questions asked by male theorists and researchers reflect their position in the world challenged the assumptions of logical positivism—including objectivity and value neutrality. Feminist research and theory has been criticized as political and biased, even as these critics continued to view research conducted by men as scientific and objective. Some feminist psychologists came to see the connection between individuals' status and identity in the world, the questions they were interested in, and their approaches to research. Thus, many feminist psychologists recognized that unexamined androcentric biases at both the epistemological and methodological levels resulted in women's experiences being devalued, distorted, marginalized, and pathologized (e.g., Cosgrove & McHugh, 2002; McHugh & Cosgrove, 2004).

Feminist Standpoint Perspective

The feminist criticism of science as biased led to a recognition of the importance of perspective or standpoint. Some critics have contended that individuals who are outsiders to a culture or group are more likely than insiders to recognize cultural or group assumptions (e.g., Mayo, 1982). Feminism provoked some feminist scholars to recognize male bias and to view aspects of male-dominated society, including the practice of research, through an alternative lens. The realization that women and men might view the world differently, ask different questions, and use different methods to answer those questions led some feminists to adopt a standpoint position. Hartstock (1983) argued that women's lives offered them a privileged vantage point on patriarchy and that such an epistemological perspective had liberatory value.

In the feminist standpoint perspective, women's ways of knowing are considered to be different from and potentially superior to men's ways of knowing (Belenky, Clinchy, Goldberger, & Tarule, 1986). As outsiders or marginalized individuals, women have

a unique perspective on their own experience, on men, and on sociocultural patterns of domination and subordination (Mayo, 1982; Westkoff, 1979). Like feminist empiricists, advocates of a feminist standpoint perspective typically accept the existence of a reality but recognize that one's position within a social system impacts one's understanding of that reality (McHugh & Cosgrove, 2004). A standpoint epistemological perspective argues that there are important research questions that originate in women's lives that do not occur to researchers operating from the dominant androcentric frameworks of the disciplines (Harding, 2008). Furthermore, standpoint theory has allowed some of us to recognize that traditional research has typically served the purpose of the researcher rather than the researched (Letherby, 2003); the experiences of marginalized people are not viewed as a source of interesting or important questions. For example, research on motherhood and women's experience of embodiment was not conducted prior to feminist influence on social science (Chrisler & McHugh, 2011).

Standpoint epistemology views the relationship between knowing and politics as central and examines how different types of sociopolitical arrangements impact the production of knowledge (Harding, 2008). The answers to questions about women and other marginalized groups may originate in the lives of marginalized individuals but typically involve an analysis of the social and power relations of dominant and marginalized groups to answer. Feminist standpoint epistemology calls for a critical analysis of women's experiences as described through women's eyes (Leavy, 2007). For example, DeVault (1990) documents the skills that women have developed from their work feeding their families, and Jaggard (2008b) examines women's skills at reading emotion as having developed through their care-taking roles.

In an important contribution to feminist standpoint, Smith (1987) argued that social science knowledge systems are used as systems of control and that those who develop knowledge are typically separated from everyday life. She describes knowledge as controlled by an elite (i.e., racially and economically privileged men) who have no interest in or knowledge of the women who serve their needs. Smith (2008) notes that questions regarding women's work originate in the consideration of women's lives, which have historically not been examined. Consideration of women's daily lives leads to the recognition that women are assigned the work that men do not want to complete and to the realization

of the processes by which that work is devalued and trivialized. Such insights are not constructed by the elite and may have liberatory value for women.

In an early consideration of this perspective, Westkott (1979) recognized that feminist researchers were both insiders and outsiders to science and that this was a source of both insight and a form of self-criticism. Furthermore, Westkott argued that the concern with the relationship of scientist/observer to the target/object stereotypically represents the focus of women on relationships, whereas the detachment of the traditional researcher is consistent with a stereotypic masculine role. Similarly, Letherby (2003) commented that androcentric (male) epistemologies deny the importance of the personal and the experiential, whereas the feminist researcher often values the experiential, the personal, and the relational rather than the public and the abstract.

In feminist standpoint theory, knowledge is mediated by the individual's particular position in a sociopolitical system at a particular point in time (Hawkesworth, 2006). In feminist standpoint perspectives, an oppressed individual can see through the ideologies and obfuscations of the oppressor class and more correctly "know" the world (Hawkesworth, 2006). Recognition of a feminist standpoint raises the possibility of other standpoints, and Fine (1992) argued that a single woman's or feminist standpoint was not plausible. Thus, race and class and other identities within the sociocultural system impact the individual's understanding of the world.

In particular, black feminist theorists (e.g., Collins, 1989) have articulated the existence of a black feminist standpoint, arguing that the position of black women allows them to recognize the operation of both racism and sexism in the sociopolitical system. According to Collins (1989), black women have experienced oppression and have developed an analysis of their experience separate from that offered by formal knowledge structures. The knowledge of black women is transmitted through alternatives like storytelling. Such knowledge has been invalidated by epistemological gatekeepers. Thus, black feminist standpoint theorists contend that at least some women have an ability referred to as "double visions" or "double consciousness" (Brooks, 2007). Smith (1990) similarly recognized in women the ability to attend to localized activities oriented to maintenance of the family and, at the same time, to understand the male world of the marketplace and rationality. The narrative of hooks (2000) as a black child in Kentucky reveals a double consciousness

with regard to her own community and the white world across the tracks.

Postmodern Perspectives on Research

The third epistemological position, the postmodern approach, challenges traditional conceptions of truth and reality. Postmodernists view the world and our understanding of the world as socially constructed and therefore challenge the possibility of scientists producing value-neutral knowledge (Cosgrove & McHugh, 2002; 2008). Postmodern scholars view attempts to discover the truth as an impossible project and equally reject grand narratives and the experimental method. From a postmodern perspective, life is multifaceted and fragmented, and a postmodern position challenges us to recognize that there are multiple meanings for an event and, especially, multiple perspectives on a person's life. Postmodern approaches examine the social construction of concepts and theories and question whose interests are served by particular constructions (Layton, 1998). Social constructionism requires a willingness to make explicit the implicit assumptions embedded in psychological concepts (e.g., identity, gender, objectivity, etc.). By doing so, social constructionists encourage researchers to recognize that the most dangerous assumptions are those we don't know we're making. From the postmodern position, all knowledge, including that derived from social science research, is socially produced and therefore can never be value free. Someone's interests, however implicit, are always being served (Cosgrove & McHugh, 2002).

The postmodern perspective emphasizes the relationship between knowledge and power. The postmodern perspective suggests that, rather than uncovering truths, the methods we use construct and produce knowledge and privilege certain views and discount or marginalize others (Cosgrove & McHugh, 2002; Gergen, 2001; Hare-Musten & Maracek, 1994b). Social constructionists are less interested in the answer to research questions and more interested in the following: What are the questions? Who gets to ask the questions? Why are those methods used to examine those questions? Postmodern thought can open a new and more positive way of understanding and can contribute to the transformation of intellectual inquiry (Gergen, 2001). Although some feminists have rejected the postmodern approach, Hare-Musten and Maracek (1994b) argued that interrogation of the tension between feminism and postmodern perspectives can be used to transform psychological research.

The conduct of feminist research from within the postmodern approach involves conducting research in which women's interests are served.

Postmodern feminists view empiricist and standpoint feminists as reverting to essentialist claims, viewing women as an identity. Cosgrove (2003) explains essentialism as viewing women as a group, as having a single point of view, or as sharing a trait (i.e., that women are caring). The standpoint position is that women have a shared perspective or a unique capacity (different from men's) or voice; the standpoint position is viewed as problematic from a postmodern perspective. Brooks (2007) explains the problem of essentialism of feminist standpoint theory: "Beyond the difficulty of establishing that women, as a group, unlike men as a group, have a unique and exclusive capacity for accurately reading the complexities of social reality, it is equally problematic to reduce all women to a group" (p. 70). Thus, the essentialism inherent in empirical and standpoint positions does not acknowledge the diversity and complexity of women's perspectives and voices and does not attend to the ways that gender is produced through socialization, context, roles, policies, and interactions. Cosgrove (2003) similarly explained that "the hegemony of the essentialist claim of women's experience or voice has had the unfortunate effect of reinforcing normative gendered behavior" (p. 89). Essentialism that views gendered behaviors as universal, biological in origin, and/or residing within women as traits or inherent characteristics is essentially problematic.

Gergen (1988) explained the relationship of research methods to essentialism. The decontextualized approach to traditional research results in studying women apart from the circumstances of their lives. Social and cultural factors including discrimination, violence, sexism, and others' stereotypes are eliminated from the view of the researcher. Subsequently, researchers are likely to attribute observed behavior as due to women's traits or natural dispositions. Gergen concluded that research should be conducted without violating the social embeddedness of the participant.

(I met Lisa Cosgrove when I was a faculty member at Duquesne University in 1985, having recently completed my degree. She was completing her doctorate in clinical psychology at Duquesne; at Duquesne, she was trained in phenomenological psychology with a very strong background in philosophy of science. A few years after she had graduated and moved to Boston, we began collaborating. Both feminists, I had experience as an empiricist

and she was trained as a clinician and a phenomenologist. We wrote a series of papers on feminist research, the study of gender and gender differences, and epistemological issues that are cited here and are the basis for this chapter. Discussions with Lisa led me to the adoption of a postmodern position in regards to feminist research.)

Implications for Feminist Qualitative Methods

I have briefly reviewed the feminist epistemological positions to illustrate alternative feminist positions and to trace transformations in the theory and conduct of feminist research and the development of feminist postmodernism. Equally important is the demonstration of how feminist criticism of logical positivist science relates to the development and use of qualitative research approaches. Feminist critiques of research led some psychologists to a loss of confidence in the scientific method; postmodern feminists object to the privileged status given to scientific researchers, especially the scientific method in the positivist tradition (Chrisler & McHugh, 2011). Feminist critics argued that the experimental method, including its reductionism, the creation of an artificial context, the failure to understand the context of women's lives, and the inherent inequality of psychological experiments is not a superior method for understanding the psychology of women. For example, McHugh, Koeske, and Frieze (1986) reviewed feminist arguments that context matters and that the methods of empiricism that decontextualize the individual may support oppressive status quo conditions. McHugh and her colleagues argued that the controlled and artificial research situation may elicit more conventional behavior from participants, may inhibit self-disclosure, and may make the situation "unreal" to the participants (McHugh et al., 1986). The impetus for the adoption of alternative epistemological positions came, in part, from the criticism that the scientific method put the experimenter in the position of influencing, deceiving, manipulating, and/or interpreting "subjects." Feminists working from a social constructionist perspective are interested in examining the implicit assumptions embedded in traditional psychological research and theory. For example, Unger (1979) acknowledged that our position regarding what constitutes knowledge is the basis for our choice of research methods and the usefulness of our research to advance women.

Feminist researchers seek approaches to research that advance our understanding of women without committing essentialist errors or contributing to gender inequities.

The idea that women need to express themselves (i.e., find their own voice and speak for themselves), rather than have their experience interpreted, coded, or labeled by men, is consistent with feminist standpoint theory. Qualitative methods are preferred by many feminist psychologists because they allow marginalized groups, such as women of color, to have a voice and to impact the conduct of research. Feminists value the representation of marginalized groups and the use of subjective and qualitative approaches that allow such participants to speak about their own experiences. Postmodern feminists might argue that liberation or equality may be enacted or experienced when women resist patriarchal conceptualizations of their/our experience and grasp the power to speak for ourselves (Chrisler & McHugh, 2011).

Values of Feminist Research

In contrast to traditional research, feminist research has paid special attention to the role that the values, biases, and assumptions of the researcher has on all aspects of the research process. Selection of topics and questions, choice of methods, recruitment of participants, selection of the audience, and the potential uses of the research results are choices made within a sociohistorical context that ultimately influence what we “know” about a topic or a group of people (cf. Bleir, 1984; Harding 1986; Keller, 1985; Sherif, 1979). Feminist research recognizes that, as a result of unexamined androcentric biases at both the epistemological and methodological levels, women’s experiences have been neglected, marginalized, and devalued. Feminist scholars, recognizing that values play a formative role in research, believe that values should be made explicit and critically examined (Hawkesworth, 2006). Feminist research is explicit in its ethical and political stance; feminist research seeks epistemic truth and social justice and challenges social bias as existing in some existing knowledge claims (Jaggar, 2008a).

Feminist researchers have explicated their value systems, realizing that an unbiased, objective position is not possible. Feminists are aware that the product cannot be separated from the process (Kelly, 1986) and strive to conduct research in an open, collaborative, and nonexploitative way. The voice of the participants is often the focus of the research, but the researchers themselves are encouraged to

reflect on and report their own related experiences and point of view (McHugh & Cosgrove, 2004; Morawski, 1994).

Reflexivity

Feminists have questioned the possibility of and the preference for value-free or neutral research and the value of the detached, disengaged researcher who is objective in the conduct of research. Not only do we all and always have some relation to the subject under study, but a connection to or experience with the phenomena may actually be an asset. As Brooks and Hesse-Biber (2007) suggest, “rather than dismissing human emotions and subjectivities, unique lived experiences, and world views as contaminants or barriers in the quest for knowledge, we might embrace these elements to gain new insight and understandings or, in other words, new knowledge” (p. 14). The feminist epistemological perspective pays attention to personal experience, position, emotions, and worldview as influencing the conduct of research (Brooks & Hesse-Biber, 2007). In feminist research, there is a realization that such connections cannot be removed, bracketed, or erased, but we do consider it important to reveal them. The researcher is expected to acknowledge her situated perspective, to reflect on and share how her life experiences might have influenced her choice of topics and questions.

In a related vein, Reinharz (1992) recommended that valid listening to the voices of others requires self-reflection on “who we are, and who we are in relation to those we study” (p. 15). Feminist research has frequently engaged in this process of questioning, referred to as “reflexivity.” The reflexive stance may involve critically examining the research process in an attempt to explicate the assumptions about gender (and other oppressive) relations that may underlie the research project (Maynard, 1992). Incorporating reflexivity is a complex and multidimensional project, one that necessitates a constant vigilance with regard to the epistemic commitments that ground our research (Cosgrove & McHugh, 2002).

In feminist research, a commonly used reflective approach is one in which the researcher provides an “intellectual autobiography” (Stanley & Wise, 1993) tracing her interest in relationship with and approach to the questions and to the research participants. Ussher (1991) for example, traces her interest in women’s madness to her mother’s “mental illness,” thus eliminating the illusion that she is a detached or disinterested knower. Hollway

(1989) also offers such an extended reflexive stance by deliberately and thoroughly examining how she made decisions and interpretations throughout her research on heterosexual relationships. Fine (1992) offers multiple examples of reflections on the research process, arguing that we should demystify the ways in which we select, use, and exploit respondents' voices. Letherby (2003) provides an extended examination and analysis of feminist research issues by describing her own history and her experience conducting individual and collaborative research interviewing women who experienced infertility and childlessness.

(In this chapter, I have included some of my own biography as a feminist psychologist. I hope to share part of my own journey, starting as an enthusiastic empiricist, then becoming a critic of biases in research, to the adoption of a view of research as political. Having traced that journey, I recognize the potential contribution and the potential risks that exist in any research undertaken, and I appreciate the diversity of feminist positions in research. Currently, I view myself as encouraging feminist researchers to recognize the problems identified by postmodern critics and to realize the potential for a postmodern perspective to resolve issues and dilemmas in feminist research.)

Power

Feminist researchers are cognizant of the impact of power on the research process. Jaggar (2008a) described feminist research as concerned with the complex relationship between social power (and inequalities in social power) and the production of knowledge. Part of the feminist critique of traditional research includes the power and authority of the researcher to construct and control the research process and product. In traditional science, the power of the researcher is connected to his position as an objective expert "knower" in relation to the uninformed and ignorant subject of his inquiry (Hubbard, 1988). Similarly Smith (1987) and Collins (1989) have examined the power of the educated elite to ignore and invalidate the experiences and knowledge of women and other marginalized groups. Feminist researchers challenge this oppressive status hierarchy in a number of ways. Feminists challenge both the objectivity and the expertise/knowledge of the scientist and view women (or men) participants as knowing about their own experiences. Feminists more than nonfeminists see power as a socially mediated process as opposed to a personal characteristic and recognize the role

of power in efforts to transform science and society (Unger, 1988). Thus, feminist research recognizes the power inherent in the process of research and attempts to use that power to transform society. If the purpose of feminist research is to challenge or dismantle hierarchies of oppression, then it is crucial that the research process not duplicate or include power differentials. Yet it is difficult to dismantle the competitive and hierarchical power relations present across most contexts of our lives, including the research context.

An identifying aspect of feminist research is the recognition of power dilemmas in the research process (Hesse-Biber, 2007b). Consistent with this perspective, feminist research is based on a respect for the participants as equals and agents rather than subjects. In an attempt to dismantle power hierarchies, the feminist researcher is concerned with the relationships among the research team; feminist research teams are ideally nonhierarchical collaborations (discussed later). Another dilemma is how to interpret or represent the voices of the women respondents; researchers are cautioned not to tell their story, but, in the postmodern perspective, one's own position always as part of the research process.

Collaboration

Based on critiques of the experimental method, feminist research has emphasized the need for a collaborative (rather than objectifying) focus. Feminist research seeks to establish nonhierarchical relations between researcher and respondent and to respect the experience and perspective of the participants (Worrell & Etaugh, 1994). Feminist psychologists challenge the regulatory practices of traditional research by developing more explicitly collaborative practices (cf. Marks, 1993). Collaboration necessitates an egalitarian context from the inception of the research process to the distribution of results. For example, instead of conducting an outcome assessment of a battered women's shelter based on the preferred outcomes suggested by agencies, researchers, or shelter staff (i.e., how many women have left their abusive relationships?), Maguire (2008) conducted participatory research with battered women examining a question they raised. As Lather (1991) notes, empowerment *and* empirical rigor are best realized through collaborative and participatory efforts.

Often, relationships among researchers and respondents, although referred to in the literature as partnerships, collaborations, or otherwise egalitarian relations, may be better characterized

as ambivalent, guarded, or conflicted (McHugh & Cosgrove, 2004). Being committed to seeing things from the respondents' position is a necessary aspect of feminist research, but it is also important to recognize our privileged position within our relationships with respondents and with co-workers. Often credentials and our status within the academy place us in a privileged position.

(Feminists idealize the collaborative approach, but I, like others, have experienced difficulties in some of my collaborations. Often, collaborations are not an experience of equality or sisterhood. Rather, differences in power, status, and experience can impact the collaborations, which may be more hierarchical than feminists might want. Feminist researchers may not recognize that they do not share the same epistemological perspectives. I also experienced differences in styles of working and writing as especially painful and problematic, in that class and worldview are incorporated in nonconscious ways.)

Research as Advocacy and Empowerment

Although I believe that feminist research should explicitly address issues of social injustice, the issue of doing research as advocacy is complex. It is impossible to know in advance how best to empower women and other marginalized groups. Indeed, many scholars have argued that researchers tend to position themselves as active emancipators and see participants as passive receivers of emancipation (e.g., Lather, 1991). Conducting and using research for advocacy requires the researcher to engage in critical reflection on his or her epistemic commitments. Feminists try to design studies that avoid objectifying participants and foster a particular kind of interaction. For example, participatory researchers work with communities to develop "knowledge" that can be useful in advocacy and provide the basis for system change. In terms of doing research with and for women, it is important to develop knowledge collaboratively and, whenever possible, share the knowledge with a wider audience. Often, empowerment is viewed as the process by which we allow or encourage respondents to speak for themselves or to find their voice. Certainly, teaching women to engage in speech or actions that are of our choosing is not empowering, but empowerment of other women is a complicated issue, as discussed below. Wilkinson and Kitzinger (1995) suggest that, in feminist research, we speak for ourselves and create conditions under which others will speak.

Challenges to Feminist Research Voice

An important contribution made by feminist researchers has been giving voice to women's experiences. Davis (1994) suggests that the notion of voice resonates with feminists who hope that women's practices and ways of knowing may be a source of empowerment and that speaking represents an end to the silencing and suppression of women in patriarchal culture. Many theorists have addressed the silencing of women, the ways in which the construction of knowledge by "experts" resulted in women's voices not being heard, not being taken seriously, or questioned as not trustworthy. "Women's testimony, women's reports of their experiences, is as often discredited... from their testifying about violence and sexual assault through their experiential accounts of maladies, to their demonstrations of the androcentricity of physics" (Code, 1995, p. 26).

At first thought, it might appear that the metaphor of voice and the methods designed around it (i.e., the qualitative analysis of women's narratives) have allowed feminist psychology to articulate women's experiences. However, closer examination of this metaphor and the research methods used to support it argue for a more critical examination of research that attempts to give women voice (Alcoff, 2008). The position that women can and must speak for women and/or that women can listen to each other differently than men has been challenged. Substituting a woman's standpoint for an androcentric position privileges women's way of being, speaking, viewing the world, and knowing, but the idea of women's voice also essentializes femininity and can reify the constructs of men and women. Feminist theorists have cautioned that in our attempts to correct psychology's androcentric perspective, we must avoid a position that essentializes masculinity and femininity (Bohan, 1993; Cosgrove, 2003; Hare-Mustin & Maracek, 1990) (i.e., one that views differences between men and women as universal and as originating or residing within men and women). Similarly, Davis (1994) questions whether the notion of voice is a useful one for feminist theory. Do women have voice (i.e., an "authentic" feminine self)? Does voice refer to "the psychological focus of femininity, the site of women's subordination, or the authentic expression of what women really feel" (p. 355)? The use of the voice metaphor raises questions of essentialism. Is there such a thing as femininity, which can be discovered or uncovered?

Other feminists (e.g., Tavis, 1994) reminded us that women (and girls) do not speak the same in all

situations, pointing out that there is more than a single “women’s voice” and that there is more than one way to hear the same story. Similarly, Gremmen (1994) questions whether authentic and false voices can be distinguished in the qualitative analysis of transcripts. Others have questioned whether women are speaking for themselves when their responses are reported, presented, organized, or otherwise produced by the researcher. The emancipatory potential of research is undermined when the researcher positions herself as an arbitrator of truth and knowledge or as a judge of what is or is not an authentic voice (Cosgrove & McHugh, 2000).

There is great value in questioning who speaks for whom; indeed, who speaks may be more important than what is said (cf. Lather, 1991; 1992; Rappaport & Stewart, 1997). When we speak for women or about women’s experience, we may distort or silence women’s own voices (Cosgrove & McHugh, 2000). Can we presume to know how to express the experiences of other women? The issues are further complicated when we attempt to “speak for others across the complexities of difference” (Code, 1995, p. 30); that is, speak for women who differ from us in terms of age, class, ethnicity, sexual orientation, region, and other dimensions (Alcoff, 2008). As feminist researchers, we might recognize the degree to which we have positioned ourselves as “universalizing spokesperson” and abandon that role, choosing instead the role of “cultural workers who do what they can to lift the barriers which prevent people from speaking for themselves” (Lather, 1991, p. 47).

Relinquishing the role of “universalizing spokesperson” requires a shift in how we conduct our research and in how we analyze our data. Marks (1993) encouraged us to reflect on the institutional power we have as researchers in order to avoid buying into the illusion of empowerment or democracy. To ensure that our hypotheses and questions are relevant, meaningful, and helpful to participants, we might ask participants to comment on, modify, add to, or even change the questions developed by the researcher. Standard research practice might include conducting a needs assessment and obtaining pilot data on the appropriateness of the focus, structure, and design of the research. The research process might begin with an opportunity for participants to voice their concerns and collaborate in the development of the research questions. In addition, Cosgrove and McHugh (2000) suggest that researchers adopt a cautious and reflective approach when editing participants’ narrative accounts. We

need to acknowledge and attend to the fact that editing changes the voice(s) heard. The way in which we frame and present quotes may involve implicit assumptions about our interpretive authority; when we are not including the entire narrative, we need to include a rationale for and a detailed description of our editing choices. The question of “who can/should speak for whom engages with issues of power and the politics of knowledge that are especially delicate in present day feminist and other postcolonial contexts” (Code, 1995, p. 26).

Struggles for the “power to name” are continually played out in politics, the media, and in the academy. Specific words are needed to describe concepts that are important to people; without those words, it is very difficult to think about—and nearly impossible to talk about—objects, ideas, and situations. Feminists have provided words and concepts to describe the previously unspoken experiences of women and girls (Smith, Johnston-Robledo, McHugh & Chrisler, 2010) including stalking, date rape, coercive sex, and intimate partner violence. Yet, our constructions and operational definitions of the phenomenon under study can also introduce limitations and distortions in women’s understanding of their own experiences (McHugh, Livingston, & Frieze, 2008). When we give a woman a label for her experience and outline for her the particulars of the phenomenon, we direct her attention and memory and impact her own construction of her experiences. In this way, science has claimed the power to name reality and has sometimes challenged the credibility of women to articulate and name their own experiences. Postmodern feminists are attentive to the power of words and examine how language or discourse is used to frame women’s experience.

Validity

Traditionally, objectivity has been equated with quantitative measurement and logical positivist approaches to science and is valued as the path to truth and knowledge. Qualitative research and research rooted in standpoint and postmodern epistemologies are frequently seen as subjective and are devalued as such. Feminist and other postmodern critics of logical positivism argue that objectivity is an illusion that has contributed (illegitimately) to the power of science and scientists to make knowledge claims (e.g., Hubbard, 1988). The position of a disengaged or impartial researcher who studies others as objects, without investing in their well-being, or the outcomes of the research, has been rejected. Objectivity in this sense is *not* seen as a superior way

to understand the world or the people in it. From a postmodern perspective, all knowledge involves a position or perspective that results in partial or situated knowledge. Furthermore, postmodern positions reject claims of grand theories and discoveries of some truth that exists “out there.” Knowledge is viewed as co-created or constructed in social interactions. Developing a theory of human behavior based on the study of a limited sample of people is viewed as inappropriate and universalizing. Some have exposed the issue of scientific objectivity as an elitist effort to exclude others from making meaning, a system by which all who are not trained to participate are devaluated and objectified (Hubbard, 1988; Schewan, 2008).

Feminist qualitative research as described here has not sought universal truths about women but has increasingly been focused on particular communities of women (people), and the research is “judged” as useful in terms of its contribution to the improvement of women’s lives or to the (re) solution of a locally defined problem. Yet, some feminist theorists have grappled with the issue of validity claims. Is every interpretation or conclusion based on qualitative “data” equally valid? How can we know or evaluate our research as valid, if not objective? Questions of validity and credibility (which are sometimes discussed in terms of objectivity) remain unanswered or contested in regards to feminist qualitative research.

Schewan (2008) addresses the question of objectivity, asking “What is it about objectivity that helps to make a claim acceptable?” She argues that we do want our claims to be acceptable to some broader constituency. What do we have to do to establish such credibility? Schewan’s (2008) answer to these questions revolves around questions of trustworthiness. Her argument for an epistemological trustworthiness involves multiple dimensions of credibility including, for example, research that is critical, contextual, committed, and co-responsible; and practical, political, pluralist, and participatory. Furthermore, Schewan contends that trust is ultimately a product of community, and a basic question we might ask about our own research is in which (and how broad a) community would we look for consensus on the validity of our research? In which context do we want to articulate our claims, and how might we be evaluated in that context. In participatory action research, the researcher typically would have the participants in the project provide feedback as to the accuracy, validity, and usefulness of the project “data.”

Similarly, Collins (2008) views community and connectedness as essential to establishing the validity of black feminist theory. She observes that in the African-American community new knowledge claims are not worked out in isolation, but in dialogue. An example of the dialogue for assessing the validity of black women’s concerns is the call-and-response interaction in African-American communities, including churches. Ideas are tested and evaluated in one’s own community, which is also the context in which people become human and are empowered. Black feminist thought emerges in the context of subjugated individuals. Each idea or form of knowledge involves a specific location from which to examine points of connection; each group speaks from its own unique standpoint and shares its own partial and situated knowledge. There are no claims to universal truth. Collins also notes that this approach to validation is distinctly different from scientific objectivity in that this dialogue involves community rather than individualism, speaking from the heart, and the integration of reason and emotion.

The feminist scientist may question objectivity but continue to return to the concept when designing a feminist science (Keller, 1985). Haraway (2008) and Harding (2008) are searching for a broader form of validation of claims; they articulate their ideas for a successor science and a feminist version of objectivity. Coming from the epistemology of standpoint theory, Harding (2008) anticipates the emergence of a successor science that offers an acknowledged better and richer account of the world. In response to questions of how to maintain validity and reliability in research when objectivity is challenged, Harding (1991) proposed the solution of *strong objectivity*. Her idea of strong objectivity is based on the outsider perspective (Mayo, 1982) or the double consciousness attributed to African Americans (Collins, 1990). In Harding’s approach to validity, individuals at the margins of the institutions of knowledge may provide an outsider perspective on the conceptualization not evident to the insiders at the center. Harding argued that outsiders can bring awareness of the ways in which values, interests, and practices impact the production of knowledge. Harding argued that including the perspectives of the outsider or marginalized perspectives can strengthen the objectivity of science while retaining validity (Rosser, 2008).

Haraway (2008) offers her vision of a usable doctrine of objectivity, *embodied vision*. Consistent with Collins (2008) and Schewan (2008), Haraway’s

ideas about validity relate to conversation and community; situated knowledge is about communities not individuals. Haraway proposes that our capacity for knowing involves embodied vision; that is, we are limited to partial and situated knowledge because our vision is limited by our body in a physical location. She contrasts this idea of situated and partial knowledge with the omnipotence and omnipresence of a male (god); thus, her conception of objectivity relates to where we are located in the world, as opposed to an objectivity that comes from being above the fray. Haraway recommended that we share our knowledge with others who occupy a different space to help construct a larger vision. Haraway calls for objectivity as *positioned rationality*, rational and fuller knowledge as a process of ongoing critical interpretations among a community of interpreters and (de)coders. In her vision, feminist objectivity would make for both surprises and irony (since we are not in charge of the world). As indicated here, feminist researchers employing qualitative and post-positivist methods continue to contend with the issue of validity. Current approaches emphasize knowledge as partial and situated (as opposed to universal truth) and the validity of knowledge as established through dialogue with participant communities.

Forms of Feminist Qualitative Research

In this section, I introduce a number of qualitative forms of research and examine them in relation to feminist goals for research. All possible forms of qualitative research are not introduced or described; the selection represents in part my own areas of interest or expertise. The forms of research addressed here can be undertaken from any feminist epistemological positions, and each of these is consistent with a postmodern perspective.

In-Depth Interviews

Interviewing is a valued method for feminist researchers, allowing them to gain insight into the lives and experiences of their respondents and potentially helping others to understand a group of women. Feminists are often concerned with experiences that are hidden, for example, the lives of marginalized women (Geiger, 1990). When the goal of the research is in-depth understanding, a smaller sample is used since the interviewer is interested in the process and meanings and not in the generalization of the findings (Hesse-Biber, 2007a). In more unstructured interviews, the researcher exerts very

little control over the process, letting the interview flow where the respondent goes.

Interviewing as a feminist research strategy is designed to get at the lived experience of the respondent (Nelson, 1989). Often, a goal of interviewing is to have women express their ideas, insights, or experiences in their own words. According to Letherby (2003), the method chosen in a feminist project should allow the voices of the respondents to be distinct and discernible. Feminist interviewing is conscious of the relationship between the researcher and the researched and of the ways that power operates in the interview and in the product of the project. Letherby (2003) describes variation in how much two-way conversation she engaged in, and she also describes the relationship between the researcher and respondent as dynamic and changing over time.

One feminist perspective on interviewing is that the researcher and the respondent co-construct meaning. Oakley (1981) espoused a participatory model that involves the researcher sharing aspects of her own biography with the researched. A more conversational and sharing approach invites intimacy. Oakley also sees this as a way to break down the power hierarchy. As an example, Parr (1998) traced her own development from a positivist researcher to a more feminist and grounded approach in her interviews of mature women who returned to education. Parr (1998) started with a barriers framework that she eventually abandoned when the respondents' stories did not fit this framework: the women did not perceive themselves as experiencing barriers. Her subsequent analysis was rooted in the data, and the respondents influenced the research process. Importantly and unexpectedly, her participants gave more personal reasons for their reentry, and more than one-half of the women reported serious incidents or traumatic experiences as linked to their return to education. Parr (1998) reported that listening closely and paying attention to the women's nonverbal behaviors helped her to hear what they were telling her about the links between trauma and education "once she allowed the women's voices to be heard" (p. 100).

Narratives as Research

The use of narratives as research is compatible with a postmodern or social constructionist perspective. Narratives are the stories people tell about their lives. Narrative research focuses on the ways in which individuals choose to tell their stories, in relation to the frameworks or master narratives

provided by the culture for organizing and describing life experiences (Sarbin, 1986). Master narratives refer to the cultural frameworks that limit and structure the way that stories are told in order to support the status quo and the dominant groups' perspective on reality. Gergen (2010) described her analysis of how women's narratives differed from the cultural heroic myths of male narratives; she argued that women's narratives were more embodied, and that in women's narratives, love and achievement themes were interwoven. Story telling can be used, however, to disrupt or challenge accepted perceptions and master narratives. Stories are used to communicate experience, but they can also articulate ideology and can move people to action (Romero & Stewart, 1999).

A narrative approach can be employed to further feminist goals. Narratives have been discussed as an innovative feminist method (Gergen, Chrisler, & LoCicero, 1999) designed to reveal cultural constructions. Recognizing, resisting, or deconstructing the master narratives that have been used to restrict or limit the experiences of women is one feminist form of narrative research (Romero & Stewart, 1999). Other examples of feminist narrative research are presented in Franz and Stewart's (1994) edited volume of narratives, in which they explore the way in which narratives "create" a psychology of women. Thus, storytelling can lead to "ideological transformations and to political mobilization" (Romero & Stewart, 1999, p. xii). Storytelling is seen as a way of including women's experience, of breaking the silence of women, and as a way of giving women a voice for the expression and analysis of their own experiences (Romero & Stewart, 1999). They argue that social transformative work is done through the telling of previously untold stories and through women's naming and analyzing their own experience (Romero & Stewart, 1999).

Narrative research reveals our desire to provide a unified and coherent story and to gloss over or ignore paradoxes, inconsistencies, and contradictions in women's lives (Cabello, 1999; Franz & Stewart, 1994). The challenge for feminist researchers is to find methods for including and representing dualities and contradictions present in women's lives (Cabello, 1999). Cabello (1999) describes the methodological challenge of including the incoherence and contradictions in narrative research. She also discusses the tensions between the researcher's interpretation and the subject's active participation in the telling and interpretation of her life story.

Discourse Analysis

The main goal of discourse analysis is to investigate how meanings are produced within narrative accounts (e.g., in conversations, newspapers, or interviews). Thus, the label discourse analysis does not describe a technique or a formula, but rather it describes a set of approaches that can be used when researchers work with texts (Cosgrove & McHugh, 2008). Researchers who use a discourse analytic approach emphasize the constitutive function of language, and they address the ways in which power relations are reproduced in narrative accounts (McHugh & Cosgrove, 2004). A discourse analytic approach is grounded in the belief that meaning and knowledge are created by discourse; discourse analysts view language/discourse as constituting our experience. Based on the belief that all forms of discourse serve a function and have particular effects, and the research focus is on "how talk is constructed and what it achieves" (Potter & Wetherell, 1996, p. 164). The researcher cannot, simply by virtue of switching from a quantitative to a qualitative approach, uncover an experience or identity that exists prior to and distinct from human interaction. There are no true, real, or inner experiences or identities that somehow reside underneath the words a woman uses to describe that experience or identity. The paradigm shift from analyzing interview data to analyzing discourse involves a different perspective on the goals of research and what we can know (Cosgrove & McHugh, 2002). It encourages us to examine the practices, technologies, and ideologies that allow for the experiences that we are investigating. This shift may help us focus on structural rather than individual change strategies.

In the conduct of discourse analysis, the researcher is explicitly interested in the sociopolitical context that creates particular discourses and discourages other constructions and linguistic practices (Wilkinson & Kitinger, 1995). The implications of this epistemological shift for developing alternative methodologies can be seen in how interview-based data would be approached and analyzed. The researcher does not assume that she will discover some underlying truth about women's essential nature or personality. Instead, the researcher is interested in identifying dominant and marginalized discourses and in addressing how women position themselves in the available discourses. As previously noted, rather than denying or trying to overcome the inconsistencies, contradictions, or ambivalence in women's accounts of their experience, the researcher pursues these

contradictions. This allows for a better understanding of how women might position themselves otherwise (Burr, 1995; Hollway, 1989; Kitzinger, 1995; Potter & Wetherell, 1996). This social constructionist approach moves the researcher from the analysis of narratives as *revealing* inner subjectivity (i.e., of a woman's story as revealing who she is) to an analysis of discourse as *constituting* subjectivity. Thus, the question shifts from "what does this account reveal about women's underlying or true nature?" to "what does this account reveal about the dominant discourses to which women have been subjected?" and "what does this account reveal about discourses which have become marginalized?" The analysis of data is then carried out with a focus on the questions "when and how do women resist dominant discourses when those discourses cause them distress, and how might we allow for greater opportunities to position ourselves in alternative discourses?"

The implications for feminist research are dramatic and complex. If there is no method to "get to the bottom of things," what does it mean to create a space for women to speak for themselves? A researcher using discourse analysis would understand meaning to be produced rather than revealed. An account of an individual's experience is always located in a complex network of power relations (Cosgrove & McHugh, 2008). Thus, in analyzing women's accounts, a social constructionist approach applies an analysis of power. The interview, and analysis, is not about discovering "truths" but about identifying dominant and marginalized discourses. The analysis examines the degree and the ways in which individuals resist oppressive discourses. For example, a psychologist interested in the experience of motherhood would first recognize that the discourses of motherhood shape and confine one's understanding of oneself as a mother and as not a mother (Letherby, 2003). The analysis of the data on the experience of being a mother would be contextualized in terms of how discourses produce certain identities (e.g., "supermom," mother as the primary care-giver, etc.) while marginalizing others (Cosgrove, 1999).

Focus Groups

Wilkinson (1998) argues for the use of focus groups as a feminist method in that focus groups can meet the feminist goals of examining women's behavior in naturalistic social contexts and in a way that shifts the power from the researcher to the participants. A focus group might be described as

an informal discussion among a group of people, which is focused on a specific topic and is either observed or taped by the researcher (Morgan & Krueger, 1993). Focus groups are typically facilitated by a trained moderator who fosters a comfortable environment. Kitzinger (1994) suggests that focus group interviews might be used as an effective method when gaining information from participants is difficult; that is, when people feel disenfranchised, unsafe, or reluctant to participate. Focus groups may be useful in mining subjugated knowledge or in giving a voice to members of marginalized groups or empowering clients (Leavy, 2007; Morgan, 2004). Focus groups have been used to bridge a gap in perspective between the researcher and the informants (Morgan, 2004). The communication in focus groups may be dynamic and create a sense of a "happening" (Leavy, 2007). In successful focus groups, participants express or share some of their experiences with others using their own language and frameworks (Leavy, 2007).

Focus groups avoid the artificiality of many psychological methods. Focus groups mimic the everyday experience of talking with friends, family, and others in our social networks. The focus group itself may be seen as a social context and, at the same time, as a parallel to the social context in which people typically operate. The group-based approach of nondirective interviewing allows the participants to identify, discuss, disagree about, and contextualize issues of importance to them (Hennink, 2008). At times, the focus group may reveal the extent of consensus and diversity of opinion within groups (Morgan, 2004). The group environment can provide rich data regarding complex behaviors and human interactions.

People establish and maintain relationships, engage in activities, and make decisions through daily interactions with other people. Focus groups may use these preexisting or naturally occurring groups, or may set up groups of people who do not know each other (Wilkinson, 1998). For example, Press (1991) studied female friends talking about abortion by having them meet in one woman's home to view and discuss an episode of a popular television show. The focus group can thus avoid artificiality by making naturalistic observations of the process of communication in everyday social interaction (Wilkinson, 1998; 1999). More importantly, the focus group provides the opportunity to observe how people form opinions, influence each other, and generate meaning in the context of discussion with others

(Wilkinson, 1998; 1999). For feminists who see the self as relational or identity as constructed (e.g., Kitzinger, 1994), the focus group can be an ideal method. In focus groups, the influence of the researcher is minimized as women in the group speak for themselves and voice their own concerns and themes. Focus groups may also provide an opportunity to access the views of individuals who have been underrepresented in traditional methods (Wilkinson, 1998). Focus groups may lead to consciousness raising or to the articulation of solutions to women's problems (Wilkinson, 1998; 1999). Focus groups may be a component of participatory action projects (Morgan, 2004). The increased use of focus groups by social scientists over the past two decades argues for their usefulness as a qualitative method (Morgan, 2004).

Feminist Phenomenological Approaches

A phenomenological approach emphasizes a (paradigm) shift from observed behaviors to the importance of an individual's lived experience as the proper subject matter for psychology. Phenomenology is committed to the articulation of individuals' experience as description and does not subscribe to hypothesis testing. Husserl (1970) argued that psychologists should use descriptive methods to try and capture the meaning of individuals' experience; he emphasized the need for social scientists to investigate the personal, the life-world to capture the experiential nature of human experience. Criticizing psychology (and other social sciences) for its adherence to positivist methods, he challenged the subjective/objective distinction. (Cosgrove & McHugh, 2008). Thus, a phenomenological approach is not just another method that might be employed by a feminist researcher, but an alternative approach to knowledge (Cosgrove & McHugh, 2008). Phenomenological research uses a descriptive method that attempts to capture the experiential meaning of human experience (Nelson, 1989). Phenomenologically informed researchers do not test hypotheses but generate theory from the data (i.e., individuals' experiences). This approach does not distinguish between objective and subjective methods but does privilege description over measurement and quantification (Cosgrove & McHugh, 2008). The phenomenological researcher does not subscribe to the goal of uncovering or discovering truths about the participants' experience but has a commitment to articulating the lived

experience of the participants and analyzing the sociopolitical context in which the experience occurs (Cosgrove & McHugh, 2008). For example, a research team could investigate the lived experience of being "at home." The descriptive differences in men and women's lived experience might be described without essentializing or reifying gender.

According to Cosgrove and McHugh (2008), phenomenology shares the feminist commitment to creating a space to hear (women's) stories. In phenomenologically grounded research, the researcher may examine the ways in which gender (along with race, class, and culture) plays a key role in shaping women's experiences. Phenomenologists also share the feminist commitment to test theory against experience. Both feminists and phenomenologists recognize the limits of laboratory-based research, emphasize the importance of listening to individuals' experiences, and appreciate the possibilities of a descriptive science (Nelson, 1989). Cosgrove and McHugh (2008) suggest that some feminists would agree with the phenomenological perspective that relying, epistemologically and methodologically, on quantification and measurement to the exclusion of life-world description is a limited approach that produces alienated rather than emancipatory knowledge.

Both feminists and phenomenologists view research as an interaction or dialogue between the researcher and the participant (Garko, 1999). The phenomenological approach emphasizes connections among self, world, and others and allows the researcher to hear women's experiences as contextualized within the larger social order. Consistent with feminist research, a phenomenological perspective demands that we hear, describe, and try to articulate the meaning of women's experiences, including stories that have been marginalized and/or silenced (Cosgrove & McHugh, 2008).

Participatory Action Research

"Participatory research offers a way to openly demonstrate solidarity with oppressed and disempowered people through our work as researchers" (Maguire, 2008, p. 417). Maguire (1987; 2008) described participatory action research as involving investigation, education, and action. By involving ordinary people in the process of posing problems and solving them, participatory research can create solidarity and social action designed to radically change social reality, as opposed to other methods that describe or interpret reality (Maguire,

2008). Goals of feminist research, including self-determination, emancipation, and personal and social transformation, are approached by working with oppressed people, not studying them (Maguire, 2008). When working with a community group to address a problem they define, the traditional distinctions between knower and participant and between knowledge and action are dissolved (Hall, 1979).

In contrast to the traditional valuation of theoretical and pure science over applied science, participatory action research challenges the dichotomous view of applied versus theoretical research. In action research, theory is political and action has theoretical implications (Hoshmand & O'Byrne, 1996; Reinharz, 1992). Hoshmand and O'Byrne (1996) view action research as consistent with postmodern and post-positivist revisions of science; action research takes an explicitly contextual focus and thus action researchers may be less likely to commit the "errors" of essentialism and universalism (Cosgrove & McHugh, 2002). Participatory research is built on the (feminist) critique of positivist science, and the androcentrism of much of traditional social science research (Maguire, 2008) and the emancipatory impact of participatory research is dependent on feminist analysis. Researchers should explicitly consider gender and patriarchy as important components of the project (Maguire, 1987). A challenge for feminist researchers is to consider the operation of class, ethnicity, sexual orientation, and other dimensions of oppression in the research agenda.

In addition to improving the lives of the participants, education and the development of critical consciousness is a component of participatory action research (Maguire, 2008). The research process can assist the community members to develop skills in information gathering and use and in analysis. Perhaps more significantly, the community members may develop a critical understanding of social problems and underlying causes and possible ways to overcome them. By having ordinary people participate in the research, affirming and extending their knowledge about their own lives, participatory action research exposes and helps to dismantle the industry of knowledge production. Knowledge production and traditional research exclude ordinary people from meaningful participation in knowledge creation, intimidate marginalized groups through academic degrees and jargon, and dehumanize people as objects of research (Maguire, 2008).

In this spirit of research designed to create critical consciousness (of the sexual double standard),

McHugh and her students facilitated discussions in class and in focus groups of undergraduate students about their experience and observation of slut bashing and the walk of shame (McHugh, Sciarillo, Pearlson, & Watson, 2011; Sullivan & McHugh, 2009). Students shared their understanding and experience of who gets called a slut and why. In the discussion, many students recognized the operation of the sexual double standard and developed some understanding of how this impacted their own and other women's expression of sexuality. This "research" emphasizes the students as experts on this topic, helps students develop critical consciousness, and documents the existence of the sexual double standard as common social practice, in contrast to quantitative research that does not confirm the existence of the sexual double standard (Crawford & Popp, 2003).

In most social action research, the researchers design the research project to empower the individuals and communities with whom they work (Cosgrove & McHugh, 2008). In participatory research, the shared agenda is set by the community; traditional research is based on the researcher's agenda. The engagement and solidarity with participants is an important feature of participatory research, in contrast to the traditional objectivity and disengagement of the experimenter. For example, in contrast to traditional research (e.g., why battered women stay), Maguire (1987) reported on her participatory research with a group of battered women in Gallup, New Mexico. Maguire talked with former battered women in their kitchens, employing Freire's (1970) concept of dialogue. The researcher and participants moved through a cycle of reflection and action; Maguire presented the women (in their own words) as they searched for how to move forward after living with violent men. These results are in contrast to the psychologizing and victim-blaming approaches often taken in research with women who experience intimate partner violence (McHugh, 1993; McHugh, Livingston, & Frieze, 2008). Fine (1992) also identified the victim-blaming interpretations made by researchers. In a critical examination of articles published in *The Psychology of Women Quarterly*, Fine documented that authors "psychologized the structural forces that construct women's lives by offering internal explanations for social conditions, and through the promotion of individualistic change strategies, authors invited women to alter some aspect of self in order to transform social arrangements" (p. 6).

A variety of qualitative methods were described here with an emphasis on why and how each method might be used by feminist researchers. For each of the methods, feminist researchers with differing epistemological positions are likely to share certain concerns regarding the research: “attention to women’s voices, differences between and within groups of women, women’s contextual and concrete experiences, and researcher positionality” (Leckenby & Hess-Biber, 2007, p. 279). As feminist researchers, we might mine each approach for its liberatory potential.

Innovations in Feminist Research

Intersectionality

Feminist analytic strategies have been used to challenge biological reductionism, demonstrating how race and gender hierarchies are produced and maintained (Hawkesworth, 2006, p. 207). Increasingly, feminists have realized that individuals’ experiences are influenced by both race and gender and by the intersection of various identities (intersectionality). Intersectionality is an innovative approach that applies an analytic lens to research on gender, racial, ethnic, class, age, sexual orientation, and other dimensions of disparities (Dill & Zambrana, 2009). The approach of intersectionality analyzes the intersections of oppressions, recognizing that race, sexual orientation, social class, and other oppressed identities are socially constructed. Intersectionality challenges traditional approaches to the study of inequality that isolated each factor of oppression (e.g., race) and treated it as independent of other forms of oppression (Dill & Zambrana 2009). Interpersonal interactions and institutional practices can create marginalization and subsequently constrain women of color and women marginalized by other identities. In response to such recognition, feminist scholars of color have coined the term “intersectionality” to refer to the complex interplay of social forces that produce particular women and men as members of particular classes, races, ethnicities, and nationalities (Crenshaw, 1989). McCall (2005) has referred to intersectionality as the most important contribution of women’s studies; intersectionality challenges the dominant perspectives within multiple disciplines including psychology. Intersectionality recognizes the interrelatedness of racialization and gendering. The term “racing-gendering” highlights the interactions of racialization and gendering in the production of difference (Hawkesworth, 2006). The identities of women of color result from an

amalgam of practices that construct them as Other. Such practices include silencing, excluding, marginalizing, stereotyping, and patronizing.

For example, in a study of congresswomen (103rd Congress), Hawkesworth (2006) found the narratives of congresswomen of color to be markedly different from the interview responses of white congresswomen. African-American congresswomen, especially, related experiences of insults, humiliation, frustration, and anger. Hawkesworth (2006) provides a series of examples to demonstrate that Congress was/is a race-gendered institution, that race-specific constructions of acting as a man and a woman are intertwined in daily interactions in that setting. She further relates the experiences of invisibility and subordination of black congresswomen to congressional action on welfare reform and concludes that the data indicate ongoing race-gendering in the institutional practices of Congress and in the interpersonal interactions among members of Congress.

Developing Consciousness

Consciousness raising (CR) was an important method of the second wave of feminism in the United States (Chrisler & McHugh, 2011). Through group discussions, women recognized commonalities in their experiences that they had previously believed to be personal problems (Brodsky, 1973). Such discussions had the potential to reveal aspects of sexism and patriarchy and led to the realization that the personal is political; that is, that the power imbalance between women and men and the way that society was structured along gender lines contributed to women’s experiences of distress (Hanish, 1970). Undertaken as political action, CR groups were later facilitated by psychologists and became a model for therapeutic women’s groups (Brodsky, 1973). Consciousness raising groups are a form of participatory action research. Consciousness raising is a method for understanding and experiencing women’s experiences, and for understanding and resisting patriarchy. Consciousness-raising is an important contribution of feminism.

DOUBLE CONSCIOUSNESS

In an elaboration of consciousness raising, some theorists have discussed women’s double consciousness in relation to feminist standpoint theory. In one version of double consciousness, women, as a result of their subordinated position, have an awareness of their own daily lives and work (which are invisible to members of the dominant group), but they

also have an understanding of the lives of the dominant group (Nielsen, 1989). Or, women scientists, by participating in science and yet experiencing the subordinated position of women, have a unique perspective as both an insider and an “other,” to examine the operation of sexist bias in science (Rosser, 2008). Most frequently, double consciousness refers to the position of black feminist theorists that black women hold a unique position that allows them to understand the operation of both sexism and racism (Collins, 1990; 2008). Collins argues that such consciousness, based on lived experience, involves both knowledge and wisdom and that such consciousness is essential to black women’s survival. Black women share their truth by way of storytelling or narrative, and the black community values their stories. The consciousness of black women is thus forged in connection with community. Collins (2008) suggests “the significance of a Black feminist epistemology may lie in its ability to enrich our understanding of how subordinate groups create knowledge that fosters both empowerment and social justice” (Collins, 2008, p. 256).

In an elaboration of double consciousness, feminist standpoint approaches have developed into a method, as well as an epistemological position (Hawkesworth, 2006; Sandoval, 2000). Feminist standpoint as a method begins with the “collection and interrogation of competing claims about a single phenomenon” (Hawkesworth, 2006, p. 178). The method involves the contrast and analysis of competing situated (theoretical and value-laden) claims to understand the role theoretical presuppositions play in cognition. The feminist standpoint analysis may suggest ways to resolve seemingly intractable conflicts (Hawkesworth, 2006). Hawkesworth (2006) illustrates the method with an analysis of multiple feminist positions on Affirmative Action.

OPPOSITIONAL CONSCIOUSNESS

Authors and theorists from varied backgrounds and geographies have described and theorized a form of consciousness referred to as “oppositional consciousness.” The recognition and development of “oppositional consciousness” is considered both a social movement and a method (Sandoval, 2000). As a method, cultural theorists aim to specify and reinforce particular forms of resistance to the dominant social hierarchy. “The methodology of the oppressed is a set of processes, procedures and technologies for de-colonizing the imagination” (Sandoval, 2000, p. 68). The theory and method of oppositional consciousness is a consciousness developed within

women of color feminism (Sandoval, 2000, p. 180), where it has been employed as a methodology of the oppressed. The methodology of oppositional consciousness, as theorized by a racially diverse (US) coalition of women of color, demonstrates the procedures for achieving affinity and alliance across difference (Sandoval, 2000). Through a series of dialogues, processes, meaning-making, deconstructions, and consciousness, people in search of emancipation from oppression voice, interrogate, and theorize their experiences, recognize (resist) ideologies and practices of oppression, and transcend differences to achieve an alliance, a coalition of consciousness that opposes oppression and transcends difference (Sandoval, 2000).

TRANS/FEMINIST METHODOLOGY

In a related approach, Pryse (2000) argued that the interdisciplinarity of women’s studies can contribute to the development of a “trans/feminist methodology.” Pryse (2000) contends that there is a special opportunity in the study of women’s studies scholars; faculty and students from diverse disciplinary backgrounds collaborate over questions regarding gender and its interconnections with race, ethnicity, class, sexuality, ability, and culture. Envisioning a hybrid or “trans” methodology is the challenge of interdisciplinary collaborations (Friedman, 1998; Pryse, 2000). She examines interdisciplinarity as involving intellectual flexibility and engagement in cross-cultural analyses, both of which can be conducive to cross-cultural insight and may enhance receptivity to difference. Pryse is hopeful that the work of interdisciplinary teams can develop the transversal political perspective described by Yuval-Davis (1997). *Transversal political perspectives* are contrasted with identity politics in which women from different classes, regions, nations, races, or ethnicities recognize and emphasize the differences in their material and political realities. In a transversal political perspective, women could “enter into a dialogue concerning their material and political realities without being required to assert their collective identity politics in such a way that they cannot move outside their ideological positioning” (Pryse, 2000, p. 106). Yuval-Davis (1997) described interactions of Palestinian and Israeli women who engaged in a dialogue that could be indicative of transversalism. Each member of the interaction remained rooted in her own identity, but shifted to a position that allowed an exchange with a woman with another identity. This dialogue, labeled *transversalism* was contrasted with universalism.

In transversalism, a bridge that can cross borders or differences is constructed, whereas universalism assumes homogeneity among women. In her vision, Pryse sees transversalism as a methodology that can allow feminist researchers to construct questions that emerge from women's lives without committing the error of universalizing women and by remaining specific about the differences among women. Furthermore, the transversal approach can help researchers transcend disciplinary boundaries and methods. A transversal approach is consistent with a postmodern perspective in that multiple realities and partial truths are recognized and essentialism is avoided (Pryse, 2000). The transversal viewpoint allows both difference and similarity to be simultaneously recognized and appreciated as we study women's lives. This can be seen as a form of dialectic thinking, as opposed to the traditional tendency to engage in dichotomous thinking.

DIALECTIC THINKING

In a similar approach, Kimball argued that "The major goal of practicing double visions is to resist the choice of either similarities or differences as more true or politically valid than the other" (Kimball, 1995, p. 12). Kimball (1995) called for a rejection of simplistic dichotomous thinking (about gender) and for the practice of double visions with regard to feminist theory and research on gender. Kimball's reference to double visions originates in the postmodern position that we can only have partial knowledge and that partial knowledge is, by definition, not fully accurate. Accordingly, Kimball is suggesting that we are not forced to choose between one piece of partial knowledge and another. Thus, we do not have to choose between evidence that women are caring and evidence that women are aggressive. One might choose a particular position in a certain context or prefer a given perspective on gender, but, as Kimball has noted, practicing double visions means that neither alternative is foreclosed; feminist psychologists would recognize the partiality of any perspective and respect theoretical diversity. This means that we should actively resist making a choice and instead maintain a tension between/among the alternative positions. The way forward for feminist research, according to Tuana (1992), is to avoid dichotomous thinking and either/or choices. In terms of the sex/gender difference debate, this could mean that we recognize that men and women are both alike and different or are alike in some settings and different in others (McHugh & Cosgrove, 2002).

Double visions, or a dialectic approach to sex/gender, describes the movement between or among positions as a sophisticated and theoretically grounded practice. Previously, the perspective of individuals who vacillated between denying gender differences and focusing on the common experiences of women may have been labeled as contradictory, inconsistent, incoherent, or confused. This is similar to the problem of either focusing on the differences among women or examining the common experience of being a woman in a patriarchal society. Privileging the dialectic perspective legitimizes our current confusion, giving us permission to hold contradictory, paradoxical, and fragmented perspectives on gender and women's experiences.

Applying a postmodern or dialectic approach can help to resolve epistemological and theoretical debates. For example, feminists and family researchers have been engaged in an ongoing debate about intimate partner violence as battering (of women by their male partners) or as family violence (equally perpetrated by men and women) (McHugh, Livingston, & Ford, 2005). A postmodern or dialectic approach allows us to recognize how issues of method, sample, and conceptualization have contributed to the debate and to realize that, in a postmodern world, there is not a single truth, but multiple, complex, and fragmented perspectives. Thus, women may contribute to family violence, and battering may be perpetrated mostly by men against female intimates (McHugh et al., 2005).

Ferguson (1991) and Haraway (1985) recommend irony as a way to resolve the dichotomous tensions created by two (seemingly opposing) projects or perspectives. In irony, laughter dissuades us from premature closure and exposes both the truth and the non-truth of each perspective. Ferguson (1991) describes irony as "a way to keep oneself within a situation that resists resolution in order to act politically without pretending that resolution has come" (p. 338). Similarly, Cosgrove and McHugh (2008) have encouraged the use of satire to expose and challenge the limitations of the scientific method; irony and satire can contribute to the transformation of both science and society.

Conclusion

Feminist scholars have taken issue with dominant disciplinary approaches to knowledge production. Feminist researchers have asked a range of questions, examined and adopted varied epistemological positions, and employed diverse methods. While employing varied methods, feminist researchers

share a commitment to promote women's freedom, to examine/expose oppression based on gender (and other subordinated statuses), and to revolt against institutions, practices, and values that subordinate and denigrate women.

Feminists have a long tradition of challenging the theories, methods, and "truths" that traditional social scientists believe to be real, objective, and value-free. Feminists have posed a serious challenge to the alleged value neutrality of positivistic social science. In an attempt to transform social science, feminists have developed innovative ideas, methods, and critiques, some of which were reviewed here. Classic and emergent qualitative methods have been deployed in a variety of contexts as feminist researchers critique traditional methods and assumptions and struggle to conduct research that empowers women or improves their lives. The current chapter represents an attempt to help researchers understand the methodological and epistemological underpinning of feminist research, to reflect on their own choice of methods, and to practice feminist research by engaging in a nonhierarchical and collaborative process that leads to an understanding of some aspect of women's lives and contributes to the transformation of society. Hesse-Biber and Leavy (2007) have provided a guide to feminist research practice. In conclusion to their guide, Hesse-Biber (2007) characterized the research process as a "journey... where the personal and the political merge and multiple truths are discovered and voiced where there had been silence" (p. 348).

One possibility for the future is that increasing numbers of researchers will be exposed to the feminist critique of science and will contribute to the transformation of research by developing a postmodern or dialectical approach to research. According to a postmodern approach, the transformation of society begins with a transformation of our understanding of how and what we can know. Traditional approaches to knowledge constructed, confirmed, and constrained our understanding of gender and our ideas of what is possible. The postmodern position provides a powerful epistemological position for deconstructing rather than regulating gender (Cosgrove, 2003). Thus, the transformation of science and research is an initial step toward the feminist transformation of gender and the dismantling of male dominance. Larner (1999) viewed the postmodern perspective as encouraging us to "think the unthought and ask questions unasked."

However, changing the practices of science and social science so that we can better attend to issues

of social injustice is neither an easy nor straightforward task. Hesse-Biber and Leavy (2008) note that quantitative methods continue to be privileged over qualitative in a variety of ways. In my own experience, despite the varied epistemological perspectives and the array of methodological approaches available, the majority of research reported in journals and textbooks continues to employ empirical and quantitative methods. When qualitative methods are employed, they tend to be the established classic approaches, like open-ended survey interview questions that are thematically coded. Furthermore, in a systematic review of the top undergraduate research methods texts of 2009, I observed that qualitative methods were not substantially described or discussed in most texts, and feminist critiques or research were not mentioned (Eagly, Eaton, & McHugh, 2011). Hesse-Biber and Leavy (2008) cite research and university culture as supporting the status quo and limiting the use of innovative and emergent methods. Funding sources may contribute to conservatism in science, and gatekeepers, such as journal editors, may also limit researchers' willingness to engage in innovative feminist research.

Although she was writing in 1988, Morawski could be talking about today when she suggests that a new (US) conservatism is indicated by recent losses in Affirmative Action, challenges to reproductive rights, and legislation that negatively affects large numbers of American women. She notes that feminist progress is transforming traditional social science but may easily become or remain mired in such a climate. In response to such a societal impasse, Morawski considers some possibilities for feminist deconstruction and reconstruction. She recommends that we continue to be critical and reflective and that we not commit the same errors that we have identified, for example, essentialism. She encourages us to develop a vision of emancipation, to use our imagination, creativity, and irony to overcome our current impasse.

Future Directions

Satire and irony represent one approach to the future of feminist research. "Through the resources of irony, we can think both about how we do feminist theory, and about which notions of reality and truth make our theories possible" (Ferguson, 1991, p. 339). Irony is also recognized by Shotter and Logan (1988) as a requisite for feminist research as it attempts to resist patriarchal thinking and practices even as it produces meaning within the

current patriarchal context. They see the feminist research project as developing new practices while still making use of resources that are part of the old. Shotter and Logan argue for a feminist alternative that would “allow a conversation within which the creative, formative power of talk could be put to use in reformulating, redistributing and redeveloping both people’s knowledge of themselves and their immediate circumstances, and the nature of their practical-historical relations to one another” (p. 82). Moving forward toward an egalitarian community requires a reflection and understanding of our immediate practical relationships to one another, a consideration of “in what voices we allow to speak, and which voices we take seriously” (p. 83).

One form of irony, farce, involves exaggerated versions of a phenomena resulting in both laughter and sometimes a new understanding of the issues involved. Taking an ironic approach can lead to a richer and more complex picture and necessitates a re-visioning of the epistemological and methodological frameworks that underlie psychological research and feminist theory (Cosgrove & McHugh, 2008; McHugh & Cosgrove, 2002). Although the empirical satiricism described by Cosgrove and McHugh (2008; McHugh & Cosgrove, 2002) is a quantitative method, qualitative methods based on irony and satire can certainly be developed within the participatory action or performative approaches.

(Whereas my younger colleagues may need to limit their research to methods that are acceptable to funding sources and journal editors, I realize that I am not limited by these factors. A decade preretirement, I am in a position to use emergent methods to conduct research that challenges existing ideas regarding women and gender or advocates for marginalized women. I am willing to rethink (again) my epistemological and ontological perspectives, to go beyond my disciplinary boundaries, and to engage in dialectic thinking and irony. Although I may not be successful in jumping publication hurdles, there are alternative methods for distributing or performing transformative knowledge. I hope to conduct participatory and performative research that is ironic, even farcical, to incite new knowledge).

Multidisciplinary collaborations can contribute to the adoption of new perspectives and methods that ignore or transgress boundaries set by traditional disciplines that have served to restrict or constrain our conceptions on how to conduct research. The interdisciplinary practice of women’s studies has contributed to innovations in feminist research practice. Through women’s studies and other

multidisciplinary approaches, feminists from more conservative disciplines can be introduced to post-modern perspectives and other post-postmodern and emerging forms of research. Feminists can contribute to progress by affirming, approving, and applauding the attempts at methodological innovation employed by others.

For example, feminist psychology in the United States has not yet taken the “performative turn,” although feminist researchers from other disciplinary contexts have. Leavy (2008) characterized performance as an interdisciplinary methodological genre used in a variety of fields including sociology, health, and education. Performance can be viewed as a new epistemological stance that disrupts conventional ways of knowing (Gray, 2003). In a performance, individuals act out, and the performance is experienced “in the moment.” Profound theoretical insight can occur to researcher and audience alike when we shift from the representation of reality in written records to the flow of performance. In performance, the actors and the audience help to make or co-create the meaning, and understanding involves an interaction among members of the cast and the audience (Leavy, 2008). Audience members do not need special skills or training to understand or appreciate a performance, and different perspectives on the performance may result in different interpretations or insights. Thus, the knowing that results from a performance is different from the meaning constructed by the researcher in more traditional research. Leavy (2008) points out the relevance of performance to feminist perspectives that emphasize the embodied experience of women (e.g., Bardo, 1989). Leavy (2008) described arts-based methods as a hybrid of arts and science; she characterized performative methods as innovative, dynamic, holistic, creative, as involving reflection and problem solving.

An aspect of the performative turn is the emerging interest in research on the mundane, or the study of the everyday. Contemporary nonrepresentational theory calls us to study the flow of everyday practices in the present rather than constructing post hoc interpretations of past events. Profound theoretical insight and innovations in methods could result if we were to shift from the representation of reality to the flow of performance, if we were to take the mundane or everyday practices of women seriously (Chrisler & McHugh, 2011). This philosophical position builds on the phenomenological approach, an approach Cosgrove and McHugh (2008) have recommended for integration

into feminist methods. This approach is also consistent with the position taken by some feminists that women's ways of being in the world (i.e., as emotional and connected beings) have validity and importance and should not be eliminated in the name of rationality and science.

As early as 1988, Aebischer marveled at the feminist transformation that social science had undergone, when it had become possible to intellectually study "aspects of everyday life and everyday people and to be taken seriously." Even then, she recognized the study of personal experiences, intimate relationships, emotional reactions, and body experiences as a significant transition from one value system to another. Contemporary calls for the exploration of the everyday reveal the extent to which social science in the past had been focused on the unusual, the non-normative, or the pathological. Emphasis on the exceptional, on public domains, on cognition, and on achievements (of men) reflects the androcentric bias of social science. Furthermore, traditional approaches to research such as the experiment, the survey, and systematic observation are not conducive to the study of everyday routines and experiences. Women's everyday experiences such as gossip (McHugh & Hambaugh, 2010), feeling at home (McHugh, 1996), and street harassment (Sullivan, Lord, & McHugh, 2010) have traditionally not been valued as significant topics. In some ways, the current emphasis on the study of everyday lives is a continuation or an extension of an angle of vision adopted primarily within sociology (Scott, 2009). Perhaps what is more innovative is the development of new and emerging methods, including the performative, for the study of affect and the everyday.

The study of the everyday experiences and routines of women is just one example of the directions that future US feminist researchers may take as they shift away from the limitations of logical positivism and, with postmodern permission, strategically adopt multiple ontological, epistemological, and methodological perspectives. Removing the methodological shackles of positivism, modernism, and empiricism, we can exercise epistemological and methodological freedom and move toward feminist research that transforms science and society and liberates women.

(Writing this chapter has been challenging and has caused me to further reflect on myself as a feminist researcher. I have recognized the barriers that have impeded my research in the past decades. Some of these barriers are personal and others

are more about the reception that I have received as a feminist researcher and a postmodern theorist. I have reaffirmed the importance to myself of intrinsic motivation and finding meaning in my work, as opposed to external recognition. Through writing this chapter, I have come to an appreciation of the value of research that I have conducted (for example, on the meaning of home and the positive aspects of gossip) and could continue to conduct that provides partial and situated knowledge and research that adopts an emergent research method. I am inspired to pursue more feminist research and to encourage my students to employ varied and more innovative feminist methods.)

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Critical Approaches to Qualitative Research

Kum-Kum Bhavnani, Peter Chua, and Dana Collins

Abstract

This chapter reflects on critical strategies in qualitative research. It examines the meanings and debates associated with the term “critical,” in particular, contrasting liberal and dialectical notions and practices in relation to social analysis and qualitative research. The chapter also explores how critical social research may be synonymous with critical ethnography in relation to issues of power, positionality, representation, and the production of situated knowledges. It uses Bhavnani’s framework to draw on Dana Collins’ research as a specific case to suggest how the notion of the “critical” relates to ethnographic research practices: ensuring feminist and queer accountability, resisting reinscription, and integrating lived experience.

Key Words: Critical inquiry, dialectical analysis, critical ethnography, politics, reflexivity, accountability and engaged practice

Qualitative research is now ubiquitous and fairly well-respected throughout the human sciences. That Oxford University Press is producing this much-needed volume is further testament to that notion, and one which we applaud. However, although there are different approaches to conducting qualitative research, what is often not addressed are the philosophical notions underlying such research. And that is where the “critical” enters. Indeed, “critical,” used as an adjective and applied, within the academy, to methods of research is also a familiar phrase. The question is, therefore: what does “critical” mean, and how might it be translated such that present and future researchers could draw on some of its fundamentals as they plan their research studies in relation to progressive political activism?

The popularity of critical research is not predictable. Although the 1960s and early 1970s did offer a number of publications that engaged with critical research traditions (e.g., Gouldner, 1970),

and the 1990s also led to a resurgence of interest in this area (e.g., Harvey, 1990; Thomas, 1993), it is now two decades since explicit discussions of critical research have been widely discussed within the social sciences (see Smith, 1999; Madison, 2012, as exceptions).

In this chapter, we first outline meanings associated with “critical.” We then suggest that the narratives of critical ethnography are best suited for an overview chapter such as this. We consider critical ethnography to be virtually synonymous with critical social research as we discuss it in this chapter. In the final section of our chapter, we discuss Dana Collins’ specific research studies to suggest how her approach embraces the notion of “critical” (Collins, 2005; 2007; 2009).

The “Critical” in Critical Approaches

“Critical” is used in many ways. In everyday use, the term can refer, among other definitions, to an assessment that points out flaws and mistakes

(“a critical approach to the design”), or to being close to a crisis (“a critical illness”). On the positive side, it can refer to a close reading (“a critical assessment of Rosa Luxembour’s writings”) or as being essential (“critical for effective educational strategies”). A final definition is that the word can be used to either denote considerable praise (“the playwright’s work was critically acclaimed”) or to indicate a particular turning point (“this is a critical time to vote”). It is this last definition that is closest to our approach as we reflect on “critical” in the context of qualitative research. That is, drawing from the writings of Marx, the Frankfurt School, and others (see Delanty, 2005; Marx, 1845/1976; Strydom, 2011), we suggest that critical approaches to qualitative methods do not signify only a particular way of thinking about the methods we use in our research studies, but that “critical approaches” also signify a turning point in how we think about the conduct of research across the human sciences, including its dialectical relations to the progressive and systematic transformation of social relations and social institutions.

The most straightforward notion of “critical” in this context is that it refers to (at the least) or insists (at its strongest) that research—and all ways by which knowledge is created—is firmly grounded within an understanding of social structures (social inequalities), power relationships (power inequalities), and the agency of human beings (an engagement with the fact that human beings actively think about their worlds). Critical approaches are most frequently associated with Marxist, feminist, and antiracist, indigenous, and Third World perspectives. At its most succinct, therefore, we argue that “critical” in this context refers to issues of epistemology, power, micropolitics, and resistance.

What does this mean, both theoretically and for how we conduct our research? Most would agree that whereas qualitative research does not, by definition, insist on a nonpositivist way of examining the social world, for critical approaches to be truly critical, an antipositivist approach is the sine qua non of critical research. Furthermore, it is evident as we survey critical empirical research that issues of reflexive and subjective techniques in data collection and the researcher’s relationship with research subjects also frame both the practices and the theories associated with research.

The following section begins by drawing attention to developments and debates involving the more restricted use the term critical as related to Marxism

and then explores the ramifications for varying attempts to conduct critical qualitative research.

The Critical Debates

Karl Marx, Friedrich Engels, and their contemporaries (see Engels, 1877/1969; Harvey, 1996; Lenin, 1915/1977; Mao, 1990; Ollman, 2003) developed dialectical materialist notions of critique and “critical” that were substantively different from prior notions. They incorporated these dialectical materialist notions to develop Marxist theories and politics.

Dialectical materialism refers to an outlook on reality that emphasizes the importance of process and change that are inherent to things (such as objects, phenomena, and situations), as well as of the importance of human practices in making change. Significantly, human struggle over existing conditions and contradictions in things creates not only new conditions, but also new contradictions. This outlook serves as an analytical tool over idealist and old-fashioned materialist worldviews and as a source of strength for exploited peoples in their struggle against ruling elites and classes. It emphasizes that correct ideas, knowledge, and theoretical abstractions are established initially, and perhaps inevitably, through practice.

Dialectical materialism may be used to examine two aspects of the research process and the production of academic knowledge. The first aspect involves the writing process as it is carried out among multiple authors. At the drafting phase, the authors craft their distinct ideas into textual form. Contradictions in ideas are bound to exist in the draft. In doing revisions, some contradictions may become intensified and remain unresolved, yet, most frequently (and hopefully!), many are addressed in the form of clearer, more solid, and coherent arguments, thus resolving the earlier contradictions in the text. Yet, new struggles and contradictions emerge. The synthesis of ideas and argument in the final manuscript may again, however, engage in new struggles with the prevailing arguments being discussed.

The second aspect involves the relationship and interaction between the researcher and the interviewee. As their relationship begins, contradictions and differences usually exist between them, for instance, in terms of their prior experiences and knowledge, their material interests in the research project, and their communication skills in being persuasive and forging consent. The struggle of these initial contradictions could result in new conditions and contradictions. For example, this could lead to

(1) the establishment of quality rapport between them, allowing the interview to be completed while the researcher maintains control over the situation;

(2) the abrupt end of the interview due to the interviewee refusing and asserting her or his right to comply with the interview process; or

(3) an explicit set of negotiations that address the unevenness in power relations between them, along with an invitation for both to be part of the research team and to collaborate in the collection and analysis of data and in the forging of new theories and knowledges.

In the first possibility, the prevailing power relations in interviews remain but shift to beneath the surface of the relationship, under the guise of “rapport.” In the second possibility, power relations in the interview process and initial contradictions are heightened, resulting in new conditions and contradictions that the researcher and research participant have to address, jointly and singly. In the third possibility, the research subject is transformed into a researcher as well, and the relationship between the two is transformed into a more active co-learning and co-teaching relationship. Still, new conflicts and contradictions may emerge as the research process continues to unfold.¹ In short, dialectical materialism stresses the analysis of change in the essence (1), practice (2), and struggle (3). Such analyses are at the root of how change may be imagined within the practices of social research.

Dialectical materialism, which forms the basis of the concept of “critical,” emphasizes the need to engage with power, inequality, and social relations in the arenas of the social, political, economic, cultural, and ideological. Based on this status, it is argued that an analysis of societies and ways of life demands a more comprehensive approach, one that does not view society and social institutions merely as a singular unit of analysis but rather as ones that are replete with history. Dialectical materialism directs its criticism against prevailing views or hegemonies, and, within the context of academic endeavors, engages in debates against positivism and neo-Kantian forms of social inquiry. It is this basis of “critical” that defines it in the context of research as a deep questioning of science, objectivity, and rationality. Thus, the meaning of the term “critical,” based on the idea of “critique,” emerges from the practice and application of dialectical materialism.

Historical materialism emerges from and is based on dialectical materialism. That is, any application

of the dialectic to material realities is historical materialism. For example, any study of human society, its history, its development, and its process of change demands a dialectical approach rooted in historical materialism. This involves delving deeper into past and present social phenomena to thereby determine how people change the essence of social phenomena, and, simultaneously, transform their contradictions.

Dialectical materialism regards positivism as a crude and naïve endeavor to seek knowledge and explain phenomena and as one that assumes it is the task of social researchers to determine the laws of social relationships by relying solely on observations (i.e., by assuming there is a primacy of external conditions and actions). In addition, positivism separates the subject (the seemingly unbiased, detached observer) and object (the phenomenon/a under consideration) of study. Dialectical materialism overcomes the shortcomings of positivism by offering a holistic understanding of (a) the essence of phenomena; (b) the processes of internal changes, the handling of contradictions, and the development of knowledge; (c) the unity of the subject and object in the making of correct ideas; and (d) the role of practice and politics in knowledge creation.

Dialectical materialism directs its criticism against dominant standpoints. These standpoints can offer a simplistic form of idealism and philosophical materialism. Within the context of academic endeavors, the methods of dialectical materialism engage in debates against positivism and neo-Kantian forms of social inquiry. This approach challenges assertions that science, objectivity, and rationality are the sine qua non of research and that skepticism and liberalism are the only appropriate analytical positionings by which a research project can be defined as “critical.”

For instance, Auguste Comte and Emile Durkheim, in developing sociological positivism, argued for a new science to study society, one that adopted the methods of the natural sciences, such as skeptical empiricism and the practices of induction. In adopting these methods, approaches relying on early positivism sought to craft knowledge based on seemingly affirmative verification rather than being based on judgmental evaluation and transformative distinctions.

Positivism and dialectical materialism were both developed in response to Kantian and idealist philosophy. In the context of the European Enlightenment, in the late 1700s, Immanuel Kant inaugurated the philosophy of critique. Positivism

challenged Kant's philosophy of critique as the basis for the theory of knowledge.

Kant developed his notion of critique to highlight the workings of human reason and judgment, to illuminate its limitations, and to consolidate its application in order to secure a stable foundation for morality, religion, and metaphysical concerns. Politically, Kantian philosophy provided justification for both a traditionalism derived from earlier periods and a liberalism developed during the ascendancy of the Enlightenment.

Kant sought to settle philosophical disputes between a narrow notion of empiricism (that relies on pure observation, perception, and experience as the basis for knowledge) and a narrow notion of rationalism (that relies on pure reason and concepts as the basis for knowledge). He argued that the essence (termed "thing-in-itself") is unknowable, countering David Hume's skeptical empiricism, and he was convinced that there is no knowledge outside of innate conceptual categories. For Kant, "concepts without perceptions are empty; perceptions without concepts are blind" (1781/1965, pp. A 51/B 75).

The method of dialectical materialism challenges Kant's idealism for (what is claimed to be) its faulty assertion that correct ideas and knowing about the "thing-in-itself" can only emerge from innate conceptual categories, ones that are universal and transcendental. In Kantian philosophy, there is no reality (out there) to be known. Rather, it is the *experience* of reality itself that provides for human reason and consciousness.

Dialectical materialism overcomes Kant's idealism with its recognition of the existence of concrete phenomena, outside and independent of human reason. Dialectical materialism stresses that social reality and concrete phenomena reflect on and determine the content of human consciousness (and also, we would argue, vice versa). Dialectical materialism also emphasizes the role of practice and politics in knowledge development, instead of merely centering the primacy of ideas and the meanings of objects.

In sum, the core debate against positivism centers on the practices of science. Dialectical materialism regards positivist approaches as crude and naïve endeavors that seek to determine unchangeable laws of nature, rely solely on observations and "sense experience" of phenomena as the basis for knowledge, highlight the primacy of external conditions and actions to explain phenomena, and separate the subject from the object of study. That is, dialectical

materialism views positivism as a form of mechanical, as distinct from historical, materialism.

This abridged account of dialectical materialism and the critiques it offers of Kantian idealism and sociological positivism can allow for the formation of a preliminary set of criteria for what may constitute the "critical." We argue that qualitative research may be critical if it makes clear conceptually and analytically:

- The *essence* and *root cause* of any social phenomena (e.g., youth and politics);
- The *relationship* between the essence of the social phenomena under consideration to the general social totality (such as how youth and their views of politics are related to wider systems within society, such as education, age, exploitation);
- The *contradictions* within this social phenomenon (such as how young people are expressing their discontent),

and, therefore,

- How to *conduct* more reflexive practices that interrelate data generation, data analysis, and political engagement that challenge existing relations of power.

Contemporary debates between neo-Kantian idealists and dialectical materialists have often been friendly regarding the direction for carving out what is meant by a critical project in qualitative social research. These debates bring to the fore issues of politics, ethics, research design, and the collection and analysis of data. They have also prompted a variety of ways in which "critical" may be used in relation to qualitative research. For the purposes of this chapter, we suggest four substantial ways in which "critical" is used in the context of qualitative research: (a) critical as a form of liberalism, (b) critical as a counterdisciplinary perspective, (c) critical as an expansion of politics, and (d) critical as a professionalized research endeavor and perspective.

Critical as a form of Kantian liberalism is one of the more conventional uses of the term in qualitative research. This use of critical is generally contrasted against the dogmatism of positivist approaches within social scientific research. Yet, to use critical in this way means that we embrace a liberalism that ends up promoting idealism in outlook and pluralism in practice. That is, Kantian liberalism presents itself as a "critical" and novel analysis by combining eclectic ideas and theories while not making known its political stand and its material interests. As a result, it supports prevailing modes of thinking that emphasize

abstraction over concrete reality, and it succumbs to relativist and pragmatist practices in research, such as “anything goes” in collecting data. In terms of methods, this use of “critical” promotes looseness and leniency in ethics and data collection and analysis, often without a structured accountability to the many constituencies that underlie all social research. Furthermore, the use of, for example, phrases such as “critical spaces,” when applied to social research, may be better understood as a celebration of method above theory and meta-theory and an engagement with some (of the often rather) excessive approaches to reflexivity and meta-reflexivity. In sum, this understanding of “critical” lacks appropriate structures of ethics and accountability and often tends to reject dialectic materialism.

The second use of “critical” in regards to qualitative research proposes a more analytical disagreement with conventional scholarly disciplines and, in so doing, seeks to take up counterdisciplinary positions (Burawoy, 1998; 2003; Carroll, 2004; Smith, 2007). There are two main strands in this use of “critical.” One strand argues that “critical” is a means of exposing the weaknesses of conventional academic disciplines such as anthropology, political science, psychology, and sociology. At the same time, this strand maintains the viability of these core social science disciplines. For instance, academic feminists have continually highlighted the masculinist and heterosexist bias in what is considered top-tier scholarship and the need for these disciplines to be more inclusive in terms of perspectives and methodological techniques (e.g., Fonow & Cook, 1991; Harding, 1991; Ray, 2006). Yet such an approach may not inevitably focus on the fundamental problems, such as a neglect of the study of power inequalities (e.g., Boserup 1970; and see examples in Reinharz & Davidman, 1992). This second strand seeks to carve out interdisciplinary and multidisciplinary fields such as women studies, cultural studies, and area studies to overcome the paradigmatic and fundamental crises within core disciplines (Bhavnani, Foran, & Kurian, 2003; March, 1995; Mohanty, 2003). Many of these interdisciplinary and multidisciplinary fields have often been more historical and qualitative in their approaches, seeking to go beyond positivist limitations and present a more nuanced and thorough analysis. However, even these multi-, inter-, and antidisciplinary fields have an uneven impact on dominant and conventional knowledge.

Moreover, both strands have not been able to overcome the increasing corporatization and

neoliberalization of academic institutions. This issue addresses the increasing restructuring of public education into a private domain, one that relies on privatized practices and funding of both teaching and research. The neoliberalization of the academy is found in the ties of academic research to corporate grants, individualized career advancement, excessive publishing demands and citation indices, and the use of outsourcing for transcription, interviewing, online education, and private research spaces that are “rented” by public institutions, to name a few. These neoliberal conditions of research usually push out those critical researchers who attempt to avoid such exploitative avenues for research, writing, and collaboration. This use of “critical,” however, does expose that critical research is taking shape within contemporary processes of neoliberalism and the increasing privatization of the academy (Giroux, 2009; Greenwood, 2012; Pavlidis, 2012).

The third and less familiar approach is to view “critical” as invigorating politics through the practices of feminist, antiracist, and participatory action research. This approach, for example, highlights the importance of analyzing power in research, as in terms of the conduct of inquiry, in political usefulness, and in affecting relations of power and material relations. Yet this view of “critical” is dogmatic because this approach demands that every research study meet *all* criteria of criticality comprehensively and perfectly.

A final use of “critical” emerges from the many scholarly and professionalized approaches that engage with the politics of academic knowledge construction while making visible the limits of positivism. “Critical” is used here as a means to focus primarily on revitalizing scholarship and research endeavors. However, we argue that even this use of “critical” ossifies the separation of the making of specialized knowledge from an active engagement to transform social life. Such a separation is antithetical to dialectical materialism. Often, this fourth form of the term “critical” is based on the logics of the Frankfurt School of critical theory (such as that of Adorno [1973], Habermas [1985], and Marcuse [1968]) and other Western neo-Marxisms (from Lukacs [1971] and Gramsci [1971] to Negri [1999]). Critical ethnographers and other critical social researchers, drawing from this tradition, often develop public intellectual persona by writing and talking about politics through scholarly and popular forms of publishing and speaking presentations and are even seen to take part in political mobilizations.

Yet they can also shy away from infusing their research with a deep engagement in political processes outside the academy.

Later in this chapter, we discuss how to avoid some of the pitfalls of these four types of “critical,” but suffice it to say, in short, that it is the *politics* and the explicit situatedness of research projects that can permit research to remain “critical.”

Is Critical Ethnography the Same as Critical Research?

George Marcus (1998) argues that the ethnographer is a midwife who, through words, gives birth to what is happening in the lives of the oppressed. Beverley Skeggs (1994) has proposed that ethnography is, in itself, “a theory of the research process,” and Asad (1973) offered the now-classic critique of anthropology as *the* colonial encounter. However, although many approaches to and definitions of ethnography abound, it is the case that they all agree on one aspect: namely, that ethnographies offer an “insider’s” perspective on the social phenomena under consideration. It is often suggested that the best ethnographies, whether defined as critical or not, offer detailed descriptions of how people see, and inhabit, their social worlds and cultures (e.g., Behar, 1993; Ho, 2009; Kondo, 1990; Zinn, 1979).

It is evident from our argument so far that we do not think of ethnographic approaches to knowledge construction as being, in and of themselves, critical. This is because an ethnographic study, although not in opposition to critical ethnography or to critical research in general, has practices rooted in social anthropology. Therefore, its assumptions are often in line with anthropological assumptions (see Harvey [1990] for a recounting of some of these assumptions). Concepts such as “insider” versus “outsider,” “going native,” “gaining access,” and even conceptualizations of a homogenized and/or exoticized “field” that is out there ready to be examined by research remain as significant lenses of methodological conceptualization in much ethnographic research.

Despite, or perhaps because of, the move to reflexivity in ethnographic research, there remain enduring assumptions about best practices. As a result, a certain fetishization of research methods transpires, one that is often epitomized as reflexivity. In this instance, ethnographic and qualitative research become an ideal set of practices for extracting information. In sum, “best research practices,” as ways to extract information, reproduce core power dynamics of racism, gender, class, imperialism,

and heteronormativity, which, in turn, reproduce the oppressive dynamics of noncritical qualitative research.

Furthermore, when presenting research merely as reflexive research, it is the case that the researcher can lose sight of the broader social structural and historical materialist context. In addition, a static notion of reflexivity can lead to the researcher not looking outward to assess the wider interconnections among the micropolitics of the research. That is, reflexivity is a dialectic among the researcher, the research process, and the analysis (Jordan & Yeomans, 1995), but it is often presented simply as a series of apparently unchangeable/essential facets of the researcher. Our final point is that for theory to be critical in the development of research paradigms, it has to explicitly engage with lived experiences and cultures for, without that engagement, it remains as formalism (see, e.g., the work of Guenther [2009] and Kang [2010] as examples of critical qualitative research). We are very much in tune with Hesse-Biber and Leavy, who have suggested that (grounded) theory building is a “dynamic dance routine” in which “there is no one right dance, no set routine to follow. One must be open to discovery” (2006, p. 76).

An example of the limitation of conventionally reflexive research is in the area of lesbian and gay research methods that focus on the experiences of gay men and lesbians conducting qualitative research. It also offers a commentary on the role that non-normative sexuality plays in social research. By looking inward (see the earlier comment on “reflexivity”), these methodological frameworks focus on the researcher’s and participants’ lesbian/gay identifications. In so doing, this can fabricate a shared social structural positionality with research participants who have been labeled “gay” or “lesbian.” Such an approach to reflexivity overlooks the fabricated nature of positionalities and ignores the sometimes more significant divisions between researchers and participants that are expressed along the lines of race, class, gender, and nationality. Reflexivity is used only as a way to forge a connection for the exchange of information. A grave mistake is made in this rush to force similarity along the lines of how people practice non-normative sexualities (Lewin & Leap, 1996; for a more successful engagement with queer intersectionality in research, see Browne & Nash, 2010).

The point to be made is that critical researchers should not merely ask “how does this knowledge engage with social structure?” Critical researchers, when contemplating the question “What is this?”

as they set up and analyze their research, could also ask, “What could this be?” (Carspecken, 1996; Degiuli, 2007; Denzin, 2001; Noblit, Flores, & Murillo, 2004, all cited in Degiuli, 2007). Perhaps, borrowing from Karen O’Reilly’s thoughts on critical ethnography, one may think of critical research as “an approach that is overtly political and critical, exposing inequalities in an effort to effect change” (Reilly, 2009, p. 51). That is, in order for qualitative research to be critical, it must be grounded in the material relationships of history, as may be seen in the work of Carruyo (2011), Chua (2001; 2006; 2007; 2012), Collins (2005; 2007; 2009), Lodhia (2010), and Talcott (2010).

Quantz (1992), in his discussion of critical ethnography, suggests that five aspects are central to the discussion of critical research/ethnography: knowledge, values, society, history, and culture. So far in this chapter, we have discussed *knowledge* and its production, *values/reflexivity* and qualitative research/ethnography, *society* and unequal social relationships, and *history* as a method of historical and dialectical materialism in order to better understand social and institutional structures. What we have not discussed, however, is the notion of culture, nor, indeed, the predicament of culture (Clifford, 1998): “Culture is an ongoing political struggle around the meaning given to actions of people located within unbounded asymmetrical power relations” (Quantz, 1992, p. 483).

Quantz elaborates by stating that culture develops as people struggle together to name their experiences (see Comaroff & Comaroff, 2012, for a sophisticated and elegant discussion of this thinking). For example, one key task of critical research is to tease out how disempowerment is achieved, undermined, or resisted. That is, the job of the researcher is to see how the disempowerment—economic, political, cultural—of subordinated groups manifests itself within culture, and, indeed, whether the subordinated groups even recognize their disempowerment. For example, “the hand that rocks the cradle rules the world” is one example of how the material disempowerment of many groups of women is presented, in fact, as a strength of women, and yet it takes the gaze away from seeing the subordination of women by ostensibly emphasizing women’s hidden social power.

It is critical qualitative research that has to simultaneously analyze how our research can identify processes and expressions of disempowerment and can then lead to a restructuring of these relationships of disempowerment. At times, critical social

researchers engage in long-term projects that involve policy advocacy and community solidarity to link community-driven research with social empowerment and community change (see Bonacich, 1998; Bonacich & Wilson, 2008; Hondagneu-Sotelo, 2007; Stoecker, 2012).

The key point is that critical qualitative research parts company with positivistic approaches because it is argued that positivism is only able to offer a superficial set of findings. Critical qualitative research hones research concepts, practices, and analyses into finer points of reference so that societal relationships may be not only understood, but also so that social power inequalities can be undermined. In short, critical social research has a Foucauldian notion of power at its very core and may thus be thought of as offering insights into people’s lived experiences (Williams, 1976) as they negotiate asymmetrical societal power relations (see e.g., Novelli, 2006).

The Practices of Critical Qualitative Research

Within our current era of enduring global inequalities, what could constitute a truly critical approach to qualitative research? More than twenty years ago, in “Tracing the Contours” (Bhavnani, 1993), it was argued that if all knowledge is historically contingent and, therefore, that the processes of knowledge production are situated, then this must apply to all research practices as well.² This argument was based on Haraway’s (1988) idea that the particularities of knowledge production do not lie in the characteristics of individuals. Rather, knowledge production is “about communities, not about isolated individuals” (p. 590). Building on this, Haraway discussed the significance of partiality and its relationship to objectivity. She suggested that it is the researcher’s knowledge of her own “limited location” that creates objectivity. In other words, knowing the limitations of one’s structural position as a researcher contributes to objective research because there is no objectivity that is omniscient, one from which all can be revealed (Haraway discusses this as the “god trick,” which is like “seeing everything from nowhere,” p. 582).

It is from Haraway’s insights that we develop our argument that situated knowledges are not synonymous with the static reflexivity we describe earlier. This is because, in this latter scenario, the researcher implies that all research knowledge is based on and derives from an individual’s personal historical and biographical perspectives. That is, researchers note

their racial/ethnic identity, sex/gender, sexuality, age, class, and ability (i.e., biographical aspects of themselves), which are presented as essential and unchanging factors and that determine the knowledge created by the research. This has also been called “absolute relativism” (Bhavnani, 1993) or “extreme relativism” (Alcoff & Potter, 1993).

We suggest that the three elements central to research being “critical” are partiality, positionality, and accountability. Partiality leads to critical research interrogating prevailing representations as the research is conducted, and this builds on difference. Positionality is not about being reflexive, but about understanding the sociohistorical/political context from which research is created and thus engages with the micropolitics of a research endeavor. Accountability makes it evident that there are many constituencies to which all academic researchers are accountable—for example, their discipline, intellectual integrity, their institution and academic colleagues, the idea of rigorous scientific research, and academic freedom in research—as well as being accountable to the people with whom the research is being conducted. It is accountability that leads to a critical research project interrogating how the lived experiences and cultures of the research participants are inscribed within the research (see Stoecker, 2012).

What might the necessary elements be for ensuring that our research practices retain the criticality we have discussed earlier? We offer four possibilities that could form a filter through which one could decide if research is critical, using our definition of the term. First, all critical qualitative researchers should interrogate the history of ethnographic research that has led to the systematic domination of the poor; working classes; ethnic, racialized, sexual Others; women; and colonized peoples. That is, critical qualitative researchers must begin research with an understanding of how previous research, including their own, may continue to play a part in the subordination of peoples around the world, for example, by reinscribing them into predictable and stereotypical roles. Second, critical qualitative researchers should work to develop a consciousness of what might constitute critical research practices—without fetishizing methods—that challenge the system of domination often present in social research. Third, researchers who embrace critical qualitative approaches must develop comfort with the notion that they are conducting research with a purpose; that is, researchers grapple with and comprehend that critical research demands that they engage with the idea that they conduct research into

research inequalities in order to undo these inequalities. Finally, critical qualitative researchers comprehend that their level of comfort can extend into the idea that research does not simply capture social realities; rather, the critical research approach is generative of narratives and knowledges. Once this last idea is accepted—namely, that knowledge is created in a research project and not merely captured—it is then a comparatively straightforward task to see the need for a researcher’s accountability for the narratives and knowledges he or she ultimately produces. In so doing, it is possible to recognize that all representations have a life of their own outside of any intentions and that representations can contribute to histories of oppression and subordination.

We propose that it is the actual practice of research, and, perhaps, even the idea of researcher as witness (Fernandes, 2003), and not a notion of “best practices,” that keeps the politics of research at the center of the work we do. This includes insights into the redistribution of power, representation, and knowledge production. We suggest that critical research is work that shifts research away from the production of knowledge for knowledge’s sake and edges or nudges it toward a more transformative vision of social justice (see Burawoy, 1998; Choudry, 2011; D’Souza, 2009; Hussey, 2012; Hunter, Emerald, & Martin, 2013).

Thoughts from the Field

Here, based on Collins’s fieldwork, we highlight a set of critical methodological lessons that became prominent while she was conducting her field research in Malate, in the city of Manila, the Philippines, currently a tourist destination but once famous as a sex district. We define her work as a critical research practice.

Since 1999, Dana Collins has conducted urban ethnographic work in Malate, exploring gay men’s production of urban sexual place. She has been interested in the role of “desire” in urban renewal, and, in particular, how informal sexual laborers (whom she terms “gay hospitality workers,” a nomenclature drawn from their own understandings of their labor and lives) use “desire” to forge their place in a gentrifying district that is also displacing them. This displacement has involved analyzing urban tourism development, city-directed urban renewal, and gay-led gentrification, as well as informal sexual labor.

The research has involved her precarious immersion in an urban sexual field. She undertook participant observation of gay night life in the streets, as well as in private business establishments, and conducted

in-depth and in-field interviews with gay business owners, city officials, conservationists, gay tourists, and gay-identified sexual laborers. In addition, she drew on insights from visual sociology and also completed extensive archival work and oral history interviewing. In all of this, she explored the collective memories of Malate as a freeing urban sexual space.

There exist multiple and shifting positionalities of power, knowledge, exchange, and resistance in her research. For one, she points out that she occupies multiple social locations as a white, lesbian-identified feminist ethnographer from a US university, one who forges complicated relationships with urban sexual space, sex workers, and both gay Filipino men and gay tourists.

A critical research practice at heart involves the shifting of epistemological foundations of social science research by addressing core questions of how we know what we know, how power shapes the practices of research, how we can better integrate research participants and communities as central producers of knowledge in our research, and how we can better conceptualize the relationship between the research we do and the social justice we are working toward in this world.³ Such questions function as a call to action for critical researchers not only to examine the power relations present in research, but to generate new ways of researching that can confront the realities of racism, gender and class oppression, imperialism, and homophobia. This is about not only becoming better researchers, but also about seeking ways to shift the very paradigm of qualitative research and ensuring its service to social change. We have learned to use these questions as a central and ongoing part of the research we do.

Feminist and Queer Accountability to the Micropolitics of the Field

One of the primary tenets of critical qualitative research is that researchers must work with a wider understanding and application of the politics of research. For Kum-Kum Bhavnani (1993), this means that one needs to be accountable to the micropolitics of research because such accountability destabilizes the tendency to conduct and present research from a transcendent position—the “all knowing” ethnographer, the “outsider” going in to understand the point of view of “insiders,” the attempt to (avoid) “go(ing) native,” and the researcher who aims to “gain access” at all costs and in the interests of furthering research. Micropolitics is not only the axis of inequality that shapes contemporary field relations; it is also the historical

materialist relationship that constitutes the field and informs the basis of critical qualitative research. Micropolitics therefore is a critical framework that questions the essentializing and power-laden perceptions of research spaces and people because it encourages both a reflexive inquiry into the limited locations of research, and it involves the more critical practice of the researcher turning outward, to comprehend what Bhavnani calls the “interconnections” among researcher, research participants, and the social structural spaces of “the field.”

Micropolitics illuminates how all research is conducted from the limited locations of gender, race, class, sexual identification, and nationality, as well as illuminating the interconnections among all of these locations. This is not a simplistic reflexive practice of taking a moment in research to account for one’s positionality and then moving on to conduct normative field work; Bhavnani has been critical of such moments of inward inspection that lack substantial accountability to the wider micropolitics of the field. Rather, this move requires an ongoing interrogation of the limited locations of research that show how knowledge is not transcendent. Furthermore, when used reflexively, limited locations offer a more critical framework from which to practice research.

Micropolitics encouraged Collins’ attention to the limited location of a global feminist ethnographer doing research on gay male urban sexual space in Manila. For one, she moved among different positionalities throughout her research—of woman, queer-identified, white, US academic, tourist, *ate* (Tagalog term for older sister)—and none of these positions was either a transcendent or more authentic standpoint from which to conduct ethnographic work. So, for instance, as a white tourist, she moved easily among the gentrifying gay spaces because these spaces were increasingly designed to encourage her movement around Malate. This limited location showed the increasing establishment of white consumer space, which encouraged the movement of consumers like herself yet dissuaded the movement of the informal sexual laborers with whom she was also spending time—the gay hosts. Her limited location as a white woman researcher from a major US university meant that gay hosts sometimes shared their spaces and meanings of urban gay life with her, yet many times those particular spaces and dialogues were closed—she was not allowed into the many public sexual spaces (parks and avenues for cruising and sex late at night), yet gay hosts treated her as an

audience for their many romantic stories about the boyfriends they met in the neighborhood.

Hosts emphasized that they gained much from hosting foreigners in terms of friendship, love, desire, and cultural capital. Yet they monitored the information they shared because she remained to them a US researcher who wielded the power of representation over their lives, despite her closeness with a group of five gay hosts. Hence, gay hosts often chose to remain silent about their difficult memories of sex work or any information that could frame them as one-dimensional “money boys,” as distinct from the “gay”-identified Filipino men who migrated to Malate to take part in a gay urban community.

Micropolitics challenges the authenticity of any one positionality over another; it was Collins’ movement among all of them, as well as her ongoing consideration of their social structural places, that provided her with a more critical orientation to the research. She suggests that she was not essentially a better “positioned” researcher to study “gay” life in Manila because she too is gay. Rather she found that differences of race, class, gender, and nationality tended to serve as more enduring, limited locations that influenced relationships within this research and that required ongoing critical reflexive engagement.

We want to add that a queer micropolitics of the field also offers critical insight into how identities are not stagnant but rather can be fabricated and performative during the research process. This moves researchers away from an essentialist take on their standpoint because an essentialist mind-set can lead to a search for the *authentic* insider and outsider. It can also lead to an essentialist social positionality that is more conducive for researching. Queer micropolitics show that research is made up of a collection of productive relations and identities. So, for example, her lesbian identification did not create a more authentic connection with gay hosts in Manila; rather, she often fabricated a shared “gay” positionality. This was a performance that served as a point of departure for her many conversations, from which she could proceed to share meanings of what it meant to be “gay” in the Manila and the United States.

Some of the productive relations that arise in research are the continuum of intimacies that develop while doing research. So, like feminists before her, she chose to develop close friendships with hosts where they genuinely loved (in a familial way) as they spoke of love. While learning about gay life in Malate, she stroked egos, offered advice, cried over broken hearts and life struggles, and built and maintained familial relations. Queer micropolitics

shows, however, the limitations of such intimacies because intimacy does not equal similarity—the differing social locations of class, race, gender, and nationality meant that the experiences of urban gay life varied immensely. Thus, building such intimacies across these differences requires both the recognition and respect for boundaries that hosts constructed. She had to learn to see and know that when hosts became quiet and pulled away these were acts of self-preservation as well as acts of defiance against the many misrepresentations of their lives that had taken shape in academic research and journalistic renderings of their place in “exotic” sex districts.

A queer micropolitics also shows how research is an embodied practice: researchers are gendered, racialized, classed, and sexualized in the field. This became most apparent as she walked alone at night in the “field” and developed a keen awareness of the deeply gendered aspects of Malate’s urban spaces. For one, her embodiment was a peculiar presence because women in Manila do not walk alone at night. This includes women sex workers who publicly congregate in groups or with clients and escorts; otherwise, they are subject to police harassment. Hence, her very movement in the field as a sole woman felt like a transgression into masculine urban space because her feminine body was treated as “out-of-place” in the public spaces of the streets at night—she was flirted with, name called, followed, and sexually handled as she walked to gay bars for her research. As much as her queer location afforded her an understanding of how gender is a discursive production on the body, replete with the possibility of her being able to transcend and destabilize the gendered body as a biological “reality,” she confronted the discomfort of being read as a real woman in what became predominantly men’s spaces at night.

Yet this gendered embodiment, in part, shaped her knowledge of the district as she developed quick and knowledgeable movement through the streets, a queer micropolitical reading of urban space that arose out of this limited gender location. She was aware of the spacing of blocks, the alleys, the street lighting, and the time of night when crowds spilled out from the bars and onto the streets, allowing her to realize that a socially vibrant street life actually facilitated her movement. This queer micropolitical reading of urban space showed how both researchers and research participants do not simply exist in a neutral way in city space; rather, gender leads to our use and misuse of urban space. She has juxtaposed her experience with those of research participants in her study. The latter spoke at length about their exploratory

and liberatory experiences of urban space, replete with their access to masculine sexual spaces—parks for cruising and sex, city blocks for meeting clients or picking up male sex workers, and alleys, movie theaters, and mall bathrooms for anonymous sex.

This queer micropolitical read of Malate's gentrified space showed how very different was her access to the newly opening bars, restaurants, cafés, and lifestyle stores. Her whiteness signaled assumptions of her class location and positioned her as part of the international presence that this gentrifying space was targeting and whose movement among establishments was encouraged. She received free entry, free drinks, exceptional hospitality, and invitations to private parties, and her movements were closely monitored as she entered and exited establishments for the sake of "protecting a foreign tourist from street harassment" (interview with bar owner).

Overall, she experienced whiteness and class as equally embodied because these locations signaled her power as a "legitimate" consumer, allowing access to urban consumer sites and a privileged movement among gentrified spaces. This embodied experience of gentrified space differed from that of her gay hosts, who were often denied access to these establishments for being Filipino, young, working class, gay, and interested in foreigners. Contrarily, their bodies were constructed as a "threat" to urban renewal in the district.

Resisting Reinscription

Critical qualitative research is also concerned with the politics of representation in research. This requires a hard look at the implicit imperialisms of ethnographic work, including the tendency to go in and get out with abundant factual information, as well as the lasting impact of objectificatory research practices on fields of study. Such practices are evident in the now global rhetoric about the so-called Third World prostitute, who in both academic and journalistic renderings tends to be sensationalized and sexually Othered. This rendering is part of a long history of exoticization that has denied subjectivity and rendered invisible the lived experiences of sexual laborers around the world.

Such failed representations are part of what Kum-Kum Bhavnani (1993) has called "reinscription"—the tendency in research to freeze research participants and sites in time and space, thus rendering them both exotic and silenced. Reinscription denies agency to research participants and renders invisible the dynamic lived experiences of those same research participants. Doing research

in both postcolonial and sexual spaces means that researchers must grapple with how our research participates in histories of reinscription—we both enter into and potentially contribute to a field that has been already "examined," overstudied, and often exoticized. Thus, a critical qualitative approach is one that begins with a thorough understanding of these histories of representation so that we are not entering fields naïvely, as spaces only of exploration. Rather, we enter with knowledge of how the field has already been constituted for us through reinscription. A critical orientation has a core objective of understanding how our representations of research at all levels of the research process could contribute to exoticization by reinscribing participants and sites.

The issue of reinscription became particularly apparent when Dana Collins interviewed gay hosts and grappled with what appeared to be their elaboration of a contradictory picture of their sexual labor, as well as of their lives. In short, hosts tended to "lie," remain silent, embellish "truths," and articulate contradictory allusions to their life and labor in Malate. When Collins began her interviewing, she held the implicit objective of obtaining the "truth" about hosts' lives, which she believed resided in "what they do" in the tourism industry. She was concerned with the "facts" about their lives, even though gay hosts were more likely to express their desire—desire for relations with foreigners, desire to migrate to a "gay" urban district, desire for rewarding work, and desire for community and social change. She struggled with many uncertainties about the discussions: how could they hold a range of "jobs" and attend school, yet spend most of their days and nights in Malate? How could they understand gay tourists as both boyfriends and clients? Why resist the label "sex worker" yet refer to themselves as "working boys" and claim to have "clients?" She struggled to make sense of the meanings that hosts offered even as she simultaneously felt misled concerning the "real" relations of hospitality.

Interviewing hosts about sexualized labor—as a way to produce a representation of sex work—did not facilitate the flow of candid information; hosts later expressed their view that sex work and their lives were already "overstudied." Many researchers had previously descended on Malate to study sex work, and the district was a prime location for the outreach of HIV/AIDS organizations, some of which had breached the confidence of the gay host community. In short, Dana mistakenly started her research without the knowledge of Malate as a

hyperrepresented field, and her research risked reinscribing gay hosts' lives within that field as static and unchanging.

Importantly, those gay hosts who resisted becoming the "good research subjects" who give accurate and bountiful information, prompted a radical shift in her research framework. They told her stories about their imagined social lives, which encouraged her to rethink her commitment to researching sex work because the transformation of the discourses offered another view of the district, their work, and lives, one that offered a more visionary perspective. She began to focus less on "misinformation" and instead followed how hosts framed their lives. She treated these framings as social imaginings in which Malate features prominently in their understandings of gay identity, community, belonging, and change. In short, their social imaginings functioned as counternarratives to reinscription and offered their lived experience of urban gay place. Such imaginations expressed hope, fear, critique, and desire—in short, they present a utopic vision of identity, community, and urban change.

Integrating Lived Experience

Finally, critical qualitative research is a call to study lived experience, which is a messy, contradictory realm, but a deeply important one if we as critical researchers are truly interested in working against a history of research that has silenced those "under study" (see Weis & Fine, 2012). Paying attention to lived experience allows us to better engage with the contradictions mentioned earlier because lived experience is about understanding the meanings that research participants choose to share with researchers, and it is also about respecting their silences. As Kum-Kum Bhavnani (1993) has argued, silences can be as eloquent as words. Finally, integrating lived experience can take a critical qualitative project further because lived experience allows researchers to explore the epistemological relationship of the meanings and imaginings offered by research participants and to be explicit about the project of knowledge production. In other words, a central guiding question of critical qualitative research is how can research participants speak and shape epistemology, rather than solely being spoken about or being the subjects of epistemology?

Collins used hosts' social imaginings as an epistemological contribution because their imaginings showed how hosts draw from experiences of urban gay community to articulate their desires for change, despite their simultaneous experiences of

inequality and exclusion. We read social imaginings as a subjective rendering of urban place—the hosts' social imaginings expressed their history, identity, subversive uses of urban space, and, ultimately, the symbolic reconstitution of that urban space. In this way, hosts were refiguring transnational urban space by writing themselves and their labor back into the district's meaning, even as the global forces of tourism and urban renewal threatened to displace them.

Conclusion

In conclusion, we seek to highlight how critical research insists on the interplay of reflexivity, process, and practice. In particular, we encourage critical researchers to be mindful of the multiple meanings and usages of the term "critical" so that we can make more explicit our political interests and stand within our disciplines, the academy, our community, and the world. We offer dialectical materialism as a distinct mode of critical analysis that emphasizes an analysis of change in essence, practice, and struggle. We also suggest that, for researchers to be critical in their research, they should strive to take up research questions and projects that study change, contradictions, struggle, and practice in order to counter dominant interests and advance the well-being of the world's majority. We should strive to build new research relationships—such as overcoming the faulty divides between researchers and research participants and by promoting systems of community accountability—that dialectically fuse research, political activism, and progressive social change.

Furthermore, we suggest that critical research can agitate against the homogeneity of ethnographic representation, allowing for the realities of people's lives to come into view. Critical researchers recognize the contested fields of research; yet this requires our critical engagement with the research process, as a reflexive, empathetic, collective, self-altering, socially transformative, and embedded exercise in knowledge production. Therefore, critical research can resist imperialist research practices that are disembodied and that assume a singular social positioning. We use an imperative here to say that we must conduct research as embodied subjects who shift between multiple and limited locations. We also have to find more ways to remain accountable to our communities of research as a way to undo implicit imperialisms in social research. Critical research can work against the remnants of an objectivist and truth-seeking method that supports prevailing interests, classes, and groups while embracing research from social locations that offer

situated knowledges and the possibility for greater shared understandings. Finally, critical research can engage the micropolitics of research and foreground the need for the accountability of researchers to resist reproducing epistemic violence.

Notes

1. This last is an idealist imagining of what should happen. However, a number of research projects have approximated closely to these goals.
2. Parts of our argument have appeared in some of our earlier work (e.g., Bhavnani & Talcott, 2011; Collins, 2009; 2002; Chua, 2001).
3. Although we, as the chapter's three authors, do not usually use "we" in our writing as a general pronoun, it is the most direct way to offer our insights in this section.

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Decolonizing Research Practice: Indigenous Methodologies, Aboriginal Methods, and Knowledge/Knowing

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Abstract

Indigenous approaches to research are fundamentally rooted in the traditions and knowledge systems of Indigenous peoples themselves, although Indigenous methodologies and methods have become both systems for generating knowledge and ways of responding to the processes of colonization. Very specific Indigenous methods emerge from language, culture, and worldview. This chapter describes two such Indigenous research approaches drawn from the work of two Indigenous scholars with their communities in Australia and Canada. Although creative and new, these approaches draw deeply from their communities and thus express and enact traditional knowledge systems in contemporary terms. This approach may result in more pertinent research, better take-up and dissemination of research results, and a general improvement in the situations of Indigenous communities and peoples.

Key Words: Indigenous methodologies, decolonization, participatory action research

Indigenous approaches to research are as complex and multiple as Indigenous peoples themselves, but the context for understanding Indigenous methodologies or the closely related topic of decolonizing methodologies necessarily includes the overarching (and in some ways unifying) colonial structures in which peoples find themselves embedded. One of the small ironies of Indigenous methodologies is that the struggle to be defined and understood as Indigenous through specifically Indigenous knowledge production is sometimes most clearly heard by other (i.e., non-Indigenous) scholars as an oppositional rather than self-constituting process. Nonetheless, Indigenous scholars and the communities from which they come understand the expression and practice of distinct Indigenous research methodologies to reflect, enact, and revitalize those Indigenous knowledge systems themselves.

The term itself—“Indigenous”—speaks to what it is not (i.e., colonial/European) as well as to what it contains—the perspectives, histories, and

approaches to research as broadly different and varied as those of Maori, Cree, or Sámi peoples. This is comprehensible, given the spread of capitalism and Western European power over the globe in the sixteenth through twenty-first centuries (see Hardt & Negri, 2000; Wolf, 1982; Worsley, 1984), but can obscure what an Indigenous (or “Indigenist,” see Rigney, 1997) perspective entails, which may have as its source something quite specific, something best considered authentically formed by Indigenous peoples themselves (i.e., autochthonously), rather than derivative of colonialism. This is equally true of the closely related term “Aboriginal,” which also derives some of its content from the colonial experience and Western frames of thought to which it is most often opposed.¹ To understand Indigenous methodologies simply in these terms, however, no matter how well intentioned, is a potentially recolonizing act.

Fundamentally, the ground contested through Indigenous methodology is knowledge itself, and,

for Indigenous people, it is often self-knowledge that is at stake (Moreton-Robinson & Walter, 2009). For many centuries, European knowledge production systems have attended to building images of Indigenous people; Indigenous methodologies are proactive processes through which Indigenous people create their own images and stories. A short story might help show how fundamental the critique of Western knowledge systems can be.

A number of years ago I (ME) went to a large pow-wow at the Toronto Skydome with some friends. About halfway through the event, I went outside the stadium with a young Anishinabe woman to smoke. We were talking and smoking, and, at some point in the conversation, I mentioned that I was studying anthropology (I was doing my PhD at the time). This was a surprise to her, as we only knew each other socially and through circles where anthropologists in particular were greeted with some suspicion. On learning this, she paused for a moment, and then said thoughtfully, “You are the people who think we walked across the Bering Strait.” She was referring to the Bering Strait or Beringia hypothesis, which claims that the Americas were peopled between about 10,000 to 30,000 years ago via a land bridge across the Bering Strait. This is quite a contentious theory among Aboriginal communities (see Ward Churchill’s chapter entitled “Let’s Turn Those Footsteps Around” in the book, *Since Predator Came*, 2005 [1995]). The opposition to the theory is founded partly in alternative belief systems and partly in a deep concern for the amount of intellectual energy that seems to go in to understanding when Aboriginal people arrived in the Americas. The suspicion is that, at root, the core interest in proposing, arguing, and promoting the theory is in recontextualizing all human communities in the New World as immigrants. After another few seconds, she peered at me through the smoke and offered a one-word critique of the Beringia hypothesis—“Whatever” she said, and then we finished up our smokes and went back inside.

That one word—“whatever”—sums up the epistemological positioning of Indigenous methodologies vis-à-vis colonialism. That is, as a system of thought and knowledge production, Indigenous methodologies do not dispute European ones directly, but rather ignore them, and, in practice, create knowledge directly rather than as a result of disputation or opposition. In this way, Indigenous methodologies avoid being entrapped in the power relations inherent in colonial knowledge systems.

Certainly, in colonial systems, knowledge and power are intertwined. Attwood and Arnold (1992) provide one analysis of these systems in their work on

Aboriginalism, work that draws on the much earlier Aristotelian concept of *phronesis*, which Flyvbjerg (2001) describes as prudence or practical wisdom/knowledge, or “true state, reasoned, and capable of action with regard to things that are good or bad for man” (p. 2). *Phronesis* goes beyond the notion that knowledge is about simple facts to consider the role of values and power in judgments and decisions made by a social or political actor. Flyvbjerg argues that phronetic social science focuses on four value-rational questions: (1) where are we going? (2) who gains and who loses, and by which mechanisms of power?, (3) is this development desirable?, and (4) what should we do about it?

Thus, Attwood and Arnold look to Aboriginalism as an intellectual development of constructions of authoritative truths about “Aborigines/Aboriginals,” one characterized by the relationship between power and knowledge. Aboriginalism exists on three levels: the first as Aboriginal Studies through the teaching and scholarly pursuit of knowledge about Aborigines/Aboriginals by non-Indigenous intellectuals who claim Aborigines/Aboriginals cannot represent themselves and therefore must be represented by experts who know more about them than they know about themselves. The second level is based on a style of thought that places emphasis on the imagined distinction between Aborigines/Aboriginals and Europeans in order to construct them as the “Other” and to form a “Them” and “Us” relationship. The third level refers to corporate and government institutions exercising authority over Aborigines/Aboriginals, claiming rights, laws, and information about them. Unfortunately, it is at this point Attwood falls silent and leaves off the Indigenist project of Indigenous people developing, controlling, and determining their own epistemological trajectory. Research can play a key role in empowering Indigenous people to fulfill this role.

There is, then, a sort of knowledge-based empowerment that sits at the very heart of the development of Indigenous methodologies. This is a proactive stance, building on the work of Indigenous critics of Western knowledge systems (most notably Deloria, 1969; 1973; subsequently, see Alfred, 1999; Battiste, 1986; Churchill, 1997; Ermine, 1995), but the germinal work in this regard is Tuhiwira L. Smith’s *Decolonizing Methodologies: Research and Indigenous Peoples* (1999). It is important here to note that Smith’s work is framed in terms of decolonization, in opposition to colonial processes, including those of knowledge producers be they colonial officials, historians, or social scientists. The work also references

a number of proactive responses, and, indeed, in her concluding chapter, Smith provides some very general signposts for how a careful scholar might seek out particular and appropriate Indigenous methods. At one level, Smith's is a moral guide, directing scholars to decolonize their own practices; at a deeper level, it is a primer on where and how such scholars could find suitable Indigenous actors to speak with about whether and how an appropriate research undertaking is possible.

What Smith does not do is frame research methods beyond methodologies. The distinction here between method and methodology is important, but making it runs the risk of descending into the trite. Without claiming too much, we would like to suggest that, for heuristic purposes, method here be understood as a technique for generating data and methodology be conceptualized as a higher order system that affects the selection of methods in any one instance via a set of principles regarding the nature of knowledge and information and the suitable sources from which such information might be derived. There is an epistemological underpinning to methodology that subsequently patterns action in the research space and, thus, knowledge.

Indigenous methodologies and participatory ones are, in this regard, quite similar (see Evans, Hole, Berg, Hutchinson, & Sookraj, 2009) and share a history of struggle. Arising from scholars and communities working in opposition to colonial oppression (Fals Borda, 1987; Friere, 1970) and now adopted by any number of people(s) working from marginalized positions, *participatory action research* (PAR) is used to seek insight from, not simply information about, people and communities in the context of research. For Indigenous communities in particular, such insights may well be derived from deep epistemological roots expressed and reproduced in language and culture. Certainly, within the work of contemporary Indigenous scholars, the concern about Indigenous language and culture is very much tied up with the unique perspectives or worldviews derived from these sources.

Indigenous methods derive from Indigenous perspectives, language, and culture and are thus exactly that—Indigenous; not simply postcolonial or decolonizing, they are epistemologically revitalizing as well. Having now made that claim (i.e., that Indigenous methodologies are, at least potentially, distinct from Western systems of knowledge production), we can move on to a couple of examples that speak to the fundamental goal of Indigenous methodologies—facilitating Indigenous people to develop knowledge

and speak for and of themselves about any and all elements of the worlds they inhabit.

Cyclone: An Australian Aboriginal Approach to Knowledge Production and Dissemination

Tropical cyclones are a seasonal weather condition that Indigenous peoples in Northern Australia have experienced for thousands of years. These meteorological events are firmly embedded in the daily lives of Aboriginal people, and this is reflected in language and cultural practices. The Jirrbal people are the keepers of the cyclone story, and sites of significance are maintained and cared for by descendants. Arising from the epistemology of my Jirrbal language and the long experience of my community (AM) in north Queensland, the cyclone model resonates with people and thus provides a culturally cogent mechanism for both generating and disseminating research.²

Historically, research has not been a positive experience for many Aboriginal and Torres Strait Islander communities; researchers have a responsibility to cause no harm, but traditional forms of research have been a source of distress for Indigenous peoples due to inappropriate methods and practices (Cochran et al., 2008; Miller & Speare, 2012). More recently, PAR has offered a way forward, to make research meaningful for the community and to enable an action research cycle that assists in improving processes for addressing important issues from the communities' perspectives. It has potential to reduce the negative effects that conventional research has had on Indigenous people (Baum, MacDougall, & Smith, 2006).

Importantly, when communities seek control of the research agenda and seek to be active in the research, they are establishing themselves as more powerful agents (Baum et al., 2006). With the increasing use of PAR approaches to address public health and educational issues, there is potential for bridging the gap between research and practice in addressing social issues and creating conditions that facilitate people's control over the determinants of their health (Cargo & Mercer, 2008; Miller & Speare, 2012). Cargo and Mercer (2008, p. 327) suggest that a "key strength of PAR is the integration of researchers theoretical and methodological expertise with nonacademic participants' real world knowledge and experiences into a mutually reinforcing partnership." Partnerships formed with marginalized and vulnerable populations need to ensure that concepts of cultural humility and cultural safety are integrated so that academic and

nonacademic partners are able to establish and maintain mutual respect and trust.

Participatory action research can be a collaborative, participatory, and equal partnership among Indigenous community members, organizations, research assistants, and researchers to examine an issue, gather information about it, analyze the data that come from the process, and then take some action to address that issue. It is driven and owned by the community and the researchers and involves a two-way, respectful conversation that feeds into both the process and the outcomes of this research.

Rigney (1997) promotes the concept of an Indigenist (read Indigenous) methodology that focuses on developing an “anti-colonial cultural critique of Australian history in an attempt to arrive at appropriate strategies to de-colonise epistemologies” (p. 110). Indigenist research is informed by three fundamental and interrelated principles: (1) resistance as the emancipatory imperative in Indigenist research, (2) political integrity in Indigenous research, and (3) privileging Indigenous voices in Indigenist research (p. 118).

I have applied these principles by constructing my research worldview on the following quote in my Jirrbal language:

ηααα ηαμβαιριηου (I think)
ηαλι ηινδα ηαμβαιριηου (You and I are thinking)

I endeavor to pursue research through the understanding that I am a thinking person (*ηααα*

ηαμβαιριηου), a sentiment denied to my recent past relatives and ancestors sanctioned on the basis of contrived social theories like polygenesis and social Darwinism (McConnochie, Hollinsworth, & Pettman, 1988). Such theories were used to label Indigenous peoples as being unable to use their minds and intellect; unable to invent, build, cultivate land, produce items of value, and participate in the arts of civilization (Smith, 1999). Indigenist methodologies counteract this premise by privileging Indigenous voices and intelligence.

In applying Indigenous research principles, it is important to critically look at the past to find answers for the future from Indigenous and non-Indigenous voices. Therefore, I also believe that resolving problems collaboratively (*ηαλι ηινδα ηαμβαιριηου*) is a pathway to understand and address many of the socioeconomic and health problems experienced by Indigenous people.

My research worldview combines both Indigenous research principles and PAR and formalizes it in my own cosmological and cultural framework; a tropical cyclone analogy. Tropical cyclones are significant to Indigenous communities in Northern Australia for not only their destructive power but also for their regenerative and cleansing effects. They are cosmologically and spiritually significant to many Indigenous communities in northern Australia.

The main features of a tropical cyclone are destructive winds and a calm inner eye. I have labeled these features in my language, Jirrbal, in Figure 10.1.

Tropical cyclone analogy

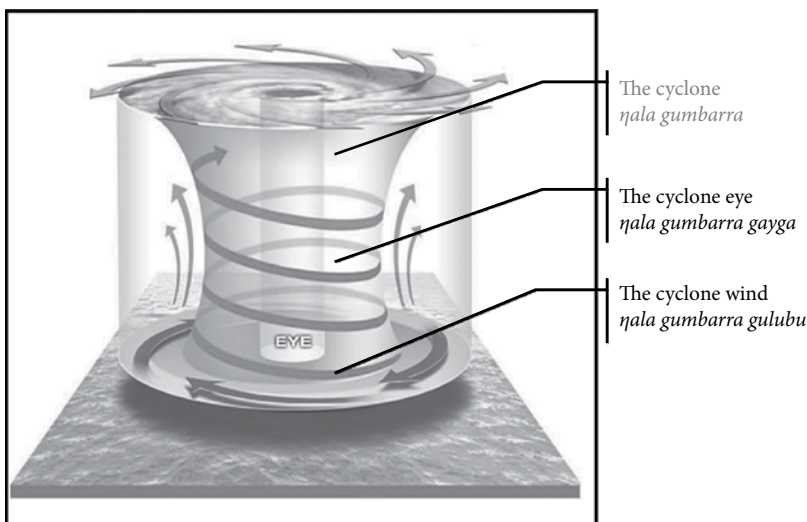


Figure 10.1 The tropical cyclone features destructive winds and a calm inner eye. Accessed from <http://www.ga.gov.au/hazards/cyclone/cyclone-basics/causes.html>

World-view

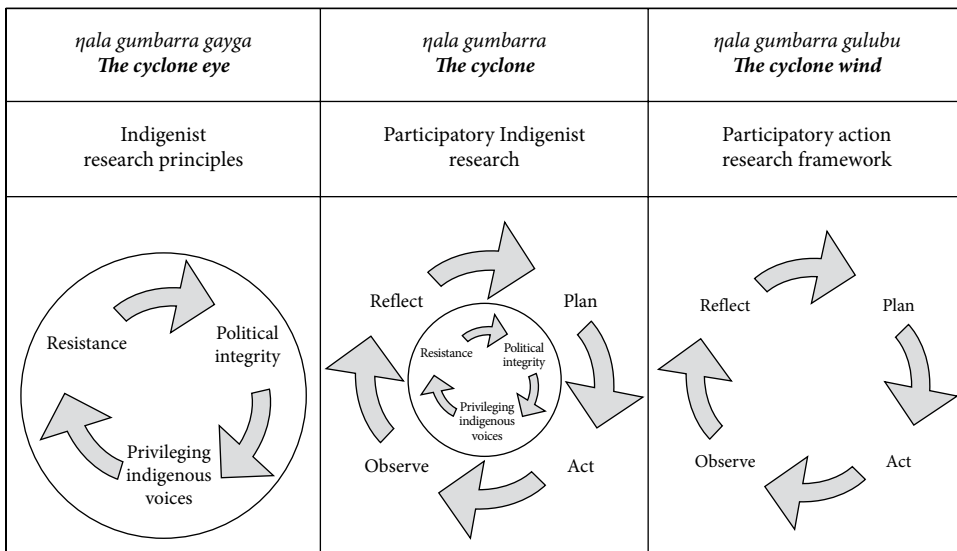


Figure 10.2 The combination of principles from Indigenist research and participatory action research are conceptualized by the eye and the wind of the cyclone.

In combining Indigenist research principles and PAR, (Figure 10.2), I have conceptualized both within the eye (*nala gumbarra gayga*) and wind (*nala gumbarra gulubu*) of the cyclone (*nala gumbarra*).

Indigenous research principles are the eye or center of the research analogy, with the cycles of the PAR framework forming its outer momentum. By using this approach, researchers can take into account the complex dynamics faced by Indigenous communities by planning, acting, observing, and reflecting to bring about change and action as experienced by Indigenous communities (Kemmis & McTaggart, 2005; Rigney, 1997).

In some recent research regarding the impact and context of communicable disease in the Torres Straits, we (Massey et al., 2011) have employed just such a model, combining PAR and the Indigenous research principles embedded in the cyclone. In two linked studies, one on influenza and the other looking at *Strongyloides stercoralis* (threadworm), these principles are applied as a checkpoint at every stage of the research. This is undertaken through application and the ongoing reflection on three questions: (1) Are we undertaking research that is a priority or of importance to Indigenous people in this context? (2) Are we recognizing and acknowledging the political integrity of this research with Indigenous people? (3) Are we ensuring that we actively promote Indigenous voices in this research (Rigney, 1997, p. 118)? The purpose for asking such questions is in guiding the effective and meaningful

participation of communities and organizations involved (Figure 10.3).

Plan

The communities and organizations involved in these studies are based on cultural connections, historical associations, and political assertiveness. Employment and capacity development have been core activities in forming relationships and collaborations. The ideal qualitative sample is one that is small enough to yield rich information to inform the research questions and that contains “critical cases,” “typical cases,” and also occasionally “deviant” cases (Schutt, 2006). The study of more than one case or setting strengthens the generalizability of the findings, hence the inclusion of quite diverse regions.

Acting Stage: Data Collection

During this stage, interview questions have been developed and piloted before interviews are undertaken. Notes should be taken during the interview and validated with the interviewee(s) at the end. Additional observational notes are taken about any other events that have arisen during the interview. The types of data collected could include:

- In-depth interviews, focus groups, observations
- Obstacles and aids to data collection
- Reflections on data quality (valid, reliable, and “thick”)

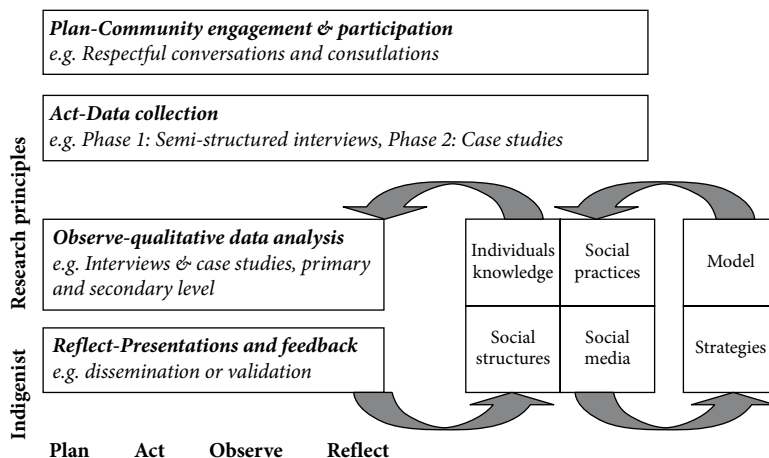


Figure 10.3 Indigenist research principles in practice.

A semistructured interview guide should be used, and participants who consent for interview will be asked a series of questions. Recruitment will continue until saturation is reached; that is, up to the point at which new interviews yield little additional information. The sample will include “critical,” “typical,” and “deviant” cases, as well as include more than one setting.

Observation Stage: Qualitative Data Analysis

This study design requires interviews to be thematically analyzed to develop a model that can be locally contextualized and implemented. Indigenous cultural protocols need to be adhered to in relation to the interviewer’s self-identity, gender, age, language, and confidentiality. Body language, prompts, judgmental language and gestures, dress standards, and the location and timing of the interviews are taken into consideration, and no individuals are identified in the data.

An example of primary level data analysis (Schutt, 2006) includes:

- Documentation
- Conceptualisation and coding
- Examining relationships and displaying data
- Authenticating conclusions
- Reflexivity

A secondary level data analysis example (Kemmis & McTaggart, 2005) seeks further analysis of the data to sort according to Individual’s Knowledge, Social Practices, Social Structures, and Social Media and to re-categorise the findings in a PAR matrix.

Reflect: Presentations, Feedback, Dissemination, or Validation

Presenting and disseminating preliminary findings to communities and organizations involved in a study is an essential step in this example. This allows for communities and organizations to provide early feedback and validation of the findings and to ensure active participation in the study. Perhaps more urgently, though, this is the point at which the cycle begins anew—reflection is an essential part of the next planning process. The image of the cyclone, that of a continuous swirl of people and ideas coming together to create change and renewal, is an essential element in communicating the purpose, process, and results of the research itself.

It is often stated that Aboriginal communities do not feel connected to research and cannot or do not understand or access research results (Estey, Kmetc, & Reading 2008; Hoare, Levy, & Robinson, 1993; Kirkness & Barnhardt, 1991). The cyclone process is one way to change that, by keeping people abreast of the intention, form, and content of research in an ongoing process that is integral to the research itself. Here, knowledge production and knowledge translation/dissemination are seamlessly (cyclonically) connected.

Between Two Methods: A Parenthetical Comment

Quite recently, Gobo (2011) observed that many Indigenous methodology studies seem to use pretty standard methods, and the studies discussed here do, indeed, use methods drawn and expressed in ways entirely consistent with and embedded in Western medical knowledge systems. But—and

this matters—the methodology, the overarching set of principles that contextualizes that knowledge is located in place, in the cyclone as a metaphor, in the cyclone as a means of communicating with the Aboriginal communities involved, and in the cyclone as the process through which Aboriginal peoples and interests remain at the “eye.”

One might argue (and we do) that it is vital to see and utilize the strengths of differing knowledge systems and contexts thoughtfully and creatively in academic and community settings (Wiber & Kearney, 2006; in Estey et al., 2008). This “two-eyed-seeing” (Iwama, Marshall, Marshall, & Bartlett 2009) also refers to the ability to turn a critical eye toward Western knowledge as “situated,” cultural knowledge, and it allows a simultaneous deconstruction of the dominant paradigm while resurrecting and generating Indigenous knowledge.³ The nature of this process, particularly as it exposes power and privilege, often suggests that Western and Indigenous worldviews are conflicting and in opposition to one another; yet, although the worlds are very different, they are not necessarily incompatible (Smylie et al., 2004). Western research is dominated by “epistemological and ontological disputes that tend to dichotomize quantitative and qualitative research approaches” (Botha, 2011). This dichotomy is both epistemologically and practically antithetical to Indigenous methodologies. Rather than knowledge as being paradigmatically oppositional, Indigenous knowledge is a “collective” knowledge generated by three different knowledge sources: traditional knowledge, empirical knowledge, and revealed knowledge (Castellano, 2000). According to Botha, Indigenous research methodologies can and should go beyond the current hermeneutic borders of conventional qualitative research to embrace more appropriate epistemological and axiological assumptions and suggests a mixed-methods approach as a vehicle for moving beyond these paradigms.

Indigenous ontology is frequently characterized as being “process oriented”; that is, an action and “eventing” approach to life versus a world of subject–object relationships. “Individuals live and enact their knowledge and, in the process, engage further in the process of coming to be—of forming a way of engaging others and the world” (Duran & Duran, 2000). Positivist research paradigms not only produce “colonizing research,” they are contrary to the understanding that knowledge is founded on subjectivity (Cajete, 2000; Marsden, 2003). Subjectivity, as an enactment of an Indigenous research ethic

that derives knowledge from ways of knowing, being, and doing (Martin and Mirraboopa, 2003), is also informed by internally informed sources such as dreams, visions, stories, interspecies communications, and internal efforts to maintain spiritual balance (Cajete, 2000; Deloria, 2006; Getty, 2010; Kawagley, 2001). These ways of knowing are, among other things, deeply metaphorical and symbolic and must be understood within a particular cultural, geographical, and linguistic context, and it is this knowledge that has been most impacted by cultural oppression.

One cannot separate these two because the research itself is embedded in activism (Swadner & Mutua, 2008). Indigenous scholars advocate for clear, culturally informed ethics to guide both research and the ongoing dialogue between intersecting worldviews (Ermine, 2005; Tait, 2008). This involves approaching the research with commitment and following the “right path” in the quest for meaning and understanding and how knowledge is handled legally, economically, and spiritually (Cajete, 2000).

Both PAR and Indigenous methodologies focus on process, relationships, justice, and community and are therefore theoretically oriented to evolving research designs and plans. Indigenous methodologies, however, are frequently grounded in the tribal affiliation of the researcher as a statement of identity and respect (Kovach, 2009) and as a process that enables the illumination of particular cultural values and beliefs (Wilson, 2008). As we’ll discuss shortly, culturally derived relational metaphors are often used to both frame the research paradigm and explicate the findings and are reflective of a relational epistemology focusing our attention on our interrelatedness and interdependence with each other and our greater surroundings. These relations are part of complex and multilayered, multiembedded systems that are dynamic and evolving (Getty, 2010; Henderson, 2000; Little Bear, 2000). From particle to universal, each system contributes to the functioning of a larger encompassing system. “All relationships are tied to other relationships. There is a vertical process and a horizontal process, and these processes are constantly intertwining with each other to create reality” (Cajete, 2000, p. 41). In other words, iterative and positioned processes typify Indigenous knowledge systems.

Building a Red River Cart

The Métis are a distinct and constitutionally recognized Aboriginal community in Canada. Born of

the interaction of First Nations and Europeans in the fur trade, the Métis developed as a distinct and politically self-conscious nation in the nineteenth century, co-occupying a vast area in central and northern North America (see the collections edited by Peterson & Brown, 1985; St-Onge, Podruchny, & MacDougall, 2012) until colonization by the Canadian and American States abruptly marginalized them. In Canada today, although legislative and legal distinctions are between Métis and First Nations (see Teillet, 2009), unfortunately, one of the things shared between Métis and First Nations is that their interactions with the same colonial government has resulted in similar, although not the same, social issues. For example, both Métis and First Nations share a similar inequity in health status when compared with the general population of Canada (Adelson, 2005; Gracey & King, 2009).

To address this inequity in health status with the general population, Métis communities, along with other colonized Indigenous peoples, have called for programs developed by their own community. This is an alternative to receiving programs and policies that are derived from outside of the community, one that hopes, in part, to provide a service that is culturally imbued or familiar with the expectation that such familiarity increases participation in and the effect of the program.

The *community readiness model* (CRM), originally developed by Plested, Edwards, and Jumper-Thurman (2006), is one that seeks to understand, assess, and increase community readiness for program interventions in an integrated fashion. The model is particularly useful for health-related program development because it considers readiness in terms of a specific issue and in ways that can be measured across multiple dimensions, with due concern for variation across dimensions and between and within communities. Readiness can be

increased during the process of assessment by bringing key actors together to consider an issue. Indeed, the development of a community consensus and assessment is, in fact, an essential element of developing the strategies and interventions required. By using participatory methods in investigating the readiness of a community, the technique promotes community recognition and ownership of the issue and its solution. Effective inclusion of community promotes cultural continuity and sustainability by promoting the use of community experts and resources while developing a program that is manageable by the community (i.e., consistent with its readiness and capacity). The community must identify befitting strategies that are congruent with their level of readiness. In 2008, as part of a broadly conceived research program with the Métis community in the Okanagan Valley of British Columbia, Hutchinson facilitated research on the readiness of the community to take greater control of their community health agenda and to identify one or more key issues.

To do this, he and his collaborators started by assessing previous efforts made by the community around the issue of health, the general knowledge of those efforts within the community, and how current community leaders were addressing health. Additional concerns were the general community's understanding of the issue, its priority, and what resources were available to address the development of a community health agenda. The seven-step model is reproduced in Figure 10.4.

The method utilized to assess readiness within the CRM is primarily interviews and surveys. Plested, Edwards, and Jumper-Thurman (2006) also suggest utilizing reviews of policies and programs and academic literature to finalize an assessment of a community's readiness. The interviews and surveys rely on scaled responses from participants to provide

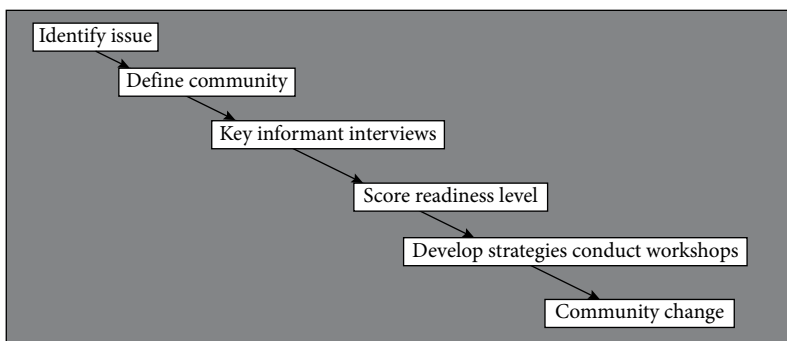


Figure 10.4 Seven steps in the community readiness model (Plested et al., 2006).

the basis for analysis and determination of a community's level of readiness. A community may be at several different levels of readiness, including:

- No Awareness
- Denial/Resistance
- Vague Awareness
- Preplanning
- Preparation
- Initiation
- Stabilization
- Confirmation/Expansion

After assessing the community's readiness, researchers and the community itself are in a better position to address the issue being investigated.

After using the CRM, a very Métis-specific critique emerged, with people indicating that the model itself (that is to say, the methodology) was not sufficiently reflective of their own experiences. The community members, service providers, and leaders wondered if readiness was really quantifiable, and, perhaps more importantly, if complex issues are usefully reduced to a single issue and whether the research focus—rather than a program delivery focus—was warranted. As an Aboriginal community that is (and indeed was) fundamentally dispersed, people noted that the CRM assumed a high level of cohesion within the community (overall and within specific issues) and, indeed, almost presupposed that the community was geographically bounded (note that this has been identified as an issue for Métis health-related research more generally; see Evans et al., 2012). People also noted that there was a danger in framing the work as research based on

a one-time assessment because community needs are continuous and evolving.

As a result, a large gathering was held to consider approaches to community change around health issues and to derive the community's own model. Expressed in terms derived from the original CRM, to which they had been introduced, the major insight that people felt needed to be incorporated was that the process be reiterative and reflective upon itself at every new stage (see Figure 10.5). In a community as complex as that of the Métis, the appropriate interlocutors (i.e., the community) change as an issue is identified; in defining the community, the issue will change to reflect the community's areas of interest; by effective action, community change occurs throughout the process; key informants affect the framing of the issue; and, in workshops and strategies, community and the issue are redefined. At the meeting, one participant noted that it was like a wheel spinning, in that the same point would come around again and again with new information and in a slightly different context.

The new model had to allow for a borderless community because Métis are located both physically and sociologically within other communities, tied to each other by kinship, identity, and culture (for a discussion of this in the British Columbian context, see Barman & Evans, 2009; Evans, Barman, Legault, Dolmage, & Appleby, 2012). Rather than readiness, a model of preparedness was proposed, prioritizing knowing the community, recognizing and engaging the infrastructure within the communities, and being responsive to change. The Métis felt that readiness was very static, and investigating readiness as proposed would become burdensome

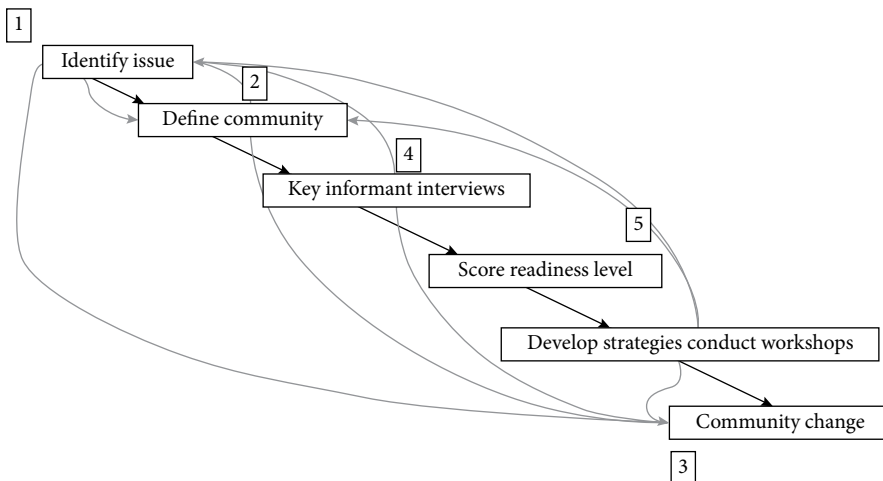


Figure 10.5 Revising the community readiness model within a Métis community.

to actually delivering programs to address community issues.

To illustrate the newly developed process of investigating preparedness to respond to Métis community issues, a Red River Cart was utilized (Figure 10.5). The Red River Cart, pulled by an ox or occasionally by a horse, was developed by Métis during the 1700s as a means of transporting people and goods across Canada. It is no longer used except as a symbol of Métis identity. To illustrate the new model, the Métis focused on the wheel of the cart while noting that the entire cart was representative of the whole of society (see Figure 10.6).

The rim of the wheel represents the community members interacting with service providers (represented by the spokes). The spokes are held in place by the hub and the rim. The hub is the community organized together as a political or advocacy group, whereas the axle is a group of Métis who work with multiple Métis groups at a larger geographic level (provincially and nationally). The Métis Red River Cart Model is a culturally salient image of and for community preparedness, through which multiple issues and agendas may form. The model also highlights the necessity for resource sharing because no

one single part can operate independently of the other; an increased number of spokes and a larger rim can be supported, but this requires a stronger hub and axle. The wheel on the other end of the axle is representative of the non-Aboriginal population; to assure equity in society (being able to carry a load in the cart), both wheels require the same number of spokes and the same strength in the hub and axle.

The mobility evoked by the cart image also reflects the reality that the Métis are not geographically bound, and Métis communities are frequently much more difficult to pin-point and encompass than those of other Aboriginal peoples. In terms of self-governance and determination, this requires the Métis to effectively communicate with the larger population, share resources, and utilize administrative centers or hubs. Communication is central to the success of any program; with established links between community members, service providers, advocates, and political representatives, Métis community members can find out about new programs, while service provider can find out about the needs of the community members and relay them to advocates and political representatives. As each spoke shares the load of the cart, so do service providers

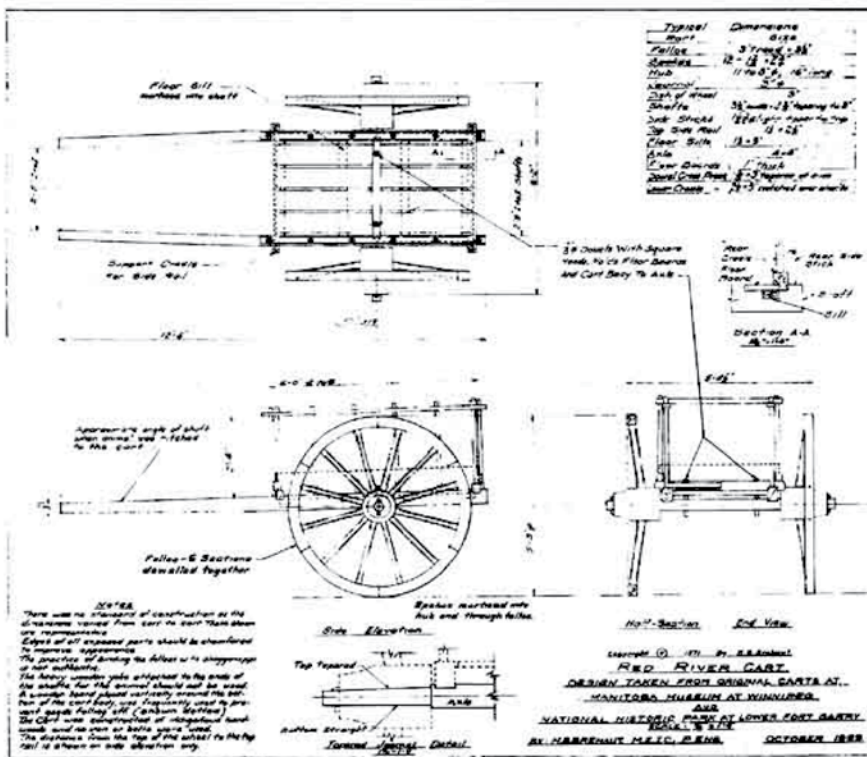


Figure 10.6 Historic diagram of a Red River Cart (Brehaut, 1971–2).

share resources when delivering services and distributing resource demand over many services. Having a centralized administrative hub reduces the need for each service provider to be expert in finances, grant writing, or political wrangling. Administrative centers can provide a central focal point for communication, networking, and development work between chartered communities.

So, as the wheel turns, the load is distributed across some spokes more than others. This represents when community members are in direct interaction with service providers. It is at this point that service providers are allowed insight into the community and any new, changing, or resolved issues. With this insight, the service providers can share their knowledge with advocates and political organizations so that they, in turn, can share it with the larger population in order to address the issue in a novel manner specific to the community. This model supports and promotes a community that is prepared to address issues rather than consume scarce time and resources through a model constantly re-researching issues to resolve. It promotes a method, then, that draws directly, appropriately, and compellingly from the community from which it comes.

Using the analogy of the Red River Cart as part of a community-building process iteratively embeds Métis values and protocols; CRM is thus transformed by these values into a process more appropriate (and yes, more Métis) than CRM in the first instance. Even though modern Métis may not have any dealings with a Red River Cart in their lifetime, the image resonates, and the icon matters in terms of motivating people to manage change—to be ready to move as it were. In our process, the possibility of using metaphors and meanings derived directly from Michif (the language of the Métis) did arise, but relative absence of Michif in the community today meant that language-based epistemological difference was less accessible, and the use of a cultural icon provided a better link between visual representation and realized process. The process, thus (re)constructed, was one of reiteration, reflection, and revision in a circular manner—like a wheel spinning forward.

Conclusion

That the effects of colonialism on Indigenous peoples in Australia and Canada are profound is as obvious as the resistance that Indigenous peoples have mounted in response. At a fundamental epistemological and ontological level, Indigenous

methodologies are just that, Indigenous: they arise in the context of a response to colonial pressures. But these Indigenous ways of knowing, these ways of finding out, are also an autochthonous expression of the knowledge systems that order lifeways in and among Indigenous communities, and both Indigenous methods and methodologies in turn contribute to the vitality of those communities and people. At once part of decolonization, Indigenous methodologies are more as well; they are positive affirmation that Indigenous people themselves can draw on their own epistemological resources to enact something other than the chaos that characterizes the last few hundred years. Time will tell what new orders of things, people, and relationships may arise.

Future Directions

Indigenous studies has emerged rapidly over the past decade or so as a distinct academic discipline. National organizations representing and facilitating the work of scholars in the field are numerous, and, more recently, a transnational organization, the Native American and Indigenous Studies Organization (see <http://www.naisa.org/>) has emerged. The development of Indigenous methodologies is related to the rise of Indigenous studies as a discipline, but the relationship between Indigenous scholars and research (including that of non-Indigenous scholars in traditional Western academic disciplines) in Western institutions like universities and Indigenous communities remains conflicted.

Over the next several years, the nature and positioning of a professional practice in Indigenous studies will shift and develop. At issue are both institutional and wider political relations and how the specific research traditions of particular communities inform and interact with each other in the context of a more general practice of Indigenous research. That is, how do very specific Indigenous methods interrelate? What are the axes of similarity and difference between particular traditions, and how do these intersect with a common colonial history and commonalities that precede (and carry into and through) the impact of colonization? These are not simply questions for the academe or for Indigenous intellectuals and politicians as a group, but rather they are of immediate concern for Indigenous peoples in communities. There are very practical questions considering how the efficacy of Indigenous methodologies are assessed in their impact on the utility of research being done

in Indigenous communities. Do Indigenous measures of success emerge from the methodologies themselves and, if so, how? Furthermore, how do communities themselves take control of research practices? What are the basic capacities that communities need to develop to undertake research using Indigenous methodologies? How shall Indigenous researchers be trained? And when and how will the contributions of non-Indigenous researchers be integrated into contemporary Indigenist research agendas?

All these issues have implications for Indigenous people inside communities and inside educational institutions, and knowledge, power, and pragmatic concerns are very much in the foreground. This is as it should be, and the recognition of the affect of knowledge and knowledge claims on Indigenous people is a key step in decolonizing old systems of thought and reindigenizing new ones.

Notes

1. In this paper, we use the terms Indigenous and Aboriginal interchangeably. There is significant variation in the terminology from place to place, although in both Canada and Australia the term “Aboriginal” is in use. Even here, however, there are differences, with the term usually being used as an adjective in Canada and frequently as a noun in Australia. Naming matters (see Chartrand 1991), and so when the discussion is linked to a particular place, we will use the naming conventions of the Indigenous peoples of that place; consistent with the literature, the term Indigenous is used to refer to original peoples generally and collectively.
2. A careful reader will note a shift in voice here. This section of the paper is written primarily by AM, an Aboriginal scholar, describing research undertaken drawing on the knowledge and epistemology of his mother’s people. A similar but slightly different shift occurs in the second case study, where a plural pronoun is used to reflect the fact that PH and CD participated in the process described, and, more importantly, there was a direct and collective process through which conclusions were derived.
3. This is entirely consistent with Donna Haraway’s radical admonition in her 1988 paper “Situated Knowledges” that the overarching god’s-eye view of claims of Western knowledge systems be disputed from grounded and transparent positions and knowledge systems.

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Narrative Inquiry, Field
Research, and Interview
Methods

Practicing Narrative Inquiry

Arthur P. Bochner *and* Nicholas A. Riggs

Abstract

This chapter focuses on the intellectual, philosophical, empirical, and pragmatic development of the turn toward narrative, tracing the rise of narrative inquiry as it evolved in the aftermath of the crisis of representation in the social sciences. Narrative inquiry seeks to humanize the human sciences, placing people, meaning and personal identity at the center, inviting the development of reflexive, relational, and interpretive methodologies and drawing attention not only on the actual but also to the possible and the good. The chapter synthesizes the changing methodological and ethical orientations of qualitative researchers associated with narrative inquiry; explores the divergent standpoints of small- story and big- story researchers, draws attention to the differences between narrative analysis and narratives-under-analysis; and reveals narrative practices that seek to help people form better relationships, overcome oppressive canonical identities, amplify or reclaim moral agency, and cope better with contingencies and difficulties experienced over the course of life.

Key Words: narrative, storytelling, narrative identity, reflexive methodologies, small stories, narrative analysis, autoethnography, qualitative inquiry, acts of meaning, interpretive social science

We grasp our lives in a *narrative*. In order to have a sense of who we are, we have to have a notion of how we have become, and of where we are going.

– Charles Taylor (1989)

We tell stories because that’s what we have to do. It’s what we’re all about. We care for one another with the stories we place in each other’s memory; they are our food for thought, and life.

– Richard Zaner (2004)

People are constantly telling stories. We tell stories to ourselves and stories to others; stories about ourselves and stories about other selves. Apparently, self-telling is a human preoccupation. We assume there is something akin to a “self” to tell stories to or about. As we tell stories about others, we construct images or meanings of them and their actions, categorizing or classifying them—in a sense, making

them up (Hacking, 1999). The same can be said about the stories we tell about ourselves. On this view, one’s self—my-self or your-self—can be understood as a telling (Schafer, 1980) and a consequence of “relational being” (Gergen, 2009). As a result, the idea of a unified, fixed, and singular self ontologically prior to and apart from a person’s living experience is replaced by the notion of a

multiple, fluid, and negotiated identity that is continuously under narrative construction—a process that is never complete as long as we live and interact with others.

Moreover, telling stories is one of the primary ways we “reckon with time” (Ricoeur, 1981, p. 169). We are historical beings who live in the present, under the weight of the past and the uncertainty of the future. Our language alerts us to a consciousness of there and then, here and now, and sooner or later. We are called on to make sense of and remember the past in order to move ahead and attend to the future. Thus, time, memory, and narrative are inextricably linked.

A newborn baby is devoid of story. Still, each of us is born into a world of stories and storytellers, ready to be shaped and fashioned by the narratives to which we will be exposed. Whether we like it or not, our lives are rooted in narratives and narrative practices. We depend on stories almost as much as we depend on the air we breathe. Air keeps us alive; stories give meaning to our lives. They become our equipment for living. As Myerhoff (2007, p. 18) observed, “It is almost as if we are born with an inconclusion and until we fill that gap with story, we are not entirely sure, not only what our lives mean, not only what secrets require our attention, but that we are there at all.”

When we are children, we soak up cautionary tales that shape and guide us. We are exposed to fairy tales and tall tales, ballads and legends, myths and fables, epics and folklore. From *The Arabian Nights* to Grimm’s *Fairy Tales*, *Aesop’s Fables*, *Uncle Remus* stories (see Joel Chandler Harris, 1881) and beyond, the plots and moral precepts of the human dramas into which we have been born are transmitted to us in stories. Along the storied highway of life, we meet monsters and heroes, fantastic creatures with extraordinary, magical powers, saints and evil-doers, beauties and beasts. Over and over again, we hear, “*Once upon a time*,” “*Happily ever after*,” and “*The moral of the story...*” Gradually, we accumulate a reserve of stories to which we can appeal when the occasion calls for it. If we get in trouble, we may even summon a story to save our skin.

As students and as family members, we read, write, and listen to stories, learning to compare and analyze them. The stories to which we are exposed tell us who we are; where and how we are located in ethnic, family, and cultural history; where we have come from, where we may be going, and with whom. Passed to us by our elders and significant others, these stories become our narrative

inheritance (Goodall, 2005). In the grip of stories, we absorb the lore of the past and find expression for codifying our dreams about the future. We watch the characters in these stories work through the dramatic plots and troubles of a lifetime. We learn to feel and identify with some, but not all, of the characters. The plots of these stories introduce us to good and evil, love and hate, heaven and hell, right and wrong, birth and death, war and peace, suffering and healing, and a wide swath between the extremes. Throughout our lives, we are coached to keep some stories private and to guard these secret stories as if our lives depended on protecting and keeping them safely out of sight or earshot.

In the meantime, we find we must move on, living out and through our storied existence. Sometimes, we find ourselves in stories we would rather not be living. Often, we re-story our lives, revising the meaning of the tales in which we have been immersed, constructing new storylines to help us exert control over life’s possibilities, ambiguities, and limitations. In some of our stories, we claim ourselves as heroes; in others, we are dreamers; in still others, we are traumatized victims or survivors. Other people in our lives are characters in our stories, and we are characters in theirs (Bochner, 2002; Parry, 1991). A storied life is a negotiated life collaboratively enacted and performed in dialogue with the other characters with whom we are connected. Thus, the stories we live out are a relational, co-authored production. As Arthur Frank (1997, p. 43) says, “Stories are the ongoing work of turning mere existence into a life that is social, and moral, and affirms the existence of the teller as a human being.” It turns out that the stories we tell are not only *about* our lives; they are *part* of our lives (Rosenwald & Ochberg, 1992).

The philosopher Heidegger (1889–1976) construed humans as “beings whose lives are *at issue* or *in question*” (Richardson, Fowers, & Guignon, 1999, p. 220). Similarly, Ricoeur (1985, p. 263) wrote that “On a cosmic scale, our life is insignificant, yet this brief period when we appear in the world is the time in which all meaningful questions arise.” In other words, we are self- and other-interpreting animals for whom being is constituted in and by questions about what is important, good, or meaningful. To be a person, I am compelled to ask what kind of life is worth living and to measure the meaningfulness of my life against some version of the good acceptable to me, which requires a narrative understanding—“a sense of what I have become which can only be given in a story” (Taylor, 1989, p. 48).

Thus, the human condition is largely a narrative condition. Storytelling is the means by which we represent our experiences to ourselves and to others; it is how we communicate and make sense of our lives; it is how we fill our lives with meaning. To study persons is to study beings existing in narrative and socially constituted by stories. From bedtime stories to life reviews—across the span of our lives—we listen to stories and tell stories of our own. Myerhoff (2007, p. 18) called this passionate craving for story a “narrative urge,” while Fisher marked it as an Archimedean point signified by the phrase *Homo narrans* (Fisher, 1984, 1987).

The Rise of Narrative Inquiry in the Social Sciences

It seems as if a lot of people have been waking up after a long and strange slumber, asking: Why don't we study people? Mark Freeman (1998, p. 27)

It took a long time for the social sciences to come to narrative (Bochner, 2014). Not until 1982, when Donald Spence published *Narrative Truth and Historical Truth*, a book that challenged one of the foundational premises of psychoanalysis, did psychology begin to show a concerted effort to understand how individuals are shaped and changed by the stories in which they live and act (Josselson and Lieblich, 1997). Spence (1982) argued that psychoanalysis was not akin to an archaeological excavation of a person's historical past, as Freud (1914) had argued, but rather involved a collaborative construction of a coherent and credible story shaped out of bits and pieces of disclosed memories, imagination, and associations. It wasn't the events themselves, but the meanings attributed to events, that shaped a person, and these meanings could be reframed and reshaped into a story that gave new hope and promise to a despondent individual plagued by doubt, despair, and/or dejection.

Four years later, Theodore Sarbin (1986) published *Narrative Psychology*, an edited collection of essays and research monographs that focused on “the storied nature of human conduct.” Reacting to “the epistemological crisis in social psychology,” Sarbin (1986, p. vii) offered narrative psychology as “a viable alternative to the positivist paradigm” of psychological research, one which could pull psychology out of its state of disillusionment by replacing the mechanistic and reductionist postulates of positivism with a humanistic paradigm highlighting story making, storytelling and story comprehension (Sarbin, 1986). Sarbin's conviction that

narrative could serve as a root metaphor for a revitalized social psychology grew out of conversations in 1979 with three of the most profoundly influential narrative theorists—historians Louis Mink (1970) and Hayden White (1975, 1980) and the narrative theologian Stephen Crites (1971)—while he was a visitor at the Center for the Humanities at Wesleyan University. Although Mink and White were deeply skeptical of narrative's capacity to represent real events—“stories are not lived but told” (Mink, 1970, p. 557)—both affirmed narrative's constitutive role in history's search for and claim to knowledge, as well its formidable power to provide a framework that can make the past intelligible. In Crites' (1971) manifesto on narrative, he resisted the temptation to view narrative as merely one way to organize and make sense of experience, arguing instead that everything experienced is experienced narratively—human life is storied life all the way down and back. Acknowledging the significance of time and memory, Crites (1971) argued that human consciousness takes an inherently narrative form.

Prior to the publication of the books authored by Spence (1982) and Sarbin (1986), the term “narrative” had no recognizable status in psychology either as a methodological orientation or as a topic of research in the study of personal, interpersonal, or therapeutic relationships. By 1992, however, Krieswirth (1992) felt it necessary to account for what he called “the narrative turn” in the human sciences. Not only was psychology turning toward narrative but so were economics (McCloskey, 1990), law (Farber & Sherry, 1993), education (Connelly & Clandinen, 1990), history (Mink, Fay, Golob, & Vann, 1987; White, 1987), psychoanalysis (Coles, 1989; Schafer, 1980; Spence, 1982), psychotherapy (White & Epston, 1990), sociology (Richardson, 1990), and ethnography (Turner & Bruner, 1986).

Between 1986 and 1994, Bruner published his essay on “life as narrative” (Bruner, 1987) and his books on “possible worlds” (1986) and “acts of meaning” (Bruner, 1990); Polkinghorne (1988) urged a fuller appreciation of the realm of meaning, and hence narrative, as a core concern for all the human sciences; McAdams (1985) defined identity as a psychosocial problem of arriving at a coherent life story; Rosenwald and Ochberg (1992) introduced a critical-cultural perspective for investigating the stories people tell about their lives; Mair (1989) made the case for a narratively grounded “poetics of experience”; Parry (1991), Schafer (1992), and White and Epston (1990) proposed a framework for narratively based therapies; Shotter and Gergen (1989) edited

a collection of essays that examined the narrative textuality of the self; Richardson (1990) argued for a sociology that narrated lives instead of abstracting forces; Ellis and Bochner (1992) developed the methodology of co-constructed personal narratives and promoted the idea of performed autobiographical research stories that would give audiences the kind of experiential, emotional immediacy lacking in traditional forms of research; Tedlock (1991) and E. Bruner (1986) described the emergence of narrative ethnography; Langellier (1989) gave credibility to the study of personal narratives as a means of validating the voices of marginal and silenced individuals and groups; Connelly and Clandinin (1990) underscored the ways in which educational research can be viewed as stories on several levels; Coles (1989) called for more stories and less theory in order to open up the moral imagination of teachers, researchers, and psychiatrists; Josselson and Lieblich (1993) initiated an annual publication focused on the study of life narrative in psychology that would call attention to people telling their own stories about what had been significant in their lives; and Freeman (1993) drew attention to the neglect of and importance for the autobiographical subject and memoir in psychology. Krieswirth (1992, p. 629) pointed out what had by then become obvious: "As anyone aware of the current intellectual scene has probably noticed, there has recently been a virtual explosion of interest in narrative and in theorizing about narrative."

As the end of the twentieth century approached, the narrative turn accelerated and intensified. In particular, personal narratives (Clandinin & Connelly, 1994; Ellis & Bochner, 2000; Langellier, 1999), life histories (Freeman, 1993; Tierney, 2000), life stories (McAdams, 1993), testimonios (Beverley, 2000), poeticized bodies (Pelias, 1999), and memoirs (Couser, 1997; Freeman, 1993; Miller, 2000) became widely viewed as significant materials and methods for conducting inquiry, as well as major topics of research across the human sciences (see e.g., Church, 1995; Denzin 1997; Ellis & Bochner, 1996; Gubrium & Holstein, 1997; Plummer, 2001). By the turn of the century, Denzin and Lincoln (2000, p. 3) could conclude, "Now, at the beginning of the 21st century, the narrative turn has been taken."

Why Narrative?

In the 1990s, narrative inquiry became a rallying point for those of us who believed that the human sciences needed to become more human. To some extent, the burst of enthusiasm for personal narrative and the study of lives (Josselson & Lieblich,

1993) was a response to the questionable ethics and doubtful appropriateness of standard methodological practices in the social sciences (Apter, 1996). In the human sciences, we are supposed to be studying people, observing their lived experiences, and trying to understand their lives, and narratives come closer to representing the contexts and integrity of those lives than do questionnaires and graphs (Freeman, 1997, 1998a). Thus, the narrative turn is widely viewed as an expression of dissatisfaction with received views of knowledge, in particular a rejection of positivist and postpositivist social science. But the enthusiasm for narrative inquiry was sparked as much by existential, ontological, and moral concerns as by a methodological change of heart. Narrative is as much about the *possible* as it is about the actual. Many of those drawn to narrative inquiry wanted to imagine, discover, or create new and better ways of living. As Freeman (1998a, p. 46) said, "We need to understand lives and indeed to *live* lives differently if we are to avoid further fragmentation, isolation, and disconnection from each other."

Now, nearly a full generation later, we can say confidently that the turn toward narrative in the social sciences is not a passing fancy. Nor is it a movement confined to a small group of disgruntled, renegade, eccentric, self-indulgent, and/or alienated individuals, as Atkinson (1997) argued (see e.g., Bochner, 2001; Sparkes, 2001, for responses to Atkinson's arguments). On the contrary, the inspiration for the narrative turn penetrates deep into the conscience of those who embrace it. To comprehend the sources of this inspiration, one must understand the demographic, intellectual, social, and cultural conditions under which the most recent generations of researchers and graduate students in the social sciences have been educated. They have been exposed to a far different conception of how and for what purposes knowledge is produced than academics entering the social sciences prior to the 1990s.

Turning away from the Correspondence Theory of Knowledge

A turn toward something can be seen as a turn away from something else. To understand the context in which researchers in the human sciences began to turn toward narrative, it is helpful to consider how the postmodernism and post-structuralism of the time was challenging some of the most venerable notions about scientific knowledge and truth.

Early in the 1960s, Kuhn (1962) used the history of science to show that the building-block model of science lacked foundations. According to Kuhn (1962), scientific revolutions were more akin to conversions—from one paradigm to another—than to discoveries. Taking up where Kuhn left off, Rorty (1979, 1982), Toulmin (1969), Feyerabend (1975), and Sellars (1963) illustrated how the “facts” scientists see are inextricably connected to the vocabulary they use to represent them. At about the same time, Lyotard (1984) debunked the belief in a unified totality of knowledge, questioning whether master narratives (or general theories) were either possible or desirable; Barthes (1977), Derrida (1978, 1981), and Foucault (1970) effectively obliterated the modernist conception of the author, altering how we understand the connections among authors, texts, and readers/audiences; Bakhtin (1981) broadened the interpretive space available to the reader of a social science text by encouraging multiple perspectives, unsettled meanings, plural voices, and local knowledge that transgresses claims to a unitary body of theory; feminist critical theorists such as Harding (1991), Clough (1994), Harstock (1983), and Smith (1990, 1992) promoted the unique and marginalized standpoints and particularities of women; and multicultural textualists such as Trinh (1989, 1992), Anzaldúa (1987), and Behar (1993, 1996) exposed how the complexities of race, class, sexuality, disability, and ethnicity are woven into the fabric of concrete, personal lived experiences.

By the mid-1980s, the social sciences were experiencing “a crisis of representation” casting a shadow of doubt on the validity and efficacy of the theory of language on which orthodox approaches to scientific knowledge were based (Clifford, 1988; Clifford & Marcus, 1986; Geertz, 1988; Marcus & Fisher, 1986; Turner & Bruner, 1986). This “correspondence theory of knowledge” hinged on the assumption that language can achieve the denotative and referential function of describing objects in a world *out there*, apart from and independent of language users (Bochner & Waugh, 1995; Rorty, 1967, 1982, 1989). To hold to this assumption was to grant that the words used in scientific descriptions do not specify *a* world, but rather represent *the* world, and that words can denote what is *out there* in the world apart from, or prior to, the interpretations (or descriptions) of researchers who use them.

Beginning with Kuhn (1962), however, the history and philosophy of science showed that we

should understand language not as simply a tool for mirroring what is describable about reality, but rather as an ongoing and constitutive quality of reality (Bochner & Waugh, 1995). What it is possible to say about the world involves the indistinguishable provocations of the world *and* the interventions of language by which we make claims about that world. In short, the world we social scientists seek to describe does not exist in the form of the sentences we write when we theorize about it (Rorty, 1989).

Thus, the cultural context of social science research that launched the turn toward narrative was one in which some of the most venerable notions about scientific truth and knowledge were being contested (Denzin, 1997; Lyotard, 1984). The traditional ideas of an objectively accessible reality and a scientific method turned out to be, in Richard Rorty’s (1982, p. 195) words, “neither clear nor useful.” What was needed, argued Rorty (1982, p. 195), was an approach to social science “which emphasizes the utility of narratives and vocabularies rather than the objectivity of laws and theories.” Sensing that this was one of those rare “experimental moments” (Marcus & Fisher, 1999) akin to a Kuhnian paradigm clash (Kuhn, 1970), advocates of a meaning-centered, interpretive, and qualitative social science rapidly began to introduce new models and methodologies applicable to a paradigm of narrative inquiry (Spector-Mersel, 2010), such as systematic sociological introspection (Ellis, 1991), biographical method (Denzin, 1991), personal experience methods (Clandinin & Connelly, 1994), feminist methods (Reinharz, 1992), consciousness-raising methods (Hollway, 1989), co-constructed narrative (Bochner & Ellis, 1992), and interactive interviewing (Ellis, Kiesinger, & Tillmann-Healy, 1997), and to propose new subfields of inquiry sympathetic to the shift toward more personal, emotional, and story-based forms of inquiry such as personal sociology (Higgins & Johnson, 1988), autobiographical sociology (Friedman, 1990), private sociology (Shostak, 1996), emotional sociology (Ellis, 1991), indigenous anthropology (Tedlock, 1991), autoanthropology (Strathern, 1997), anthropology of the self (Kondo, 1990), anthropology at home (Jackson, 1987), anthropological poetics (Brady, 1991), autoethnography (Ellis & Bochner, 1996; Bochner & Ellis, 2002), and autoethnographic performance (Park-Fuller, 1998).

Disputing the capacity of language and speech to mirror experience (Rorty, 1979), postmodernists revealed that there was no access to the world

unmediated by language. No methods exist that can warrant a claim to describe reality as reality would describe herself if she could talk (Rorty, 1982). Because the world can't speak for itself, all attempts to represent the world involve transforming a speechless reality into a discursive form that makes sense. To the extent that descriptions of the social world thus involve translating "knowing" into "telling," they may be viewed as narratives (White, 1980). Thus, all social science writing is a narrative production saturated by gaps between experience and its expression.

Representing social reality accurately in language is a problem because the constitutive quality of language creates experience and necessarily transforms any data it describes. If language is not simply a tool for mirroring reality, but is rather an ongoing and constitutive part of reality, then our research agenda needs to take into account how, as social scientists, we are part of the world we investigate and the ways we use language to make and change it. Accordingly, our focus becomes showing how meaning is performed and negotiated by and between speakers (research participants) and interpreters (researchers) (Bochner & Waugh, 1995), a distinctively narrative project (Bruner, 1990).

In a succession of handbook articles dealing with perspectives on inquiry, Bochner (1984, 1994, 2002) argued that the legitimization of this sort of meaning-centered, narrative inquiry is contingent on breaking free of certain disciplinary norms pervasive across the human sciences that idealize the significance of abstractions over details, stability over change, and graphs over stories. The problem, he reasoned, is not with science per se, but with a reverent and idealized view of science that positions science above the contingencies of language and outside the circle of historical and cultural interests (Bochner, 2002; Bochner & Waugh, 1995). Although academic disciplines that have been deeply entrenched in the correspondence theory of knowledge, such as mainstream psychology, sociology, and communication studies, have been slow to respond to the challenges posed by the crisis of representation, a new generation of social and human scientists who understand language as a means of dealing with the world have responded by opening new vistas of inquiry, experimenting with new research practices, and turning increasingly toward narrative, interpretive, autoethnographic, performative, and other qualitative approaches to inquiry that emphasize ways in which research in the human sciences is a relational, political, performative, and

moral endeavor that puts human meanings and values into motion (Bochner, 2002, 2012; Bochner & Ellis, 2002; Chase, 2011; Denzin, 1997; Denzin & Lincoln, 2000, 2005, 2011; Ellis, 1995, 2004; Ellis & Bochner, 1996, 2000; Geertz, 1995; Gergen & Gergen, 2000, 2012).

Changing Demographics: Evolution of a New Academic Culture of Inquiry

Students entering graduate schools in the 1990s thus began their lives as researchers and scholars under a cloud of epistemological doubt. During this period, a dramatic shift took place in the demographic composition of the graduate student population. There was a rapid increase in the enrollment of women, middle- and lower-class people, blacks and Hispanics, and students from Third- and Fourth-World countries (Geertz, 1995). Gradually, these demographic changes led to a globalization of the curriculum and courses that stressed a greater appreciation for divergent rationalities grounded in cultural, racial, ethnic, gender, and class diversity (Shweder, 1991). Prepared by their lived histories to understand how a vocabulary of neutrality, objectivity, and scientific detachment could easily function as a tool of oppression and domination, these newcomers hungered for a research agenda that resonated with their lives and lived experiences. In the aftermath of postmodernism, they were reluctant to view the task of producing knowledge and representing reality as unproblematic. They understood research as a social process, as much a product of interaction as of observation, and one inextricably bound to the embodied experiences and participation of the investigating self. Already inspired to question conventionality, power, and a monolithic view of research practices, and now reinforced by sustained critiques of orthodox writing practices, institutionalized knowledge production, and the crisis of representation, they were eager to locate engaging, creative, and useful alternatives to the existing models of research. Inevitably, they were drawn toward a radical democratization of the research process—an intention to minimize the power differential between researchers and participants (subjects)—one that placed a greater emphasis on activism, social justice, and applied research (Denzin & Lincoln, 2001, 2004; Tedlock, 1991). Ultimately, a new research vocabulary evolved that emphasized terms such as autoethnography (Ellis & Bochner, 2000; Holman Jones, 2005; Holman Jones, Adams, & Ellis, 2013; Spry, 2011); performance ethnography (Alexander, 2005; Denzin,

2003); investigative poetry (Hartnett & Engles, 2005); co-constructed narrative and collaborative autoethnography (Bochner & Ellis, 1992, 1995; Ellis & Bochner, 1992; Ellis & Rawicki, 2013), appreciative and action research (Greenwood & Levin, 2005), feminist praxis (Dillard & Okpalaoka, 2011), transformative research for social justice (Mertens, Sullivan, & Stace, 2011), performance, and lived experience—each rooted in some extent to a turn toward narrative.

In retrospect, then, the turn toward narrative inquiry and qualitative research in the human sciences appears to have been a consequence of intellectual, social, and cultural changes—most notably the crisis in representation; greater access to previously marginalized minority populations who, in turn, championed the need to give voice to silenced narratives and marginalized groups and communities; and a growing commitment to use research to make a difference personally, emotionally, politically, and culturally. Initially reactive, the turn toward narrative became proactive. Social scientists drawn to narrative inquiry now are pursuing constructive responses to the agitating critiques of realism, modernism, and the correspondence theory of language. On the whole, they view these critiques not as an end but as a beginning, not as a reason for despair but as a cause for hope, not as a curtain closing on the excesses and illusions of the past, but as a door opening to a future that is ripe with possibilities and promise. As Gergen (1999) advised, we should be careful not to undermine the critical impulse, but, at the same time, we should be inspired by what we have learned from these critiques to emphasize the creation of alternatives. If language is the medium of expression we use to create our reality, then we need to investigate what we can do with language to create the kind of realities in which we want to live.

In light of the cultural, philosophical, and epistemic context in which the turn toward narrative inquiry originated—the desire for a more human- and justice-focused social science and the rejection of the correspondence theory of truth—researchers championing an interpretive and narrative orientation for the human sciences substantially altered how they understood and construed the research process, particularly their relational, ethical, and procedural obligations to the people they studied. Although not all narrative inquiry in the human sciences embodies this understanding of the research process, many of those who took the turn toward narrative and turned away from realist, positivist,

and modernist social science subscribe to the ideal of a reflexive, relational, dialogic, and collaborative process grounded in the following eight precepts of distinctively interpretive social science:

1. The researcher is part of the research data.
2. A social science text always is composed by a particular somebody someplace; writing and/or performing research is part of the inquiry.
3. Research involves the emotionality and subjectivity of both researchers and participants.
4. The relationship between researchers and research participants should be democratic.
5. Researchers ought to accept an ethical obligation to give something important back to the people they study and write about.
6. What researchers write should be written for participants as much as about them, researchers and participants should be accountable to each other, the researcher's voice should not dominate the voices of participants.
7. Research should be about what could be (not just about what has been).
8. The reader or audience should be conceived as a co-participant, not as a spectator, and should be given opportunities to think with (not just about) the research story (or findings).

Thus, the goals of much of narrative inquiry are to keep conversation going (about matters crucial to living well); to activate subjectivity, feeling, and identification in readers or listeners; to raise consciousness; to promote empathy and social justice; and to encourage activism—in short, to show what it can mean to live a good life and create a just society.

Definitions, Assumptions, and Goals

Due to the immense breadth and volume of work on narrative across the human sciences, the focus of this chapter must be selective. Given the space limitations of a single chapter, we could not possibly do justice to the wide range of historical, critical, cultural, philosophical, literary, rhetorical, cinematic, feminist, psychoanalytic, therapeutic, developmental, discursive, and linguistic studies of narrative, or to the huge corpus of significant works on storytelling within the fields of folklore and oral traditions. Thus, we have chosen to move away from the predominantly textual, structural, and semiotic concerns of those who focus primarily on narrative production (most notably literary, discursive, and/or linguistic works classified under the rubric *narratology*, where narrative is an end in itself) and

toward a focus on storytelling as a *communicative activity*, where the emphasis is on how humans use language to endow experience with meanings. Consequently, we will emphasize the “narrative fabric of the self,” what psychologist Mark Freeman (1998a, p. 461) has called “the poetic dimension of narrative,” reflecting each person’s struggle to make language adequate to experience, including the experience of one’s self.

Many scholars and practitioners of narrative across the human sciences are deeply immersed in and intrigued by what is called the “narrative identity thesis”—the question of how stories shape and can reshape a person’s identity. Narrative identity research focuses on the stories people tell about themselves either in mundane, everyday interactions—*small stories*—or in retrospective accounts ranging from episodic stories about epiphanies or personal troubles to full-blown life histories—*big stories*. Researchers seek to understand how people look back on their lives and how they have coped in the past with the contingencies, difficulties, and challenges of lived experience, as well as how their identities are made communicatively, through everyday interactions with others. These stories may be told within the context of a particular relationship, such as first-person accounts of a friendship or marriage; outside the relationship in the context of a research interview, conversation or dialogue; or as part of a researcher’s extended participation in a community. Although Strawson’s (2004) depiction of the narrative identity thesis as an intellectual fashion and more likely “an affliction . . . than a prerequisite for a good life” (p. 50) has stirred considerable attention in recent years, we concur with Battersby’s (2006) assessment that Strawson’s argument is riddled with unsupported assertions, poorly defined and imprecise concepts, and the lack of an alternative perspective on the relationship between self and narrative, and with Eakin’s (2008) observation that “we are embedded in a narrative identity system whether we like it or not” (p. 16). Thus, in light of the space available to us in this chapter, we are not inclined to give attention to this particular assault on the narrative identity thesis. Still, Strawson (2004) has contributed some fresh questions for debate and discussion. Readers interested in a detailed dialogue with the anti-narrative identity thesis should consult the collection of essays edited by Hutto (2007).

In this chapter, we assume that stories are social performances at least insofar as they involve a teller and an audience—the husband or wife, the friend,

the partner, the administrator, the survivor, the researcher, and the like. Normally, the stories people tell follow certain conventions of storytelling; that is, most stories contain similar elements and follow similar patterns of development. These include:

1. People depicted as *characters* in the story
2. A *scene, place, or context* in which the story occurs
3. An *epiphany* or crisis of some sort that provides *dramatic tension*, around which the emplotted events depicted in the story revolve and toward which a resolution and/or explanation is pointed
4. A *temporal ordering* of events
5. A point or *moral to the story* that provides an explanation and gives meaning and value to the experiences depicted

Storytellers portray the people in their stories, including themselves, as characters: protagonists, antagonists, heroes, victim, or survivors. Usually, the stories they tell revolve around an epiphany or dramatic event. The events take place somewhere, sometime—in a scene that can provide context and give setting, framing, and texturing to the story. The point or goal of the story is to come to terms with, explain, or understand the event(s): Why did this happen to me? How can I understand what these experiences mean? What lessons have I learned? How have I been changed?

The events depicted in a story occur over time. Most—although by no means all—personal stories are told in an order that follows linear, chronological time, giving the sense of a beginning, middle, and ending. The endpoint is particularly important not only because it represents the goal toward which the events or actions are pointed, and thus gives the story its capacity for drama and closure, but also because it is imbued with value—there is a moral to the story. “Could we ever narrativize without moralizing?” asks Hayden White (1980, p. 27), a question answered by MacIntyre’s (1981, p. 456) insistence that: “Narrative requires an evaluative framework in which good and bad character helps to produce unfortunate or happy outcomes.”

When people tell stories, they interpret and give meaning to the experiences depicted in their stories. The act of telling is always a performance, a process of interpretation and communication in which the teller and listener collaborate in sense-making. After all, meaning does not exist independent of or prior to the interpretation of experience. In other words, experience is not the same as story. Indeed,

the burden of the academic storyteller is to *find the story in the experience* (Stone, 1988) and to try to make it the experience of those who listen to the story. Storytelling attaches meanings to experiences. In the process of interpreting experiences through storytelling, people activate subjectivity, emotionality, and available frames of narrative intelligibility. Once told, the storied experiences become constitutive of the storyteller's life. *The story not only depicts life, it also shapes it reflexively*. Stories are in a continual process of production, open to editing, revision, and transformation (Ellis, 2009). As Rosenwald (1992, p. 275) observed, "Not only does the past live in the present, but it also appears different at every new turn we take."

Narratives lived, told, and anticipated occur in a cultural context and are influenced by canonical stories circulating in everyday life. Often, the frames of intelligibility that function as narrative resources are canonical and cultural stories. But people are not condemned to live out the stories passed on through cultural productions such as cinema, television, music, and other forms of popular communication or through traditions passed on and/or promoted by cultural institutions such as families, schools, synagogues, or churches. If our stories never thwarted or contested received and canonical ones, we would have no expectation of change, no account of conflict, no demand to account for our actions, and no sense of agency. Evidently, humans have a dazzling capacity to reform or reframe the meanings of their actions through stories. As Rosenwald (1992) points out, there is always an uncomfortable tension between restless desire and stabilizing conventions.

In narrative inquiry, researchers must stay wary of the temptation to treat the stories people tell as "maps," "mirrors," or "reflections" of the experiences they depict. Instead, stories should be recognized as fluid, co-constructed, meaning-centered reproductions and performances of experience achieved in the context of relationships and subject to negotiable frames of intelligibility and the desire for continuity and coherence over time. Usually, storytellers have options and alternatives (Carr, 1986). Over the course of our lives, we reframe, revise, remake, retell, and relive our stories (Ellis, 2009).

Often, narrative inquiry functions as a mode of research that invites readers to think *with* stories (Frank, 1995). Readers are invited to enter into dialogue with narratives that depict the difficult choices about how to act that we all face over the course of our lives and to contemplate the possibilities

and limitations we encounter when we attempt to become authors of our own stories.

Stretching What We Mean by Stories

The question, "what is a story?" has been talked about endlessly (Myerhoff, 2007). Most narrativists insist on beginnings, middles, and endings, but LeGuin (1989) extends the definition of a story by pointing to a runic inscription, translated as "Tolfink was here," carved into a stone located in a twelfth-century church in Wales. In the spirit of Primo Levi (1989) and Virginia Woolf (1976), LeGuin (1989) highlights Tolfink's refusal to dissolve into his surroundings. Tolfink "was a reliable narrator," LeGuin claims (p. 29), because his carving bears witness to existence—that someone was there—as well as to the brevity of life (Myerhoff, 2007). Thus, one useful way of understanding the motivating urge and desired consequences of acts of storytelling is as a primordial, existential form of bearing witness to human being and human suffering—an effort to claim or reclaim one's humanity.

Of course, not all stories deal with the existential epics, twists of fate, dilemmas, or dramas of finite human experience or with the painful contradictions of a symbolic identity joined to an imperfect and limited body (Becker, 1973). Crites (1971, p. 296) emphasized how humans live "from the sublime to the ridiculous," noting that our life experiences range from the sacred to the mundane "and the mundane stories are also among the most important means by which people articulate and clarify their sense of the world" (Crites, 1971, p. 296).

A somewhat different conception of the ridiculous, one that nevertheless attempts to turn greater attention to the realm of the mundane, has been advanced by Bamberg (2007), who laments the disproportionate emphasis placed on "big" as opposed to "small" stories. Bemoaning the neglect of everyday, interpersonal interactions—the *real* stories of our lives (Bamberg, 2004)—through which identity is negotiated, Bamberg (2004, 2006), Georgakopoulou and Goustos (2004) and Georgakopoulou (2006, 2006a, 2007) have exhorted researchers to concentrate on small stories. Although small stories are "not particularly interesting or tellable" and "not even necessarily recognized as stories" (Bamberg, 2006, p. 63), researchers focusing on small stories want to rectify what they interpret as the privileged and quasi-ontological status of big stories in narrative inquiry. The term "small stories" refers to "the smallness of talk, where fleeting moments of narrative orientation to the world (Hymes, 1996) can be easily

missed out by an analytical lens which only looks out for fully-fledged stories” (Georgakopoulou, 2007, p. 146). Narrative gets “taken down to size” (Freeman, 2007, p. 156) in research on small stories as investigators attempt to show how identity is constructed interpersonally, closer to the action of everyday life, and how images of the self are “thoroughly moored in social life” (p. 156).

In our opinion, the tensions between advocates of big and small stories are unfortunate and potentially obstructive. As Freeman (2006, p. 132) observed, “There is plenty of meaning to go around,” and it is not a question of which type of story is truer to life. Big and small stories simply represent “different regions of life,” and neither can provide privileged access to truth (Freeman, 2006, p. 137).

Human beings are relational beings (Gergen, 2009) whose identities rest on relationships with others. We are bound up with others, and our understanding of ourselves rests on our connections to others, whether casual or intimate. Thus, the question of how identity is made in interpersonal interaction deserves serious and concentrated attention. Small-story advocates, however, should not need to take an oppositional stance toward big-story inquiry in order to justify or defend their concern for how identity work is accomplished. The mundane and the sacred stand side by side; they do not compete with each other. They can best be conceived, in our opinion, as preferences for taking certain points of view toward our subject matter—narrative. In Rorty’s (1982) words, these different views are “not issue(s) to be resolved, only... differences to be lived with” (p. 197).

Still, we think it may be necessary for small-story researchers to address the grounds on which one can conclude that identity is a narrative achievement, as well as what kind of identity work we are talking about (Eakin, 2008; Neisser, 1988). Scholars of small stories want us to stretch the meaning of story to accommodate their conception of storying as an interactional activity through which identities are created and negotiated. But by extending the idea of a story in this fashion, these researchers beg the question of whether a process referred to as “storying” ought to produce something akin to a story replete with many of the elements we ordinarily associate with narrative—plot, character, scene, an ethical standpoint subject to evaluation, or the kind of bearing witness to which LeGuin (1989) referred to in her discussion of Tolstoy’s carving. No doubt, interactants in these small story studies are organizing and negotiating the meanings of experience and

co-constructing reality, but should the process of communication by which their identities are made and/or changed be called storying? Is there a point at which an utterance or set of utterances can be too small or devoid of the elements of narrative reasonably to be called a story? Are the interactants themselves assuming a position akin to what Arthur Frank (2010) called “the standpoint of the storyteller?” Bamberg (2006a) has referred to some of his own interactional examples of identity in the process of being made as “story-like,” which evokes a question about how much like a story an utterance or a set of utterances needs to be for us to consider it or them a story.

Genres of Narrative Inquiry

One way of sorting the different agendas of narrative inquiry is to make distinctions between different types of narrative research. For example, Polkinghorne (1995) differentiated two distinct types of narrative inquiry that correspond to Bruner’s (1986) distinction between paradigmatic and narrative reasoning. In Polkinghorne’s (1995) schema, *analysis of narratives* refers to storytelling projects that are grounded on pragmatic reasoning. These projects treat stories as “data” and use “analysis” to arrive at *themes* that hold across stories or on delineating types of stories and/or storylines. Grounded theory (Charmaz, 2000, 2005; Glaser & Strauss, 1967), in which researchers work inductively from the ground of the stories upward and present the analysis in the form of a traditional social science report, is one method commonly used to analyze narratives. Later in this chapter, we will provide a more detailed discussion of various approaches to the analysis of narrative, including modes of conversation and discourse analysis akin to the small-story orientation of Bamberg (2006a, 2007) and Georgakopoulou (2006, 2006a, 2007).

In Polkinghorne’s (1995) second type of narrative inquiry, which he calls *narrative analysis*, the research product is a story—a case, a biography, a life history, an autobiography, an autoethnography—that is composed by the researcher to represent the events, characters, and issues that he or she has studied. Polkinghorne (1995) clarifies the differences between the products of an analysis of narrative and a narrative analysis. Whereas an analysis of narrative(s) ends in abstractions, such as a set of themes, narrative analysis takes the form of a story. Unfortunately, this distinction can be confusing. For example, Riessman (1993) has written a methodological primer titled *Narrative Analysis*, but,

within Polkinghorne's (1995) typology, the kinds of narrative inquiry on which she focuses would fall under analysis of narrative not narrative analysis.

Beginning in graduate school, most social scientists are taught that research projects aren't completed until the dots have been connected. Thus, it should come as no surprise that a great deal of narrative inquiry focuses on identifying themes and/or storylines. But the themes of a story don't necessarily tell us what the story does, how it works, what relationships it shapes or animates, or how it pulls people together or breaks them apart. Moreover, narratives are typically analyzed from the perspective of the analyst, who often holds preconceived notions or hypotheses based on previous research literature about what he or she is likely to find (or discover) in the stories being studied. Stated simply, the standpoint of the analyst will be different from the standpoint of the storyteller, and these differing standpoints affect how the listener/researcher will hear, understand, and interpret the story.

Narrative inquiry is confronted by the troubling fact that what a story means to an analyst may be quite different from what a story means to the storyteller. Often, the storyteller wants a listener/analyst/researcher to "get into" his or her story, whereas a story analyst, especially a researcher, may be centrally interested in what he or she can "get out" or "take away" from a story (Greenspan, 1998). We see a world of difference between treating stories as "data" for analysis—thus privileging the standpoint of the analyst—and encountering stories experientially—thus privileging the standpoint of the storyteller. In the former case, how a story makes sense is strictly a scientific/analytic question; in the latter case, it's an ethical and relational one. In the former instance, the researcher wants to go beyond the story, to think about it and use it for the sake of advancing sociology, psychology, or communication theory; in the latter instance, storytelling is a means of being with others, of thinking with their stories in order to understand and care for them.

Paul Atkinson (2006, 2010) represents the hyperorthodox camp of narrative inquiry, which sees no alternative but to subject stories to rigorous and methodical analysis. Indeed, he condemns any form of narrative inquiry that enters into a story "appreciatively" and from the standpoint of the storyteller. But as Arthur Frank (2010, p. 5), a self-proclaimed "narrative exceptionalist," points out, reluctance to take the standpoint of the storyteller risks failing to "recognize why the story matters deeply to the person telling it" (Frank, 2010,

p. 6). Frank's observation coincides with Denzin's (1997) insistence that the living dialogue inspired by appreciative narrative inquiry needs to be set off from traditional empiricist approaches to the analysis of narratives. Following Trinh (1989, p. 141), Denzin (1997) opposes the inclination to turn a story told into a story analyzed because, in effect, the meaning of the story is sacrificed at the altar of methodological rigor. Then we lose what makes a story a story: "They (the analysts) only hear and read the story from within a set of predetermined structural categories. They do not hear the story as it was told" (Denzin, 1997, p. 249).

Ordinarily, we understand or identify with characters in a story through a plot that ties together what happens and invites readers or listeners to evaluate the meanings of the actors' actions and decisions. Here is the place where we enter an ethical dimension in which narratives invite evaluations of "goodness" and "character," evoking reflections, evaluations, and reactions and calling up concerns about such things as "faithfulness," "thoughtfulness," and "responsiveness." Often, we find ourselves evaluating or coming to terms with the degree to which characters have participated with and for others (Ricoeur, 1992).

Dwelling in the moral space of narrative introduces an ethical standard that could be applied—we think should be applied—to the ways in which researchers relate and respond to the stories and storytellers they behold as well. To enact this ethical stance would alter what it means to be rigorous or to conduct methodical analysis. Frank (2010) insists that analysts need to be answerable to the storytellers whose stories they elicit and/or witness. Yet, the kind of analysis favored by hyperorthodox empiricists often takes the form of reductionism and thus "reduces stories to inert material devoid of spirit" (Frank, 2010, p. 6) and indifferent to the storyteller's inspirations and interpretations.

Ironically, the so-called methodical research practices denounced by Frank (2010) are some of the same ones condemned during the crisis of representation that initially inspired the narrative turn. By treating narratives as unexceptional and narrative inquiry as no different from any other kind of social science inquiry, hyperorthodox narrativists implicitly dispute the very moral, ethical, political, and ideological grounds on which the narrative turn rests. Narrative exceptionalists, on the other hand, embrace drastically different views of objectivity and rigor, as well as what it means to be methodical. They see their work as itself a form of storytelling

and they seek to talk *to*, talk *with*, and *inquire as empathic witnesses on behalf of* their research participants. They choose not to color over what they hear with concepts organized into systems of thought of interest to social scientists but of little relevance to participants themselves. By taking the standpoint of the storyteller, they promote a social science of caring and community, an engaged and passionate social science that requires researchers to develop caring relationships with the people they study instead of standing apart from them in the name of objectivity, rigor, and science (Bochner, 2010). The narrative exceptionalists eschew the technologies of disengaged reason and seek instead a social science of narrative inquiry in which researchers open their hearts as well as their minds and listen attentively to stories that feel raw, cut deep, and resist distance and abstraction (Bochner, 2010).

The distinction we have drawn between narrative exceptionalists and hyperorthodox narrative analysts may simply reflect the differences between those who situate research on storied lives within a poetic, embodied, ethical, existential, and ontologically driven ideal of narrative inquiry and those who still cling to the ideals of scientific knowledge as something to be possessed, ordered, and organized into determinate systems of mastery and control.

In the next two sections of this chapter, we divide narrative inquiry into work that takes the stance of the storyteller and work that takes the stance of the story analyst. We begin by sketching the development of several strands of narrative inquiry that fall within the rubric of what we consider the standpoint of the storyteller.

Personal Narratives: Putting Meanings into Motion

After Arthur Frank (2000, 2010), we use the term “standpoint of the storyteller” to refer to personal narratives in which “the language of science merges with the aesthetics of art” (Benson, 1993, p. xi). Although many types of life writing fall within this broad category—illness narratives, autobiographies, memoirs, and so on—we are concerned principally with works published by academics, especially first-person accounts, autoethnographies, self-narratives, performative narratives, and narrative ethnographies. These research stories are a genre of “artful science” (Brady, 1991) insofar as they apply the imaginative power of literary, dramatic, and poetic forms to create the effect of reality, a convincing likeness to life as it is sensed, felt, and lived. As a form of expressive and dialogic

inquiry, these stories break away from the traditional forms of mainstream, representational social science. The focus is less about “knowing” and more about living; less about controlling and more about caring; less about reaching immutable truths and more about opening dialogues among different points of view; less about resolving differences and more about learning how to live with them; less about covering life experience with disembodied concepts and more about finding ways to personify the “untamed wilderness” of lived experience (see Jackson, 1995).

Instead of going beyond, searching beneath, or edging behind—as Jackson (1995, p. 163) says, “putting reality on the rack until it reveals objective truth”—social scientists drawn to this kind of artful, poetic social science want their work to produce “experiences of the experience” (Bochner & Ellis, 1992; Ellis & Bochner, 1992). They want their readers to enter the experience of others, usually as empathic witnesses. By putting themselves in the place of others, readers or listeners are positioned to reflect critically on their own experience, to expand their social capabilities, and to deepen their commitment to social justice and caring relationships with others. The goal of this kind of evocative storytelling, which Richardson (2000) referred to as “creative analytic practices,” is to put meanings into motion, showing how people cope with exceptional, difficult, and transforming crises in their lives, how they invent new ways of speaking when old ways fail them, how they make the absurd sensible and the disastrous manageable, and how they turn calamities into gifts.

The corpus of narrative inquiry to which we are referring offers a distinctive alternative to traditional canons of research practices in the social sciences. These stories seek to activate subjectivity and compel emotional responses from readers; they long to be used rather than analyzed, to be told and retold rather than theorized and settled, to offer lessons for further conversation rather than truths without any rivals, and they promise the companionship of intimate detail as a substitute for the loneliness of abstracted facts. Evocative research stories not only breach ordinary and canonical inscriptions about living, but also challenge traditional norms of writing and research, encouraging social scientists to reconsider the goals of research and the conventions of academic writing, as well as to question the venerable divisions between Snow’s conception (1959) of two cultures of inquiry that segregate literature from social science.

The narrative turn marked a shift toward a more personal social science, one that already was

proliferating in the mainstream press, new journalism, creative nonfiction, literary memoir, autobiography, and autopathography (Buford, 1996; Harrington, 1997; Hawkins, 1993; Parini, 1998; Stone, 1997). Most of the genres of life writing (see, e.g., Tierney, 2000) were shifting toward more intimate, personal, and self-conscious writing. At about the same time, social science researchers began to embrace less anonymous, more personal styles of writing that paralleled the focus on personal writing genres in literature, nonfiction, and journalism. Among the abundant examples of this movement within the social sciences were special issues of journals such as the *Journal of Contemporary Ethnography* (Ellis & Bochner, 1996a), *Qualitative Sociology* (Glassner, 1997; Hertz, 1997; Zussman, 1996), and *Communication Theory* (Geist, 1999); the book series *Ethnographic Alternatives* edited by Ellis and Bochner (Angrosino, 1998; Banks & Banks, 1998; Bochner & Ellis, 2001; Brady, 2002; Drew, 2001; Ellis & Bochner, 1996; Goodall, 2000; Gray & Sinding, 2003; Holman Jones, 1998, 2007; Lagerway, 1998; Lockford, 2004; Markham, 1998; Pelias, 2004; Richardson & Lockridge, 2004; Tillmann-Healy, 2001; Trujillo, 2004); and a subsequent one edited by Bochner and Ellis (Adams, 2011; Charles, 2007; Ellis, 2009; Frentz, 2008; Goodall, 2008; Nettles, 2008; Pelias, 2011; Poulos, 2008; Richardson, 2007; Rushing, 2005; Tamas, 2011); the edited collections by anthropologists (Benson, 1993; Brady, 1991; Okely & Callaway, 1992); sociologists (Ellis & Flaherty, 1992; Hertz, 1997; Zola, 1982), communication researchers (Banks & Banks, 1998; Ellis & Bochner, 1996; Perry & Geist, 1997), psychologists (Lieblich & Josselson, 1997), and educators (Hertz, 1997; Tierney & Lincoln, 1997); and the numerous articles, forums, and monographs (e.g., McLaughlin & Tierney, 1993) featured in academic journals and annuals such as *American Anthropologist*, *Anthropology and Humanism Quarterly*, *Auto/Biography*, *Feminist Studies*, *Journal of Loss and Trauma*, *Life Writing*, *Narrative*, *Narrative Inquiry*, *The Narrative Study of Lives*, *Narrative Inquiry*, *Qualitative Inquiry*, *Qualitative Communication Research*, *Sociology of Sport Journal*, *Sociological Quarterly*, *Studies in Symbolic Interaction*, *Symbolic Interaction*, *Text and Performance Quarterly*, and *Women's International Quarterly*.

We can identify five distinguishing features of this type of personal narrative inquiry. First, the author usually writes in the first person, making her- or himself the object of research (Tedlock, 1991), thus transgressing the conventional separation of researcher and subject (Jackson, 1989). Second, the narrative breaches the traditional focus on generalization

across cases by focusing on generalization within a single case extended over time (Geertz, 1973). Third, the text is presented as a story replete with a narrator, characterization, and plotline, akin to forms of writing associated with the novel or biography and thus fractures the boundaries that traditionally separate social science from literature. Fourth, the story often discloses hidden details of private life and highlights emotional experience and thus challenges the rational actor model of social performance that dominates social science. And fifth, the ebb and flow of relationship experience is depicted in an episodic form that dramatizes the motion of connected lives across the curve of time (Weinstein, 1988) and thus resists the standard practice of portraying a relationship as a snapshot (Ellis, 1993).

Academic storytellers who adopt the stance of the storyteller hold a distinctly different understanding of the work they want narrative inquiry to do. They don't see a split between theory and story but rather understand the aim of stories as putting meanings into motion (Bochner, 2012a). They reject the received traditions of empiricism in favor of a relational, dialogic, qualitative, and collaborative conception of inquiry (Gergen & Gergen, 2012). They are less concerned about representation and more concerned about communication. Giving up the illusion of transcendental observation, they seek to make narrative inquiry a source of connection, contact, and relationship between tellers and listeners by eliciting conversation and deliberation about the personal, political, moral, and institutional values associated with lived experience. They see stories as the fundamental human medium of being, knowing, and participating in a social world. As an academic practice, evocative narrative inquiry thus shifts the meaning of the activity of theorizing from a process of *thinking about* to one of *thinking with* (Frank, 1995, 2004). Theory merges with story when we invite others to think with a story rather than about it (Bochner, 1997, 2010). As listeners or readers, we are not asked merely to receive the story or analyze it from a distance, but rather to encounter it, get into it, and engage with it, using all the senses available to us (Stoller, 1989). As Frank (1995, p. 23) observed: "To think about a story is to reduce it to content and then analyze the content... to think with a story is to experience it affecting one's own life and to find in that effect a certain truth of one's own life."

The point of an evocative personal narrative is not to turn the story into "data" in order to test or verify theoretical propositions and thereby produce

knowledge that can be received by others. Instead, the objective is to link theory to story by inviting others to think and feel with the story, staying with it, resonating with the story's moral dilemmas, identifying with its ambiguities, examining its contradictions, feeling its nuances, letting the story analyze them (Frank, 2004). We think with a story from within the framework of our own lives. We ask what kind of person we are becoming when we take a story to heart and consider how we can use it for our own purposes, what ethical direction it points us toward, and what moral commitments it calls out in us (Coles, 1989).

Forms of evocative narrative writing and performative social science (Gergen & Gergen, 2012; Gray & Sinding, 2003) seek a personal connection between writer/performer and reader/audience. The stories invite others to think *and* to feel. To achieve this goal, a writer/researcher must depart the safe and comfortable space of conventional academic writing. Unfortunately, the conventions that regulate (and discipline) academic writing do not encourage forms of communicating research that can build a personal connection between the text and the reader/audience member. Normally, we don't expect academic texts to make our hearts skip a beat (Bochner, 2012; Hyde, 2010). But if our research has something to do with human longing, desire, fulfillment, pleasure, pain, loss, grief, or joy, shouldn't we hold authors to some standard of vulnerability? Can our work achieve personal importance—can it matter—if the authors aren't willing to show their faces? Shouldn't one of the standards by which social science inquiry is judged be the extent to which readers feel the truth of our research stories?

Seeking to open a space for this kind of personal narrative inquiry, Ellis and Bochner (1996) developed a project they called “ethnographic alternatives” (Bochner & Ellis, 2002; Ellis & Bochner, 1996). They took the poststructuralist critique to mean that social science writing could be usefully conceived as a material intervention into people's lives, one that not only represents but also creates experience, putting meanings in motion. They believed that research texts, whether first-person accounts or more traditional ethnographic storytelling, could be understood as “acts of meaning” and, as Bruner (1990) suggested, that's precisely the work of storytelling. Wanting to create a space in which social science texts could be viewed as stories and their authors—the researchers—as storytellers, Ellis and Bochner (1996) invited scholars to experiment with various forms of personal, emotional,

and embodied narration that depart radically from the conventions of rational/analytic social science reporting. If we experience our lives as stories, they asked, then why not represent them as stories? Why shouldn't social scientists represent life as temporally unfolding narratives and researchers as a vital part of the action? Their ethnographic alternatives project offered stories that showed the struggles of ordinary people coping with difficult contingencies of lived experience—brimming with characters, scenes, plots, and dialogue—stories that enabled readers to *put themselves in the place of others* (Jackson, 1995) and consider important aspects of their own lives in the terms offered by the contexts and details of other peoples' stories, such as how lived experience is riddled with contingencies that concede the incomplete and unfinished qualities of human relationships (e.g., Bochner & Ellis, 1992; Bochner, Ellis, & Tillmann-Healy, 1998, 2000; Ellis, 1996; Ronai, 1996; Tillmann-Healy, 1996).

Both *Ethnographic Alternatives* and the book series project that followed, *Writing Lives: Ethnographic Narratives*, problematized the conventions of writing in the social sciences. As scholars, we realize that there is no alternative but to turn life into language. But there is more than one way to do this. Traditional social science writing favors the types of events and “data” that are amenable to conceptual analysis and theoretical explanation. Ambiguous, vague, and contingent experiences that cannot so easily be covered by concepts or organized into a coherent system of thought are bypassed in favor of experiences that can be controlled and explained (Bruner, 1990). Immediate experience is grist for the theoretical mill. Moreover, distancing oneself from the subject matter, like a spectator at a sporting event, is taken as an appropriate and normative model of research and writing practices. Thus, social science texts usually are written in a third-person, objectifying, neutral, and scientific voice. Although contradictions, emotions, and subjectivities may be recognized as concrete lived experiences, they usually are expressed in forms of writing that dissolve concrete events in solutions of abstract analysis. The reader is left to look through a stained glass window, to use Edith Turner's (1993) apt analogy, seeing only murky and featureless profiles. The concrete details of sensual, emotional, and embodied experience are replaced by typologies and abstractions that remove events from their context, distancing readers from the actions and feelings of particular human beings engaged in the joint action of evolving relationships. Readers are not encouraged to see

and feel the struggles and emotions of the participants and thus are deprived of an opportunity to care about the particular people whose struggles nourish the researcher's hunger for truth. It is not hard to figure out why orthodox social science writing is not widely read. What is the appeal of an inaccessible, dry, and overly abstract text?

On the whole, social science research articles and monographs are confined mainly to what LeGuin (1986) refers to as "the father tongue," a high-minded mode of expression that embraces objectivity. Spoken from above, the father tongue runs the risk of distancing the writer from the reader, creating a gap between self and other. What is missing from most social science writing is "the mother tongue" (LeGuin, 1986), a binding form of subjective and conversational expression that covets "a turning together," a relationship between author and reader. Voiced in a language of emotions and personal experience, the mother tongue exposes rather than protects the speaker through a medium that can bring author and reader closer together. The absence of a mother tongue in social science literature reflects the conventions of disembodied writing that extol the virtue of objectivity. As LeGuin (1989, p. 151) notes, "People crave objectivity because to be subjective is to be embodied, to be a body, vulnerable, violable." The real discourse of reason, she claims, is a wedding of the father to mother tongue, which produces "a native tongue." When this fusion of voices occurs, which is rare indeed, it's a beautiful thing (Eastman, 2007).

Evocative narratives work the hyphen between the mother tongue and the native tongue. Unlike orthodox social scientists, those who assume the standpoint of the storyteller see themselves first and foremost as writers and communicators, not as reporters or conduits for channeling data from a source to a receiver. For these scholars, writing and/or performing stories is an interpretive practice; it's their method for discovering, ordering, and communicating what they've experienced and what it can mean to and for others. They are committed to being rigorously empirical, but they don't take that conviction as an end in itself. Instead, they apply it in tandem with an obligation to make their prose accessible, readable, and sensuous. Moreover, they don't want to limit what they write about to what can be ordered into determinate, disembodied systems of knowledge because that leaves out the indeterminate, the ambiguous, the embodied, and the contradictory realms of experience in which so much of life is lived—the shadowy, painful, or

fateful moments on which our lives turn one way or another, one direction or another.

Like most social science inquiry, the kind of social science writing that takes the standpoint of the storyteller aspires to truth, but the kind of truths to which it aspires are not literal truths; they're emotional, dialogic, and collaborative truths. Not Truth but truth; not truth but truths. The truths of these stories exist between storyteller and story listener; they dwell in the listeners' or readers' engagement with the writer's struggle with adversity, the heart-breaking feelings of stigma and marginalization, the resistance to the authority of canonical discourses, the therapeutic desire to face up to the challenges of life and to emerge with greater self-knowledge, the opposition to the repression of the body, the difficulty of finding words to make bodily dysfunction meaningful, the desire for self-expression, and the urge to speak to and assist a community of fellow sufferers. The call of these stories is for engagement within and between, not analysis from outside and at a distance (Bochner, 2014).

This is not to say, as some critics mistakenly imply (Atkinson, 2010), that writers who take the standpoint of the storyteller fail to live up to some abstract responsibility of social science called "analysis." Reflection is the heart of personal narrative and autoethnography. As Vivian Gornick (2008) observed, "It is the depth of reflection that makes or breaks it." The plot of these stories usually revolves around trouble, presenting feelings and decisions that need to be clarified and understood. The stories function as inquiry; something is being inquired into, interpreted, made sense of, and judged. Facts are important to these academic storytellers; they can and should be verified. But it is not the transmission of facts that gives the autoethnographic story or personal narrative its significance and evocative power. Facts don't tell you what they mean or how they feel. The burden of the social science storyteller is to make meaning out of all the stuff of memory and experience; how it felt then and how it feels now. That's why the truths of stories can never be stable truths (Bochner, 2007). Memory is active, dynamic, and ever changing. As we grow older and/or change our perspective, our relationship to the events and people of the past changes too (Hampel, 1999). The past is always open to revision and so, too, are our stories of them and what they mean now (Ellis, 2009).

Narratives-Under-Analysis: Research Practices

Narratives-under-analysis refers to the analysis of narrative as story-form, what Riessman (2008)

calls “the systematic study of narrative data” (p. 6). We prefer the term, narratives-under-analysis to the misleading term “narrative analysis” because it better represents the forms in which this kind of narrative inquiry typically are expressed. Most scholars who use the term “narrative analysis” to describe their work do not analyze narratively. They do not produce analyses in a storied form. Rather, they abide by and adhere to the conventions of academic prose and procedural (scientific) objectivity. Treating narratives as objects to be deconstructed, they prefer to keep a comfortable distance (Atkinson & Delamont, 2005) between themselves and the storytellers whose stories they place under their microscopes. Transforming stories, whether big or small, into data amenable to conceptual analysis and theoretical explanation, these researchers usually resist the temptation to ask tellers what they think they were doing or meaning, choosing instead to focus on their own inferences and interpretations—grounded in conventional practices of sociolinguistic and discourse analysis of what is said or told (producing themes or topics), how the telling is organized (its structure), how it is performed, and/or how it functions intersubjectively. As analysts, these researchers normally get the first and the last word. Stories are wrestled from the sensual, emotional, and embodied contexts of the storytellers’ lives and turned into texts that can be served up to the analyst’s interests in producing snippets of talk that document types or genres of speech acts or conversational maneuvers. Stories are subjected first to interpretive practices of transcription, then to further interpretive practices of one form or another aimed at grounded clarification of the meaning of the texts and their interactive production. To most researchers who place narratives-under-analysis, stories are no different from any other kind of data to which rigorous qualitative and/or quantitative methods can be applied (Atkinson, 2010).

There are a broad array of questions and issues to which narratives-under-analysis have been applied. According to Lieblich, Tuval-Mashiach, and Zibler (1998), a study that analyzes narrative works with data that have been “collected as a story (a life story provided in an interview or literary work) or in a different manner (field notes of an anthropologist who writes up his or her observations as a narrative or in personal letters). It can be the object of the research or a means for the study of another question. It may be for comparison among groups, to learn about a social phenomenon or historical period, or to explore a personality” (pp. 2–3). Or, narratives-under-analysis can

function as a means of exploring “how people weave tapestries of story” in order to “reveal the extent to which human intelligence itself is rooted in narrative ways of knowing, interacting, and communicating” (Herman, 2009, p. 9).

One of the primary ways in which human beings come to understand themselves and the world in which they live is by making meanings in storied forms. Thus, many narrative analysts view their work as an expression of human reasoning and meaning construction—“the principle way that human beings order their experience in time” and “make coherent sense out of seemingly unrelated sequences of events” (Worth, 2008, p. 42). Bamberg (2007) stresses that “narrative analysis is less interested in a narrator who is self-reflecting or searching who *s/he* (really) is. Rather, we are interested in narrators who are engaging in the activity of narrating, that is, the activity of giving an account” (p. 170), which contributes to “a more comprehensive human experience” (Worth, 2008, p. 42) of meaning-making. For these analysts of narrative practices, it is the *how* and *for whom* of narrative telling that is highlighted. Foregrounding the form and content of stories, they seek to understand how personal identity is made in everyday, mundane interaction, which necessitates careful attention to the parameters of storytelling contexts. Thus, the work of narrative-under-analysis involves the process of producing texts for analysis, applying systematic methodological and analytical strategies to examine these texts, and arriving at conclusions about the different forms and strategic moves of storytelling, including inferences about intentions or motives of narrator(s). Whereas evocative narrative takes the standpoint of the storyteller, narratives-under-analysis normatively are governed by an analytical standpoint that positions the researcher as “other” to the storytellers whose texts are to be analyzed.

Models of Analysis

Although most analysts still cling to one version or another of scientific rigor, Herman (2009) situates narrative analysis within a humanistic, poststructural perspective that turns away from modernist and received views of scientific inquiry and thus fits squarely within the narrative turn. Focusing on the performance of narrative or narrativity, narratives-under-analysis should ideally take into account the dispositions of tellers and listeners and pay close attention to the relationship between text and context. Assuming a critical and reflexive stance toward the structuralist tradition

it seeks to transplant, Herman's perspective on narratives-under-analysis seeks research models that can apply across the human sciences (Herman, 1999). Herman (1999) refers to the proliferation of new models as "postclassical narratology" because it transforms previous ways of studying narrative by "not just expos[ing] the limits but also exploit[ing] the possibilities of the older, structuralist models" (p. 3). Thus, new models are situated as a critique of old ones. Still, modernist and structuralist models, such as the ones developed by Labov and Waletzky (1967), Barthes (1975), and Gee (1991), continue to exert a visible influence in narratives-under-analysis literature.

Three models developed in the aftermath of the narrative turn have attracted considerable attention. Riessman's (1993, 2008) model highlights the notion that narratives are ambiguous and incomplete representations of experience and underscores the ways in which researchers are inevitably involved in the production of the narratives they gather or solicit in research. "Meaning is fluid and contextual, not fixed and universal," she writes, and "all we have is talk and texts that represent reality partially, selectively, and imperfectly" (p. 15). Her model of narratives-under-analysis includes five levels of representation in the research process—*attending, telling, transcribing, analyzing, and reading*—and she stresses that "interpreting experience," which happens at all five points in the research process, "involves representing reality; we create and recreate voices over and over again during the research process" (p. 16). Offering sage advice, especially for novices, Riessman (1993) reminds researchers that they are obliged to validate their interpretations; that persuasive writing buttressed by theoretical support and the presentation of alternative interpretations is necessary for showing the salience of analytical findings; that correspondence with participants must be established in order to remain attentive to what distinguishes different subject positions; that researchers should strive for both global (whole story) and local (subjective interpretation) coherence in order to keep analysis anchored in the embedded and emergent logic of narrative data; and that narrative analysis should be aligned with a pragmatic research agenda that avoids canonical approaches to theory and method. In a subsequent book, Riessman (2008) provides a survey of studies that analyzed narratives and a guide for designing interviews to elicit narratives. Together, these publications give useful guidelines for designing and carrying out analyses of narratives.

Herman's (2009) model focuses mainly on the elements and characteristics of prototypical narratives themselves. For Herman (2009), narrative is a unique form of knowledge production and communication. His model attempts to account for the "complex transactions that involve producers of texts or other semiotic artifacts, the texts or artifacts themselves, and interpreters of these narrative productions working to make sense of them in accordance with cultural, institutional, genre-based, and text-specific protocols" (p. 8). He considers narrative not only representational but also relational. Drawing attention to the intersubjective dimensions of narrative, his four-point model—*situatedness, event sequencing, worldmaking/world disruption, and what it's like*—encourages scholars to construe narratives as representations situated in specific discourses. These discourses are ordered along a timeline in ways that introduce disruption or disequilibrium into the story-world conveyed by a narrator's depiction of a particular experience. Beginning with a description of narrative elements, Herman's (2009) model focuses on how people account for their experiences in story-forms and on the story-worlds in which these accounts are embedded and from which interpreters draw meaning.

Relying on their practical and clinical experience as psychologists, Lieblich, Tuval-Mashiach, and Zibler (1998) classified approaches to narrative analysis into four modes—holistic-content, categorical-content, holistic-form, categorical-form—that could serve as a way of organizing narratives-under-analysis research across various social science disciplines. They drew attention to the connections between personal identity and social and cultural structures of meaning, claiming that by "studying and interpreting self-narratives, the researcher can access not only the individual identity and its systems of meaning but also the teller's cultures and social world" (p. 9).

Together, these models provide a conceptual and methodological frame of reference for studies that place narratives under analysis. Still, no widespread agreement exists regarding the conceptual domain for studies using narratives-under-analysis strategies. Mishler (1995) is one of the few researchers to attempt a synthesis and comparative analysis with the goal of establishing a typology. Framing narrative analysis as a "problem-centered area of inquiry" (p. 88) that employs myriad approaches, philosophies, and methods, his typology can be viewed as a meta-model of the burgeoning field of narratives-under-analysis. Mishler (1995) made an

admirable attempt to produce a coherent synthesis of the field that could strengthen ties between theory and method and enable comparisons among studies, although his call for “more inclusive strategies that would provide a more comprehensive and deeper understanding both of how narratives work and the work they do” (Mishler, 1995, p. 117) suggests that considerably more work needs to be done.

Josselson (2007) also advocates attention to meta-analyses of narrative research. She is especially keen on the intersubjective and dialogic qualities of storytelling and storytelling research, expressing the need for developing a knowledge base that allows scholars to “engage those areas of tensions where multiple facets of understanding intersect, interweave, collide, contradict and show themselves in their shifting and often paradoxical relation to each other” (p. 15).

We get the distinct impression that narrative inquiry is on the cusp of evolving as a discipline in its own right. In addition to the work of Josselson (2007) and Mishler (1995), we can point to Cortazzi’s (1993) early review of the different disciplinary contributions to narrative inquiry, which showed the cacophony of approaches to narratives-under-analysis that scholars have taken, and the promise of narrative study for bridging the social sciences and humanities. We anticipate that the urge to achieve something akin to disciplinary status for narrative inquiry will continue to intensify over the coming decade.

Narratives-Under-Analysis Research Practices

We turn next to the practical side of narratives-under-analysis research, emphasizing some of the tensions that researchers confront as they seek to make appropriate and useful decisions about research practices and methodologies. One of the most important practical considerations is *positioning*.

Herman (2009) describes the paradigm that governs a considerable number of projects in which narratives are under analysis and in which researchers focus on “occasions of telling” (Ochs & Capps, 2001): “Interviewers are seeking to obtain as much (vernacular) speech from informants as possible, in contrast with conversation among peers in which participants in the conversation may all be trying to capture the floor at once in order to tell their own version of the story under dispute” (Herman, 2009, p. 35). In light of the researcher’s or interviewer’s potential influence over what stories get told and

how they may get told, it is imperative to recognize how one is positioned as a researcher on any particular occasion of telling.

Riessman (2008) also emphasizes how researchers “play a major part in constituting the narrative data that [they] analyze. Through [their] presence, and by listening and questioning in particular ways, [they] critically shape the stories participants choose to tell” (p. 50). Since different analysts carry out their investigations in different ways and find themselves inserted into the scene or occasion differently in particular research contexts, it is imperative that researchers attend to the ways in which they contextualize and frame the possible subject-positions of the storytellers. Storytellers always tell their stories to somebody in some place and the conversational partners and surrounding environment can influence what gets told or doesn’t, and how. Thus, it is crucial to consider the kind of interpersonal bond that is created between teller and listener. Is the analyst a full, part, or invisible participant in the storytelling interaction? Can the content of the narrative be interpreted as a fluid construction? Or is it more appropriate to interpret the story that is produced as a co-construction?

“Discursive negotiation is at the heart of the matter,” writes Kraus (2007, p. 130). Identifying *what counts* as a storytelling context and *where one stands* as a researcher and/or analyst in relation to the story (and storyteller) involves locating oneself as a somebody somewhere on a spectrum between private and public story-worlds, micro and macro levels of human encounter, and emic and etic orientations to narrative data; that is, deciding whether or not interactional patterns in storytelling can be meaningfully interpreted from within the internal storytelling context or require consideration of external, cultural contexts as well. Recognizing these inherent tensions, Riessman (2008) advises that because “narration . . . depends on expectations” (p. 25), it behooves analysts to establish themselves as action-oriented and falling somewhere between approaches that have an intersubjective slant or maintain subject/object distinction—especially when preparing for interviews as a means of collecting narrative data.”

A second practical consideration involves *formalizing* the narratives to be analyzed. To interpret and analyze a story, the researcher must formalize it in one way or another. How this is accomplished depends not only on the empirical, conceptual, and theoretical issues with which the analyst is engaged, but also with the toolkit of methodological resources

available. Narratives-under-analysis is largely a process of analytically retelling stories, which inevitably risks transforming stories into something unstory-like. As we mentioned earlier, analysts typically retell stories *by way* of their analyses and thus express what the stories mean in an analytic, often abstract, form. This can be a messy business insofar as it requires analysts to earn the trust of readers by showing that the process of analysis respects and maintains the integrity and coherence of the story and the context in which it was told. The text that is fashioned for analysis results from decisions by researchers about what they will examine and in what ways it will be interpreted and contextualized.

Riessman (2008) observes that approaches to narratives-under-analysis have recently ranged from *thematic analysis* (where the focus is on “what” is said or what gets “told”); to *structural analysis* (where the focus is on the “telling” of the story, how it is organized, and the experience of storytelling itself); to *dialogic/performance analysis* (where attention is paid to both thematic and structural components and to how talk evolves intersubjectively and collaboratively).

Although there are numerous ways to formalize narratives as texts, analysts must take into account “the material ‘facts’” (Josselson, 2007, p. 8) as well as “the meaningful shape emerging from selected inner and outer experiences” of storytellers (p. 8). Analysts should be held to a high standard of re-presentation, one in which they show sensitivity to the differences between what it may have been like to be the storyteller in comparison to what it is like to be the story analyst. Too many analysts have neglected issues of authority—whose story is it anyway?—and the important question raised by Coles (1997): what gives us the right as researchers to elicit other people’s stories, leave the scene, and tell their stories to others (Bochner, 2002; Plummer, 2001)? Interpretive practices, such as narratives-under-analysis, are saturated with ethical questions and dilemmas. As analysts, we must remain vigilant and mindful of our obligations to storytellers and to the parts we ourselves play in producing formulated narratives for analysis.

This brings us to a third practical consideration—one we have been mentioning repeatedly throughout this chapter—researcher *engagement* with storytelling participants and their stories, which returns us to the issue of standpoints. What stance should one take toward the storyteller(s) and their stories? In most cases, the analyst/researcher faces the challenge of confirming participants not

merely as subjects or storytellers, but as *people who make or co-construct meaning narratively in conversation*. Riessman (2008) urges researchers to be mindful of turns in talk, to pay attention to the length of participants’ responses in interviews, and to be thoughtful and considerate about the ways they probe for details that enrich the narrative. She asserts that these aspects of narrative research require researchers to “give up control, which can create anxiety” (p. 24), especially when the nature of the subject matter is personally or emotionally sensitive. “Although we have particular paths we want to cover related to the substantive and theoretical foci of our studies,” she says, “narrative interviewing necessitates following participants down *their* trails” (p. 24, emphasis in original). Similar to the orientation of Frank (1995, 2004, 2012), Riessman (2008) places significant emphasis on the *relational* dimension of narrativity. Calling attention to the sensitive nature of narrative as a fundamental process of identity construction through meaning-making and interpersonal bonding, she highlights how narratives “invite us as listeners, readers, and viewers to enter the perspective of the narrator. Interrogating how a skilled storyteller pulls the reader/listener into the story world—and moves us emotionally through imaginative identification—is what narrative analysis can do” (p. 9).

What’s the Use?

Narrative Practices for Everyday Life

[W]e should strive to show the payoffs of our field, to show, that is, how effectively employing *the concepts behind the terms* of narrative theory can illuminate—and even influence—the wide range of cultural phenomena that we study. Phelan (2005)

We want to conclude our journey along the trail of narrative inquiry by calling attention to some of the work that reflects a pragmatic impulse to make lives better. Once we have analyzed, conceptualized, categorized, and theorized narratives, what then? How can narrative knowledge be used and applied? Can narrative inquiry achieve its moral calling to make peoples’ actions and lives more intelligible to the people themselves, helping them achieve the humane goal of pronouncing themselves and their lives “good.” In other words, can narrative inquiry produce practical tools that help people form better relationships, overcome oppressive canonical identity narratives, amplify or reclaim moral agency, cope better with contingencies and difficulties

experienced over the course of life, and thus live better lives?

Several exemplary cases exist. Beginning in the 1970s, White and Epston (1990) began a therapy practice that was initially conceived as “a storied therapy” and subsequently became widely recognized as “narrative therapy.” Concerned about the ways in which people and the problems they confront become fused, White and Epston (1990) developed a set of *externalizing narrative practices* and interventions designed to alter peoples’ beliefs that their problems are “internal to their self or the selves of others—that they or others are, in fact, the problem” (White, 2007, p. 9). The goal is to *make the problem the problem*, not the person—to experience an identity that is separate from the problem. To achieve this goal, a person must be disabused of the notion that the problem represents the “truth” about his or her identity. Construing therapy as a process of “storying” or “restorying” the lives and experiences of persons who come to them with problems, White and Epston (1990) introduced concrete narrative practices in the form of therapeutic letters, certificates, declarations, and other narrative means that promote healing and liberating stories. These practices promote a reflexive stance that can empower people to assume a sense of authorship over their experiences and relationships. Empowering people to live their lives intentionally and with greater personal agency, narrative therapists seek to free their clients to create stories that can provide meaning and direction to their lives (Madigan, 2010; Parry & Doan, 1994; Payne, 2006).

Similarly, Penn (2001) has described the work of a research group in language and writing at the Ackerman Institute that focused on the healing effects of narrative writing practices on families who are suffering in silence with a chronic illness. “When we write,” Penn (2001) observes, “we are no longer being done to: *we are doing*... when we write we construct our listener as one who is looking forward to hearing from us, not as someone waiting to withdraw” (p. 50). Penn wants sufferers of chronic illness to experience the multiple and sometimes competing inner voices, including the listening and witnessing voice, as co-existing and in need of expression in order to cope with the issues of identity that chronic illness introduces into their lives. The silenced families with whom she works write about their relationships to and feelings about each other and their illness. They bring what they’ve written to their sessions, read them aloud, and express feelings

“in a new voice” that can become “a *lifeline* because of its power to reconnect the family and mitigate the effects of its relational traumas” (p. 33). Penn’s enthusiastic endorsement of the healing effects of writing personal stories about traumatic experiences has been echoed by Pennebaker (1997), who presents evidence of the positive health benefits of writing emotionally about the unspoken feelings and thoughts one experiences while coping with illness or trauma; Harris (2003), who views personal writing as a mode of translating “the physical world into the world of language where there is an interplay between order and disorder, wounding and repair” (p. 2); DeSalvo (1999), who endorses therapeutic writing as a mode of caring for one’s self, a form of self-analysis and self-restitution that can shift one’s perspective and thus help people integrate deeply experienced but unexpressed emotions linked to traumatic events provided it is done correctly; and Herman (1997), who cautions that “as the survivor summons her memories, the need to preserve safety must be balanced against the need to face the past” (p. 176). For Herman, traumatic memories are pre-narrative, and the work of confronting them involves a process of integrating them into one’s life story, a narrative practice akin to what Greenspan (1998) called “recounting,” a struggle between meaning and memory that was elegantly captured by one of the Holocaust survivors he studied, Leon, who observed: “It is *not* a story. It has to be *made* a story... (p. xvi).

Frank (2000) is unapologetic about his desire “to make ill people’s stories more highly credited primarily among the ill themselves and then among those who care for them” (p. 136). Frank’s agenda is unequivocally activist and political: “I hope to shift the dominant cultural conception of illness away from passivity—the ill person as ‘victim of disease and then recipient of care—toward activity” (Frank, 1995, p. xi). He sees one of the main challenges of illness as the construction of a story that can function as a meaningful and self-validating moral narrative. Recognizing the political, ethical, and personal consequences of affirming the voices of the afflicted, Kleinman (1988) emphasizes the reflexive quality of personal narratives, observing that “the personal narrative does not merely reflect illness experience, but rather it contributes to the experience of symptoms and suffering” (p. 49). The ill person must negotiate the spaces between the domination of cultural scripts of bodily dysfunction out of which one’s meanings are constructed and defined and the situated understanding of one’s

experience that seeks a unique personal meaning for suffering. Illness narratives need to be told not only because the telling of the story can provide the therapeutic benefits of redemptive understanding, but also because of the political consequences of connecting the body to the self, revealing embodiment and emotionality as legitimate and significant mediums of lived experience and inscribing bodily dysfunction with positive meaning and value (Ellis, 1998). These stories bring suffering bodies out of the darkness of the alley into the light of day, transgressing the taboos against telling and risking rejection in the name of the right to speak and the longing to be heard (Bochner, 2001). “To tell the story of one’s affliction,” writes Hawkins (1993, p. 190), “becomes a way to distance it from oneself, to move beyond it, to repair its damages and return to the living community—in a word, to heal.”

On the other side of the illness equation—the side of physicians—Charon (Charon, 2008; Charon & Montello, 2002) has worked tirelessly to develop practices of narrative medicine in which the health practitioner “recognizes suffering, provides comfort, and honors the stories of illness” (Charon, 2008, p. ix). She wants health care practitioners to be stirred by stories of illness, which means they must develop *narrative competence*. To achieve narrative competence doctors must develop a capacity for close reading, be able to acknowledge their own emotional responses to the suffering they witness, and value narrating as a means of engagement as well as an ethical obligation. Charon (2007) wants doctors to bestow attention on patients, to represent what they witness in accessible language and to participate in reflective clinical writing. “Instead of depleting us, this [narrative medicine] care replenishes us,” writes Charon (2006), “for our suffering helps our patients to bear theirs” (p. 236).

Attempting to reach deep into the unsettled subjectivity of a clinical ethicist—in this case his own—Zaner (2004) invites readers to sit at the bedside of some of the most heartbreaking and demanding cases of medical morality one could imagine. Beleaguered, vexed, and menaced by the emotional and embodied plight of patients teetering on the edge of oblivion, Zaner (2004) wavers between hope and despair, enacting a reflexive relatedness and openness to otherness through the medium of storytelling. Here, at the border region of mortality, Zaner (2004) musters the courage to listen intensely, focus reflectively, and connect humanely in an atmosphere riddled with dreadful contradictions, painful ironies, and wretched vulnerabilities.

This is a border encircled by sharp edges, where decisions must be made and mortality cannot be denied. Still, Zaner (2004) recognizes that “relationships are the centerpiece of ethics,” and his stories show how one might openly and fearlessly engage in such an encounter from the depths of one’s own subjectivity. Going deep, Zaner (2004) attempts to come to grips with questions such as how we can live “and make sense of our lives in the face of the awful happening of chance events” (Zaner, 2004, p. 101) or what can be said or done when the help one needs simply can’t be provided. In the process of searching, probing, and questioning, he delivers a heartening and uplifting expression of what ethical dialogue can mean and do.

Nelson (2001), also a narrative ethicist, has introduced the concept of the *counterstory* as a means of resistance and repair for people suffering the diminished moral agency associated with oppressive canonical identities. “Oppression often infiltrates a person’s consciousness,” she observes, “as her oppressors want her to, rating herself as they rate her” (p. 7). When this happens, a person’s identity is damaged. To become a moral agent in one’s own right, agency must be freed from the grips of oppressive master narratives. An identity damaged by oppressive master narratives must be repaired. The counterstory is a purposeful attempt to shift the meaning of a person’s or community’s social identity by dislodging the oppressive qualities of a master narrative. Nelson believes that the communities in which we are enmeshed impose on and constrain our understanding of ourselves and often deprive us of opportunities to become authors of our own actions. Master narratives that construct images and identities of the elderly, gender, race, sexuality, and disability can be neutralized by good counterstories that directly contest the narratives they resist and repudiate, offering the potential for wide circulation.

In the sphere of tourism, Noy (2012) has focused attention on identifying how subversive counterstories work their way into the performative spaces of historical and memorial sites, which can turn out to be spaces in which meanings can be contested. Usually, these sites are intended to maintain power and authority over the truth of historical “facts,” in particular how they will be remembered and understood. But the same site can mean many different things to different people and groups (Noy, 2012) and thus these sites hold the performative potential to destabilize and transform the largely ideological meanings and feelings attached to and promoted

by these historical places. When tourist spaces are performative “they get people to engage, to move around, to carry and create meanings in public spaces” (p. 147) and, consequently, to introduce and amplify alternative, even subversive, narratives.

The coercive power of a story also has been discussed by Freeman (2010) as an expression of what he calls *narrative foreclosure*, “the conviction that one’s story is effectively over, that no prospect exists for opening up a new chapter of one’s life” (p. 12). To foreclose on a narrative is to become a prisoner of one’s story, to be walled in and weighed down, obscuring all possibility of narrative freedom. Freeman (2010) emphasizes “the poetic labor of narrative” (p. 152) associated with *practices of hindsight*, which can renew or regenerate a narrative frozen in time. The challenge is “to break away from them and sap them of their coercive power . . . identifying and naming” (p. 13) the narrative one has been living.

Freeman (1997, 2010) has introduced two other narrative practices, one allied with the delayed quality of memory work, the other with decisions about how to act in consequential situations that will later be remembered. *Moral lateness* refers to the recollection and refashioning of memories through which “we see now what we couldn’t see then”—that we did not do the right thing. We were blind to the moral choices of right or wrong, or good or bad, that we faced on that occasion. Now, we feel forced to face the remorse, regret, or repentance that these memories evoke. Can we forgive ourselves? Can we reconcile how we see ourselves now in light of what we did then?

Narrative integrity (Freeman, 1997) is a practice concerned with the other end of the temporal dimension of narrative, the call of the future, which one day likely will be a memory. As a life practice, narrative integrity anticipates how we will remember what we are planning to do now or next. One day in the future, the story of how I am about to act will be a tale I will look back on either with pride and gratification or shame and degradation. Which will it be? Can I take stock of my options and authorize a story that dignifies and honors my actions? By exercising narrative integrity, we seize an opportunity to make narrative a part of the fabric of our experience and memory as we live it.

All of these innovations in narrative practices call attention to what storytelling does, how it normalizes as well as how it can transgress; how the stories we tell are constrained by patterns of relationship, culture, and history; which stories get told and which ones stay untold; who gets to speak and who

must remain silent; how stories heal and how they can damage.

Conclusion

Small-story researchers have shown that informal, everyday interactions are an important site of subjectivity and meaning-making, a site of narrative performances in which identity is performed and negotiated. In moment-to-moment, everyday interaction, people perform and negotiate identities, using small stories to achieve what Goffman (1959) once called a working consensus on the definition of the situation and to place identity under construction. Big-story researchers, conversely, have shown that human beings have a strong urge to dwell at the crossroad of narration and reflection. We are historical creatures who find ourselves thrown into the chaos of a mortal life lived in deep temporality—between birth and death, between history and destiny, between what we have inherited and experienced from the past and what we anticipate and can become in the future (Ricoeur, 1981). In short, human life is saturated with “an autobiographical imperative” (Eakin, 1985, pp. 275–278), a longing to make sense of the plural unity of time—past, present, and future. As long as we can remember, and remember remembering, we are likely to remain steadfast in our determination to recover the past and stretch what we make of it across the trajectory of our lives. Although it is true that we appear to live only in the present, we also “sojourn in the land of memory” (Hampl, 1999). Thus, it is more accurate to say that we live in between, perpetually moving forward into experience and backward into memory. Big-story researchers have shown that the narrative work of memory, the struggle between meaning and memory, involves both listening to and expressing what our memories tell us in the hope that our second and third draft can improve on the first.

We do not have a crystal ball in which to look into the future and anticipate the next turns in narrative inquiry. We will be pleased if narrative inquiry continues to situate itself within an intermediate zone between science and art, self and others, big stories and small stories, and theories and stories, and is understood and regarded as a meeting place for storytellers that promotes multiplicity and diversity, where head and heart go hand in hand, and embodied narrators work to produce a rigorous and creative body of scholarship that is passionate, political, personal, critical, open-ended, enlightening, pleasurable, meaningful, useful, and sufficiently evocative to keep the conversation going.

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Abstract

Embracing the trope of ethnography as narrative, this chapter uses the mythic story of Bronislaw Malinowski's early career and fieldwork as a vehicle through which to explore key aspects of ethnography's history and development into a distinct form of qualitative research. The reputed "founding father" of the ethnographic approach, Malinowski was a brilliant social scientist, dynamic writer, conceited colonialist, and, above all else, pathetically human. Through a series of intervallic steps—in and out of Malinowski's path from Poland to the "Cambridge School" and eventually to the western Pacific—I trace the legacy of ethnography to its current position as a critical, historically informed, and unfailingly evolving research endeavor. As a research methodology that has continually reflected on and revised its practices and modes of presentation, ethnography is boundless. Yet minus its political, ethical, and historical moorings, I argue, the complexities of twenty-first-century society render its future uncertain.

Key Words: anthropology, colonialism, epistemology, the field, intersubjectivity, Malinowski, methodology, writing culture

During my final weeks working on this chapter, I happened to watch the documentary *The Black Power Mixtape 1967–1975*—a contemporary collage of rarely seen Swedish television footage of the Black Power cultural revolution of the 1960s and 1970s. In the 1973 chapter of the DVD, there is a brief scene from inside a Swedish tour bus traveling around New York City. As the bus passes through Harlem, the tour guide—speaking in Swedish but translated as subtitles—describes the upper Manhattan neighborhood as “undoubtedly the Black man’s ghetto” where “large amounts of narcotics are circulating”; he goes on to remind the tourists of how their “welcome letter” had instructed them that the tour company did “not want anyone to visit Harlem for *personal studies* . . . because [Harlem] is only for Black people” (Olsson, 2011—emphasis added).

This human desire for personal studies, the traveler’s yearning to get off the tour bus, the curiosity

to move beyond the pretense of staged representations of life and to discover what it is really all about, underlies the post-Enlightenment project of apprehending the world through physical force, cognitive classifications and containments, and, at times, empathetic pretensions. The same impulses anticipated among Swedes in 1970s New York inspired a generation of European explorers to penetrate the dark continent of Africa (Thornton, 1983) and continue to compel turn of the (twenty-first) century visitors to Chicago to sift and sort through a sliding scale of authentic venues in search of “the real” Chicago blues experience. But, as David Grazian (2003) has effectively shown, even the most seemingly authentic of these late modern cultural products are fabricated commodifications, banking on the city’s global popularity as a blues destination.

Such realizations have implications for how we think about the history, current state, and future of

ethnography. More than merely embracing Erving Goffman's (1959) mid-twentieth-century declaration that "all life is a stage"—though its connotations are perhaps more profound than some recognize—the staging of the ethnographic project is acutely linked to an invasive mix of privilege and inquisition that sprouted in the garden of Western modernity and spread throughout the colonial hinterland. To make sense of this deep history one must begin with questions like: what does it mean to study the life of someone else? What gives anyone the right to initiate research on another community (even when they sincerely and passionately believe it is for the community's betterment)? And, pressing beyond the expected, pedestrian answers, what larger goals are we working towards or working in the service of when we undertake qualitative social fieldwork?

I can imagine our Swedish tourist being just as curious about the dealings of Wall Street investment bankers (Ho, 2009) but less inclined to consider *going there*, not necessarily out of a conscious awareness of Wall Street's inaccessibility, but due to a doxic (Bourdieu, 1977) inability to even acknowledge it as a possibility. Then again, social researchers and cultural commentators from W. E. B. DuBois (1903/1996) to Norman Mailer (1957) to Jon Cruz (1999) have observed the racially loaded fascinations that people of European descent have about those they (a) have had unproblematized access to and (b) view as most distinct from themselves, either physically, culturally, or both. Explanations for this range from the allure of the exotic and presumed primal drives towards straightaway satisfaction and survival that govern those at the other end of the civilization spectrum (here Mailer and perhaps Malinowski) to empathy with the romanticized innocence that such closeness to nature and freedom from civilization's repressive shackles offers (here Margaret Mead and perhaps Malinowski). Anthropology—the discipline to which ethnography is most historically bound—came of age as a legitimate academic field through these Western impulses while simultaneously fueling their popular interest (Thornton, 1983).

Like the threat of Swedish tourists undertaking personal studies, ethnography as a research practice is, in many respects, renegade. That is, it refuses to follow strict conventions and achieves virtue and vitality through its lack of prescription. Ethnography straddles structured research design and improvised inquisitive adventure, constantly moving betwixt and between theory, data, and

analysis (O'Dell & Willim, 2011). Although it is non-linear, it is profoundly narrative.

* * *

This chapter introduces ethnography, as a specific type of qualitative research methodology, through an historically conscious narrative of its principal and principled approaches. Much has changed in ethnography since the classic era when researchers such as Alfred R. Radcliffe-Brown (1922) and E. E. Evans-Pritchard (1940/1969) traveled to far-away places with names like the Andaman Islands and Nuerland. Their charge was to plot the topography of human cultural difference and to identify, via conditions of isolation and theories of unified wholes, the systems and processes through which social life successfully functioned. Today, most observers regard ethnography as fitting within a more sophisticated project of making sense of social life through the ways of knowing that are most meaningful and potentially most consequential to social actors themselves. Yet I caution against the tendency for each coming-of-age generation to selectively disconnect itself from those that came before.¹ Ethnographers trained in fields such as anthropology, sociology, cultural studies, and folklore recognize the importance—or have experienced the rite-of-passage mandate—of knowing their history. Still, mere knowledge of past right- and wrongdoings combined with a critical disposition neither empowers contemporary ethnographers to make the most of their approach's unique virtues nor alleviates them from its most primordial problematics. Moreover, as ethnography has propagated into such fields as organizational studies, planning, management, and industrial engineering (to name just three) concerns over research efficiency and tangible outcomes tend to eclipse the historically informed and critical perspectives that have defined its fundamental modes of understanding. What is called for, then, is an accounting of ethnography that situates it contemporarily while simultaneously integrating historical actors and the social forces they at times conformed to and at others contended with.

One of the more damaging consequences of ethnography's spreading popularity has been the propensity to view it as a method rather than a methodology.² This difference is significant. A method is simply a technique or tool used to collect data. Ethnographers often utilize a variety of tools and techniques during the course of their research, including but not limited to: establishing rapport; selecting informants; using a range of interview and

focus group forms; making observations—both participatory and non-participatory—and writing field notes based on them; conducting surveys, genealogies, and domain analyses; mapping fields; transcribing texts; and coding data.³ In contrast, a methodology is a theoretical, ethical, political, and at times moral orientation to research, which guides the decisions one makes, including choices about research methods. This distinction between method and methodology is crucial to my effort to differentiate ethnography from qualitative field research more generally. Much of what is included in this chapter will be useful to qualitative researchers on the whole. However, my primary purpose is to describe and delineate ethnography as a communally engaged and historically informed early twenty-first-century research practice.

Much like *culture*, ethnography is one of those social scientific abstractions that is readily deployed to mark out what we—as anthropologists, sociologists, and an increasing range of researchers in other fields—do as unique, yet is difficult to capture in a single precise and thoroughgoing definition.⁴ Part of the difficulty is that the term refers to both a research process and the written product of those research activities. While not losing sight of the important revisions to come out of its “crisis of representation” that have pushed scholars to acknowledge, and in fact prioritize its ultimate textual character (Clifford & Marcus, 1986; Marcus & Fischer, 1986), in this chapter, I mostly treat ethnography as a processual approach to *doing* a particular kind of qualitative research.

To begin, I present a few basic definitions of ethnography. Carol A. Bailey (2007) quite simply explains it as “a type of field research that requires longterm engagement in a natural setting” (p. 206). In a more detailed description, Martyn Hammersley and Paul Atkinson outline the ethnographic project as:

participating, overtly or covertly in people’s daily lives for an extended period of time, watching what happens, listening to what is said, asking question... [and] collecting whatever [other] data are available to throw light on the issues that are the focus of the research. [1995, p. 1]

Lastly, Clifford Geertz (1973), in his classic treatment, defines ethnography as “an elaborate venture in... ‘thick description’” (p. 6). Etymologically, ethnography combines *ethno*, meaning “culture (or race),” and *graphy*, meaning “to write, record, and describe.”⁵ Thus ethnography, which Barbara

Tedlock (2000) refers to as an “inscription practice” (p. 455), can be thought of as the process and product of writing, recording, and describing culture.

Building off of these different understandings, my treatment of ethnography is simultaneously broad and narrow. During the late twentieth century and now into the twenty-first, ethnography moved from the confined ranges of anthropology and sociology to a tremendous number of disciplines and fields, including (in addition to those listed earlier) psychology, geography, women’s studies, history, criminology, education, political science, communications, leisure studies, counseling, nursing, psychiatry, medicine, social work, and law (see Tedlock, 2000; Denzin & Lincoln, 2008; Jones & Watt, 2010), just to name a few. Attempts to put narrow disciplinary restraints on ethnography are, in my view, shortsighted and possibly even disciplinarily egocentric. Similarly, the variety of practices involved with ethnography is expansive and continually expanding. These include several traditional qualitative research methods (such as those listed earlier) as well as more recent innovations that cross into visual and sensory studies (Pink, 2006, 2009), the arts (Leavy, 2009; Schneider & Wright, 2010), action-oriented research (Kemmis & McTaggart, 2000), autoethnography (Ellis, 2004; S. H. Jones, 2008), and collaborative ethnography (Lassiter, 2005). This is not the place to explicate the multifaceted dimensions of these varied approaches, but I want to be clear in stating that all cohere (or have the potential to) with the understanding of ethnography that I put forward.

At the same time, there has been a tendency among some scholars to define almost any qualitative research project—and particularly projects involving traveling to a field site—as ethnographic. On this matter I am more stringent in explaining that ethnography involves more than just going somewhere to conduct research *on* or *within* a community. It involves a certain frame of mind, or, I will even say, historically aware sensibility that is very much its own. Ethnography is often equated with the practice of (or practices surrounding) participant observation. I agree to the extent that ethnography fits within a participant-observation framework, yet to highlight what I see as a key difference, let me return to the definition from Geertz, which is premised on his notion of *thick description*. In his classic illustration of thick description, Geertz (1973) discusses Gilbert Ryle’s (1971) distinction between the involuntary contracting of the eyelid associated with a twitch and winking. While as a physical description of action the two are the same,

properly contextualized—in the case of the wink, involving such things as impetus, intention, and success in communication—they are drastically different. Ethnography, as I am defining it (as a methodology), involves degrees of impetus, intention, and conviction that are different from simply having a participant-observatory perspective and standpoint. Although many of its characteristics have changed since the days when Margaret Mead first traveled to Samoa, like the origins of ethnography itself, these changes have been as much a gradual, reflective, and historically mediated evolution as a radical shift. Thus, a solid grounding in the history of ethnography is important to understanding how current ethnographic research differs from what we might broadly call qualitative field research.

My approach involves reviving, interrogating, and embarking on a narrative journey via ethnography's most pervasive origin story. That is the chronicle of Bronislaw Malinowski's pioneering field research in the Trobriand Islands, which, within the core fields listed earlier, is commonly held up as the ethnographic archetype (Strathern, 1987). In doing this, I attend to the multiple trajectories of development and enlightenment that follow from these mythic origins. This is complex terrain since, as most researchers now recognize, ethnography was birthed out of colonialist impetuses that included "territorial expansion, the pursuit of military power, commercial greed... the need to find raw materials and investment opportunities for accumulated capital, [as well as] an emerging 'media industry' in search of stories to sell" (Fabian, 2000, p. 4; see also Thornton, 1983). Retrospectively, the history of ethnography is comprised of hardly heroic heroes (see Sontag, 1966/1978). While I do not shy away from the intellectual temptation of unpacking the possible fictions surrounding Malinowski as a mythic figure, I ultimately treat representations as real—meaning, they are products of contested political processes that have real consequences (Hall, 1996). Thus these historical trajectories are shaped as much by what is represented and remembered, which is never fixed, as by what actually might have been.

Building on the trope of ethnography-as-narrative-journey, this chapter uses the narratives of Malinowski's early life and career as vehicles through which to present important aspects of and issues facing contemporary ethnography. This involves a series of intervallic, temporal steps out of the early twentieth century into broader historical and present-day contexts. I begin by discussing

Malinowski's mythic status in relation to some of his ideas regarding the social functioning of myths. I next review his early life experiences and education in Poland and Germany as a means to introduce key paradigmatic and epistemological underpinnings of the ethnographic enterprise. Malinowski's travels to England and association with the Cambridge School provide an opportunity to present the transition in social research practices during the late nineteenth and early twentieth centuries, which the myth of his methodological revolution belies. His initial research experiences on the island of Mailu illustrate the colonial legacy of the ethnographic project as well as the interpersonal dynamics of its research practices, and his transition from Mailu to the Trobriand Islands offers an opportunity to contemplate the changing notion of the ethnographic "field." The 1922 publication of *Argonauts of the Western Pacific* marks a watershed moment in the history of ethnography and Malinowski's career. It was here where he first presented his "modern sociological method of fieldwork" (Stocking, 1983b, p. 111). My reflections on the impact of this book segue into some important considerations surrounding what has been referred to as (among other things) ethnography's "literary turn." Finally, a consideration of Malinowski's reputation gives rise to some conclusionary remarks regarding ethnography's historical legacy and future. Journeying through the life of the man whose idealized image, more than anyone else, came to epitomize ethnography and whose divulged human frailties contributed to its reorientation highlights a degree of sophistication that is frequently omitted in deference to (too often self-congratulatory) how-far-we-have-come framings of history.

Malinowski's Myth

The history of ethnography is replete with its own myths, superstitions, and survivals. As the countless ethnographers who have studied these topics over the last century-plus have taught us, such aspects of culture should not be dismissed lightly but rather interrogated for the important purposes, both functional and symbolic, they serve. In ethnography's most prominent origin story, Polish-born, British-educated⁶ Bronislaw Malinowski is cast as its progenitor. Though the "Malinowski myth" has been discussed in several anthropology-specific treatments of methods, theory, and the history of the field,⁷ as ethnographic research has diffused into other areas, Malinowski the man, the myth, and the heuristic value of both have become dispensable.

This chapter—as much for a non-anthropological readership as for a distinctly anthropological one—aims to correct this.

Viewed through the lens of some of his own theoretical findings, Malinowski's early life and career, that is, his circuitous journey to “inventing” the ethnographic method, becomes an instructive hagiography—part travelogue, part founding fable. In developing his own version of (psychological) functionalism, Malinowski did groundbreaking work on the topics of myth, magic, and superstition. Contrasting early views that interpreted myths as “idle rhapsody” or “aimless outpourings of vain imaginings” (Malinowski, 1926/1948, p. 97), he forcefully put forward the position that myths actively affect the conduct of members of a community by exercising “a living reality, believed to have once happened in primeval times, and continuing ever since to influence the world and human destinies” (p. 100). Through myths individual reputations are made and sustained and important lessons and understandings of cultural practices are carried over time.

According to Malinowski's myth, the young Pole first became fascinated with cross-cultural study when, during a period of illness, his mother read him sections of Sir James Frazier's *The Golden Bough* (1900). After receiving his doctorate in physics and mathematics in Poland, Malinowski, as the story goes, traveled to England in pursuit of education and romance. Once there, he converted to the budding science of anthropology and in 1914 set off to do field research in the southwest Pacific where, as a consequence of the outbreak of war in Europe,⁸ he found himself stranded for several years. During this time—after realizing the importance of the anthropologist getting “off the verandah” (Singer & Dakowski, 1986b) and, instead, living among the natives—he established what he claimed was “an entirely new academic discipline” (Leach, 1957/2000b, p. 49), now known as ethnography.

Foundations of a Man and His Methodology

Like an onion, the layers of Malinowski's myth can be peeled back to reveal numerous inconsistencies, resulting from selective embellishments, missing details, lacks of contextualization, and perhaps just plain concoctions. Adopting a weighty ethnographic tag popularized by James Clifford (1986), the various versions of Malinowski's story are at best *partial truths*. Although divining the correct version of this story is not my goal, interrogating some of

its factual bases opens a didactic narrative pathway along which to contextualize the famed “father of fieldwork” (Thornton, 1985, p. 8).

Both Malinowski's class background and the role of his mother in introducing him to the work of Frazier have been scrutinized.⁹ The question of class is notable if for no other reason because early ethnography—with its demands of traveling to faraway places and associated reprieve from everyday economic necessities—was thought to be an elite profession (Nash & Wintrob, 1972; Tedlock, 2000). By the early years of his post-secondary education, Malinowski was undoubtedly familiar with *The Golden Bough*. The book's focus on the worship of Diana at Nemi in southern Italy in all likelihood resonated with Malinowski, who as a sickly youngster, upon the orders of his doctors, had traveled throughout the Mediterranean with his mother (Wayne, 1985);¹⁰ and reading Frazier's cross-cultural comparisons with “exotic” customs from around the world most certainly nourished the exceedingly ambitious Malinowski's desire to conduct his own *personal studies*.

Malinowski's journey to England was preceded by two years at Leipzig University in Germany where he was directed toward *Völkerpsychologie* through the work of the university rector and future “father of experimental psychology,” (Kess, 1981, p. 126) Wilhelm Wundt. As with his earlier path to Jagiellonian University in Poland—where his father was “a renowned professor of Slavic philology...[with] a lively interest in Polish ethnography and folklore” (Pulman, 2004/5, p. 126)—Malinowski's decision to study at Leipzig was quite literally following paternal footsteps. While at Leipzig in the 1860s, Lucjan Malinowski had “broke[n] new ground in methodology” with his doctoral dissertation in Silesian dialectics (M. W. Young, 2004, p. 12). Yet the younger Malinowski, who by all reports was never close to his father (Kubica, 1988, p. 89; Wayne, 1985, p. 529), apparently also chose Leipzig because of its reputable program in thermodynamics (M. W. Young, p. 128).

The decision to travel to England was indeed motivated by romantic interests. Shortly after arriving in Leipzig, Malinowski met the widowed South African pianist Annie Brunton—described by his daughter as a woman “considerably older than him” (Wayne, 1985, p. 531)—and the two began a stormy affair. In December 1909, when Brunton moved to London, Malinowski soon followed. He once said that “if [he] hadn't met Mrs. Brunton [he] would never have taken up sociology”

(Wayne, p. 532). Though likely an example of his characteristic hyperbole and flare for the dramatic, Brunton undoubtedly influenced the much younger “Bronius’s” intellectual growth in at least two ways. First, by pulling him from Leipzig—an institution that “represented the best of German science” (M. W. Young, 2004, p. 130) where he had the opportunity to work with a venerable master in the field¹¹—to Britain, which by 1910 was a hotbed for ethnology and home to prominent figures like Edward Burnett Tylor, William H. R. Rivers, Charles Seligman, and Malinowski’s old friend Frazier. The second influence came through Brunton’s role in (re-)exposing Malinowski to music, and, by extension, to the arts in general.

One oft-cited tension in Malinowski’s psyche was the opposition between the scientist and the artist, reason and intuition, rationality and emotion (Thornton, 1985; M. W. Young, 2004). The productive off-play of these two temperaments would serve him well—in terms of both methodological process and written product—as an ethnographer.¹² Upon arriving in Leipzig, with the intention to study the thermodynamics of liquids and gases at “the renowned centre in Europe” for such study (M. W. Young, p. 128), one could surmise that Malinowski’s pendulum had swung sharply towards science. Annie Brunton’s greatest influence on the aspiring young scholar may have been to bring him back into balance—as turbulent as a Malinowskian balance would have been—and to open his eyes to the possibilities beyond the “best of science” that had so intrigued him years before.¹³

Ethnographic Science, Ethnographic Humanity

Ethnography can take many forms and guises. Despite some commonalities in practices and politics, ethnographers adhere to multiple epistemologies and paradigmatic understandings of what constitutes good research. This creates a troublesome tension: whereas different researchers and research activities may appear the same, and may be guided by similar politics and sensibilities, they nevertheless may be foundationally grounded in different philosophies of knowledge. Malinowski, fittingly perhaps, straddled ethnography’s prime epistemic divide. Anthropology has been referred to as the social science that is closest to the humanities (Redfield, 1953; Auger, 1995). Ethnography, as its chief mode of research, is firmly situated at these crossroads. Yet this position is never fixed.

As ethnographic practices have spread into other disciplines, the potential outcomes and misunderstandings resulting from epistemological differences, although not always discussed, have become more pronounced. When people undertake ethnographic research in the fields of, for instance, architecture, marketing, and/or women’s studies, what are their goals and what are considered legitimate means of attaining these goals? Thomas Schwandt (2000) highlights three areas of concern surrounding qualitative inquiry, which are instructive for a discussion of ethnography in particular. I adapt them here:

1. Cognitive concerns surrounding how to define, justify, and legitimize claims to understanding, which might or definitively might not include questions of validity, transferability, and generalizability.
2. Social concerns regarding (in this case) the goals of ethnography: should they be emancipatory and transformative? Should ethnographers seek solutions/answers to problems/questions that are of direct interest to their own academic communities and/or to the communities they study? Or should they seek to understand the situations in which, and the social processes through which, human actions take place in the ultimate interest of working towards a better understanding of sociality in general as well as in the particular? Questions such as these are neither all encompassing nor mutually exclusive but they do point towards potentially stark divergences in the ethnographic enterprise.
3. Moral concerns as to how to “envison and occupy the ethical space” between ethnographers and those they research in responsible, obligatorily aware, and status conscious ways. (see Schwandt, 2000, p. 200)

The first of the three areas—specifically ethnographers’ epistemological embeddedness and paradigmatic adhesions—is of most immediate concern here. Nonetheless, for the ethnographer, cognitive concerns are not neatly separated from social and moral ones. Although I save discussions of social responsibility and ethics until later in the chapter, an awareness of both their impact on, and how they are impacted by, foundations of knowledge and understandings of legitimate research are important.

Before briefly outlining the guiding paradigms surrounding ethnography, I offer a few additional caveats. Whereas defining and labeling these various epistemological and methodological frameworks

is useful, it would be a mistake to give too much attention to trying to fit a particular researcher or even an instance of ethnographic research neatly into one category. Ethnographic experience is perpetually ephemeral, meaning that at times ethnographers are prone to move, transform, and shape shift between different paradigmatic classifications. Attempts to categorize also tend to highlight differences over time and disciplinary space. While differences do exist, the need to place individuals or projects in particular boxes closes down the possibility of also seeing commonalities and furthermore belies the nuanced nature of ethnographic inquiry. Nonetheless, in what follows, I label some of the traditions that ethnographers might move between and draw on variably as paradigmatic resources.

I begin, quite straightforwardly, by separating inclinations towards science and inclinations towards the arts and humanities. This can, by and large, be cast as a binary between positivism and what I will broadly call interpretivism. Although few if any contemporary ethnographers would define themselves as strict positivists, it is nonetheless necessary to discuss positivism as foundational to any social scientific enterprise. To some extent, outlining the tenets of strict positivism may be useful in explaining what most ethnographers are not. However, before dismissing it too quickly, I should point out that, particularly with regard to the mandates of certain gatekeepers of credible research reporting, ethnography is not as far removed from its positivist principles as some of its practitioners would like to think. Furthermore, there is an important post-positivist paradigm that continues to carry weight.

POSITIVISM

Positivism is premised on a belief in what is referred to as *naïve realism*—that is, the notion that there is a reality “out there” that can be grasped through sensory perception. As such, it holds empirical data—that which is produced through direct observations—as definitive evidence through which to construct claims to truth. In doing so, positivism prioritizes objectivity, assuming that it is possible for a researcher to detach his or herself from values, interests, or the clouding contamination of bias and prejudice. Following this formula, good research is achieved through conventional rigor—that is, dutifully following a prescribed, systematic, series of steps surrounding data accumulation and analysis. With this being the most scientific frame of reference that ethnography potentially occupies, standards of hypothesis testing and deductive reasoning

are principal to its practices. In that positivism recognizes a fundamental (capital “T”) Truth, which it is believed researchers can apprehend, ethnographers anchored in this tradition are more prone to concern themselves with questions of transferability (i.e., can the findings from one setting be applied to another?) and generalizability (i.e., can the findings from a particular context be generalized on to the whole?) on the assumption that such Truth has potential relevance for a broad range of social circumstances and cultural contexts. Today all ethnographic researchers recognize the role of culture and socialization in shaping social realities; thus, strict positivism has fallen out of favor. However, post-positivist orientations towards valuing empirical evidence, making efforts toward detached objectivity, and deductive reason continue, even if researchers are less confident about the conclusions.

INTERPRETIVISM

If the positivist epistemological branch, with its post-positivist paradigmatic inclinations, supports Malinowski the scientist, Malinowski the artist is perched on the interpretivist (or constructionist) alternative. This position, which issues from an acknowledgement of the constructed nature of all social reality, recognizes no single all-encompassing Truth, but rather multiple (small “t”) truths that are the products of human subjectivities. As such, cultural and contextual specifics are critical to understanding, and inductive reasoning becomes the privileged path to making sense of unwieldy social realities. Reality, which is shaped by experience, thus becomes something to be interpreted. Such interpretivism sees human action as inherently meaningful with meanings being processual, temporal, and historically unfinished.¹⁴

The subjectivity of the ethnographer is quite consequential here. Under any form of interpretivism, the outcomes of researcher bias are acknowledged. Sometimes efforts are made to mitigate researchers’ subjectivities. Such techniques might involve reflexive journaling, inventorying subjectivities, and other attempts to manage and track bias (Schwandt, 2000, p. 207 n. 11). Yet increasingly interpretivist approaches accept that within ethnography the human is the research instrument and as such, cultural, social, and personal frames of reference are inescapable.

* * *

To repeat myself, I do not think particular researchers or specific research projects should

necessarily being categorized along the broad epistemological strokes that I am painting. Although I acknowledge that many are, I think it is important to appreciate how both positivist and interpretivist foundations impact all ethnography. Indeed, I would question if a researcher with inclinations and sensibilities fully saturated in post-positivism would even fit into my rather scrupulous definition of ethnography—a confirmatory approach to assessing one’s hypothesis via the accumulation of empirical data through long-term fieldwork living as a member of a community strikes me more as a non-ethnographic form of participant observation. Nonetheless, it would be limiting to not recognize how the significance of positivist and post-positivist tenets impact ethnography.

Since Malinowski’s early-twentieth-century articulation of ethnography as a proper research method, there have been two general movements, which have overshadowed an assortment of counter-currents and inter/intra-disciplinary variations. The earlier of the two, which dominated anthropology up until the Second World War, was the movement towards legitimizing ethnography as a rigorous scientific method on par with those practiced in the supposed “harder” natural sciences. The latter part of the twentieth century witnessed the rise of a more humanistic acceptance of ethnographic research. Dennison Nash and Ronald Wintrob (1972) have suggested this may have more to do with what is institutionally accepted as legitimate research and how that shapes what aspects of the research process the researcher is willing to disclose than with what researchers themselves believe. As evidenced in his early ethnographic writings and actualized through the posthumous publishing of his field diaries (see “Malinowski’s literary (re)turn”), Malinowski, although very much a researcher of this earlier era, personified this crucial ethnographic binary.

In concluding what has been outlined, I think it is useful to highlight two pervading (non-exclusive) sets of questions that are at the core of these paradigmatic tensions: one surrounds the basis of truth, and the second is concerned with the positioning of the researcher in respect to the research endeavor.

1. Is truth something that exists independently to be discovered by researchers? Are truths the products of subjectively authored realities to be grasped by researchers? Or are these subjective “truthful realities” to be engaged with the researcher as part of the truth-making process?

2. Ethnography is defined in part by its participant-observation mandate of researcher involvement. Yet should this constitute taking up an inside/involved standpoint from which to make detached observations? Should it be based on a deeply engaged experiential understanding? Or should researchers understand themselves as active participants in shaping the social world they conduct research in?

The answers to such questions may look very different depending on the disciplinary, institutional, and personal groundings of the researcher; the standards of the outlets where they are seeking to publish, publicize, or apply their work; and/or the specific uses to which the findings of a particular project will be put. For example, commercial ethnographers working under the dual pressures of time and a need to communicate applicable findings, both customary in the business world (Ehn & Löfgren, 2009), will feel compelled to adopt a more scientifically precise mode of inquiry and reporting that steers clear of the theoretical complexities and deliberations commonly found within academia.

Malinowski Encounters the Cambridge School

In addition to his pursuit of Annie Brunton, Malinowski had a second romantic interest in England. Since a childhood visit with his mother, young Bronius had cultivated an intense attachment to anything having to do with Britain. While crossing the English Channel by ferry, he wrote an essay-letter to a Polish friend in which he confessed to having “a highly developed Anglomania” and “an almost mystic cult of British culture” (Wayne, 1985, p. 532).

It appears that his interests in anthropology were firmly set while making this journey, for once in England, he wasted little time traveling to Cambridge and introducing himself to Rivers and Alfred Cort Haddon—two men who had brought ethnological acclaim to the school by way of their 1898 Expedition to the Torres Straits (see Kuper, 1996; Stocking, 1983b; Urry, 1972). Either through these men or his own initiative, Malinowski soon got to know the other members of England’s leading circle of ethnologists¹⁵ who collectively came to be called the “Cambridge School.”¹⁶ He arrived in March 1910 and by that summer, presumably on Haddon’s advice (M. W. Young, 2004, p. 68), Malinowski was registered for classes at the London School of

Economics. There he would study under Charles Seligman, who became both mentor and something of a supportive older brother to him (M. W. Young, p. 160).

The first two decades of the twentieth century have been described as a period of re-orientation away from “the Tylorian domination of anthropology,” with its focus on culture and custom,¹⁷ and towards a serious investment in ways of going about collecting and using data (Urry, 1972, p. 48). This was a time when, on both sides of the Atlantic, the field of social/cultural anthropology formally crystallized around specific sets of prescribed methods and the conferring of degrees. Malinowski entered the world of British anthropology soon after embarking on his Pacific islands research, at precisely the moment when the decades-long clamorings for a definitive method were reaching a cusp. In a 1909 meeting of the principals from Oxford, Cambridge, and the London School of Economics, it had been decided that “ethnography” would be the term used for “descriptive accounts of non-literate peoples”—as distinct from the historical and comparative-based ethnology (Radcliffe-Brown, 1952, p. 276; see also note 15).

The cutting-edge movements of the day were toward “intensive work,” which had been outlined thoroughly (against the older standard of survey work) by Rivers in 1913:

A typical piece of intensive work is one in which the worker lives for a year or more among a community of perhaps four or five hundred people and studies every detail of their life and culture; in which he comes to know every member of the community personally; in which he is not content with generalized information, but studies every feature of life and custom in concrete detail and by means of the vernacular language. It is only by such work that one can realize the immense extent of the knowledge which is now awaiting the inquirer, even in places where the culture has already suffered much change. It is only by such work that it is possible to discover the incomplete and even misleading character of much of the vast mass of survey work which forms the existing material of anthropology. [quoted in Kuper, 1996, p. 7]

This passage is significant in demonstrating the extent to which Malinowski’s “research revolution” was already in the thoughts and minds—if not practices—of many of the Cambridge School scholars who mentored him (see Urry, 1972; Langham,

1981). Since returning from the Torres Straits expedition in 1899, Haddon had “busily propagandized” the need for “fresh investigations in the field” conducted by trained anthropologists (Stocking, 1983b, p. 80; see also Haddon, 1903).

Writing in 1912, Robert Marett had stressed that a “conscious method” was needed in anthropology and sociology. Described by Adam Kuper (1996) as “one of the last of the armchair anthropologists” (p. 7), even Marett recognized the merits of *intensive work* and intimate research. Indeed, Marett could have been dictating to his future “secretary Malinowski” (see the following section), just weeks before the latter embarked on his own field research, when he wrote:

[It is] most important at the present juncture that some anthropologist should undertake the supplementary work of showing how, even where the regime of custom is most absolute, the individual constantly adapts himself to its injunctions, or rather adapts these to his own purpose, with more or less conscious and intelligent discrimination. The immobility of custom, I believe, is largely the effect of distance. *Look more closely* and you will see perpetual modification in process. [quoted in Wallis, 1957, p. 790—emphasis added]

As with many myths, Malinowski’s serves the euhemeristic function of deification (see Stocking, 1983b), whereas a thorough examination of the intellectual environment in which he came of age strongly suggests that his pioneering work was more straightforwardly a product of the social forces and prevailing ideas on how to best research, document, and understand (and in many instances ultimately manage) human difference. This minimization of his agency and foresight gets magnified through the facts of how he came to New Guinea and eventually the Trobriand Islands, yet in surprisingly different ways from how the well-rehearsed myth of ethnography’s origins represents it. What is perhaps most telling is the extent to which, although he may have strived to, Malinowski was never successful in separating himself from the colonial impulses that characterized his upbringing and training.

Malinowski’s Journey to the Western Pacific

Even at its most scientific, ethnography is resolutely a human science conducted in a real-world laboratory. As such, the ethnographic enterprise is saturated with circumstances, situations, and personalities that are less anticipated and controllable

than its research reporting typically presents. Tedlock elaborates:

No matter how much care the ethnographer devotes to the project, its success depends upon more than individual effort. It is tied to outside forces, including local, national, and sometimes even international relationships that make research possible as well as to a readership that accepts the endeavor as meaningful. [2000, p. 466]

Often the messiness involved when one (or more) human beings commits to long-term research living among a community of human beings, who ideally and inevitably are continuing along the unforeseeable journeys that are their lives, is either managed through a series of entertaining, at times instructive, but usually incidental anecdotes or kept completely out of the research report. Again, this probably has more to do with accepted conventions of academic legitimacy than it does with particular ethnographers' lack of sophistication in recognizing the variability of their research subjects' lives. Nevertheless, conceived of in this way, the ethnographic project with its unwieldiness and unanticipated turns, has some notable parallels to the tradition of nineteenth-century travelogue reporting that the Cambridge School had been so interested in moving away from. One of the first great episodes along this adventure involves the miscellaneous twists and turns that lead ethnographers to their chosen field sites.¹⁸

In many respects, Malinowski would play the role of "bemused bystander" (M. W. Young, 2004, p. 245) in the sequence of events that led to the start of his 1914 western Pacific fieldwork. He had expressed to Seligman that he was willing to spend up to two years in the field, and, perhaps more diplomatically than intellectually, seemed content to let his various academic patrons—among them Haddon, Rivers, Seligman, and Marett—wrangle over his ultimate destination. It appears that Seligman, with the backing of Haddon, did the legwork of securing two years' worth of funding. The combination of Haddon's influence and Seligman's initiative held sway, and Malinowski's fieldwork was designed as a follow-up study of Seligman's earlier expedition to British New Guinea. Marett is widely credited with securing Malinowski's passage to the Pacific by enlisting him as secretary to the anthropology section of the British Association for the Advancement of Science meeting, which took place in Melbourne that year (Kuper, 1996,

pp. 11–12)—a position that brought with it travel funding.¹⁹

Stocking (1992, p. 242) has outlined the precarious position that Malinowski found himself in following the outbreak of war in Europe. Whereas the Malinowski myth focuses on his "enemy alien" status as a citizen of Austria-Hungary, the most consequential outcome of the Great War's outbreak for Malinowski appears to have been a lack of access to personal funds back in Poland, which placed him at the mercy of local officials and made him dependent on the good will of members of the Australian scientific community.²⁰ The myth of being stranded appears to be a fabrication, for Kuper (1996) contends that "all enemy scientists... were allowed to return to Europe" (p. 12).

Getting Off the Veranda

Malinowski's celebrated epiphany that

the anthropologist must relinquish his [*sic*] comfortable position in the long chair on the veranda of the missionary compound, Government station, or planter's bungalow, where armed with pencil and notebook and at times with a whisky and soda, he has been accustomed to collect statements from informants, write down stories, and fill out sheets of paper with savage texts [Malinowski, 1926/1948, pp. 146–147]

appears to have been inspired by both scholarly ambition²¹ and an interest in preserving indigenous customs that he quite literally saw as threatened by the civilizing mission. The early sections of Malinowski's published *Diary* (1967/1989) illustrate his growing distaste for the missionaries he lived among during his initial field stay on Mailu:

These people destroy the natives' joy in life; they destroy the psychological *raison d'être*. And what they give in return is completely beyond the savages.²² They struggle consistently and ruthlessly against everything old and create new needs, both material and moral. No question but that they do harm. [p. 41]

Malinowski's ire was chiefly directed towards Reverend William J. Saville, with whom he lived as a paying guest.²³ Saville, who with his wife had served on Mailu since 1900, at one point wrote Haddon with his own impressions of Malinowski:

You ask me about Malinowsky (I forget how you write his name)... I must candidly confess that I hope we shall never have to entertain that gentleman again... I admire his enthusiasm for his work, but he spoiled that altogether by not being intelligibly able

to understand that other people also might have a right to interests in which they are much justified and just as likely to be quite enthusiastic as he was about his... Dr. Malinowsky seemed unfortunately to think that our time and that of our people should be given up to him. He very likely did not mean this, but his experience with men seemed to be of the smallest and he was pretty much like a child with a new toy. The problems he was trying to work out were of the keenest interest to me, but the minds of some of us must have relaxations from one subject, by the tackling of others. *Had he been a man, who would enter into the position and minds of another*, whether native or white, he could have got twice as much information in one twelfth of the time. A native is not a class room student, and a native likes a bit of fun and a game, Dr. M. seems to understand neither, nor could he understand anybody who did.

[M. W. Young, 2004, pp. 357-358—emphasis added]²⁴

The described intensity and implied ambition are certainly in line with what we know about Malinowski's personality. Although Saville's account likely contains some embellishments, this early documentation of an observer observed (Stocking, 1983a) is enormously illuminating and offers important lessons for any young, zealous ethnographer. As a beginning researcher, "Malinowsky" made several flawed assumptions. Even prior to the decision to "camp...right in their villages" (Malinowski, 1922/1966, p. 6), his ethnographic fervor would have motivated him to "push research beyond its previous limits in depth, in width, or in both" (p. xvii).

Throughout his early research, Malinowski was regularly reading *Notes and Queries in Anthropology* as well as works directly authored by Rivers (see Malinowski, 1967/1989, p. 30, 64). *Notes and Queries* was the classic Royal Anthropological Institute field guide, by then in its fourth edition, designed to promote "far greater accuracy of detail...in the description of the social institutions of savages and barbarous races" in order to "enable those who are not anthropologists themselves to supply the information which is wanted for the scientific study of anthropology at home" (Urry, 1972, p. 46, 47). It had been produced largely under the direction of Edward Burnett Tylor—the monumental figure of nineteenth century British anthropology—and, in the words of Tedlock, was "filled with ethnocentric ideas and leading questions" (2000, p. 456). Early editions of the handbook were primarily intended for travelers,

merchants, colonial officials, and missionaries, but by the start the twentieth century, as Rivers and others were advocating for an end to "armchair" theorizing and the need for trained investigators conducting long-term field stays (J. L. Myers, 1923), *Notes and Queries* was in increasing demand within academic circles. The 1912 edition, the one that Malinowski brought to the field with him, had been the first to include a general chapter on methods. Thus "Malinowsky," being both ambitious and new to field research and making the critical mistake of thinking that natives represented "walking data," might have earnestly followed the direction of this research guide and, as Saville's note suggests, immediately sought to question the Magi (people of Mailu) on anything and everything possible.²⁵

Rivers, who introduced many of the methodological innovations into the 1912 edition of *Notes and Queries*, was progressive enough in his thinking to advocate the importance of narrative inquires that allowed interviewees "to talk freely on subjects or independently to volunteer information" as opposed to direct questions and answers (Urry, 1972, p. 51).²⁶ Yet there was a conspicuous gap between Rivers' ideas regarding best research practices and what he actually did in the field. For example, Rivers' most recognized contribution to anthropology, then and now, is a highly structured genealogical method—used by Malinowski (see 1922/1966, p.14)—which most certainly encouraged direct questioning and answering (Stocking, 1983b). Furthermore, many key tenets of Rivers' "intensive study," for instance the importance of studying native customs "by means of the vernacular language" (see "Malinowski encounters the Cambridge School"), were practices he did not follow himself. Rivers also very much stayed on the verandah. For example, in his celebrated "several months" (M. W. Young, 2004, p. 162) of field research among the Todas of Southern India, which Stocking (1983b) regards as his only research attempt that verged on "intensive study" (p. 89), Rivers stayed in the resort station house, which "catered to the needs and past times of colonials" (Singer & Dakowski, 1986a). One can imagine the "Rider Haggard of anthropology"—as Malinowski referred to Rivers (Stocking, 1998, p. 268)—sipping whisky and soda as he went about filling his many sheets of paper with "savage texts."

Rivers' failure to act upon his own ethnographic innovations, in my reading of this history, justifies his secondary status. Marilyn Strathern (1987) warns us that ideas alone can be deceptively

ambiguous; what matters is practice, or the “effectiveness of the vision [and] the manner in which an idea [is] implemented” (p. 253). This insight is no less true today than it was a hundred years ago. You could even, quite easily I believe, make the case that, with the expansion of higher education and most particularly academic publishing, the pressure to present a new idea, to say something different from what has come before, has increased exponentially. Thus the need—every five years it seems—to announce a new “historical moment” along the qualitative research timeline (Denzin & Lincoln, 2008; see also note 1). Without neglecting or condoning the now well-documented and discussed wrongs of ethnography’s past,²⁷ the most novel of practices for moving forward may involve the reinvestment in and scholarly extrapolation of the merits of the pioneers.

At the start of his time in Mailu, Malinowski would have likely been situated somewhere between the ideals his mentor preached and the actualities that he practiced. The novice researcher’s colonial temperature can be gauged from the inventory of supplies he purchased prior to leaving Europe, which (among the expected medicines, first-aid and camping supplies) included tins of sliced bacon, jugged hare, roast turkey, kippered herring, lobster, oysters, Swiss cheese, Dutch beans, Spanish olives, Suchard’s vanilla chocolate, Peter’s milk chocolate, six different jams, dried fruit, biscuits, and morning tea, two bottles of French brandy, an “oil-cotton coat with special collar and sou’wester,” a “Cawnpore sunhelmet complete with oilskin cover,” two pairs of “light-coloured puttees,” two pairs of “colonial boots,” two Norfolk jackets and breeches, two-dozen “custom-made” notebooks, nine writing pads, three-bottles of ink, six dozen wax cylinder records, a quarter-plate Klimax camera, and a single toothbrush (M. W. Young, 2004, pp. 264–267).²⁸ One should take care to consider this list in its proper historical context—that is, early-twentieth-century England—and certainly Malinowski’s mentors had a hand in advising him on what to take. The point is that coming from a context that represented the pinnacle of coloniality, despite his dislike for missionaries and misgivings about the colonial enterprise, it would have been impossible for Malinowski to be anything but colonial.²⁹

Ethnography’s Colonial Impetuses

Malinowski’s list of fieldwork necessities gives us pause to consider what tools and luxuries ethnographers take with them to the field. More than

a delineation of specific items—although certainly the technologies of research demand some consideration of these—this issue is more productively explored by reflecting on the relationship between researcher and research communities, and how what ethnographers choose to take comes to define them.

In Malinowski’s time, ethnography was unmistakably a colonial project with the quality and distribution of ethnographic knowledge conforming to the borders of empires (Thornton, 1983). Its continuities with European expansion are unmistakable. According to C. Loring Brace (2005), perceptions of categorical differences between groups of people—which we can consider in terms of both physical and cultural differences—emerged with advancements in nautical technology and navigational capabilities starting in the fifteenth century. Where prior travel, whether by land or coast-hugging ships, occurred in increments of twenty-five miles or less, developments in maritime machinery and knowledge enabled travelers to set out from a port in one location and arrive in destinations where people and lifeways looked drastically different. Magnified through Age-of-Exploration demands for increased trade to support Europe’s growing populations and industries as well as Enlightenment emphases on rationality and scientific understanding (Robinson, 1983/2000), accounting for human difference became an important vocation.

This effort to understand and explain differences in how people looked and lived is very much at the heart of what was thought of as anthropology during the eighteenth and most of the nineteenth century. By the close of the latter, with the project of colonial conquest reaching its apex in Africa, Asia, and the Pacific, the endeavor to document the lifeways of different social groups was seen as serving the multiple purposes of mapping human social evolution—primarily as a means of rationalizing imperial dominion—recording rapidly changing cultures, and figuring out how to better administer colonial subjects. Through the efforts of members of the Cambridge School and cross-Atlantic counterparts associated with the Bureau of American Ethnology in the United States (Judd, 1967), travel and voyages were scientificized as expeditions, and explorers, once the instruments of data collection about supposed “savage” ways of life, were replaced by ethnologists, ethnographers, and other types of anthropologic fieldworkers. Likewise the travelogue gave way to the ethnographic manuscript (Urry, 1972; Thornton, 1983).

The fact that in many cases—Malinowski’s certainly being one—there were enough colonial

agents already present in a remote field site to cast most ethnographers as familiar (i.e., a typical white person), and only circumstantially as oddities, is a telling comment on the lack of field work isolation even during this early period.

Indeed Robert Lowie gives an amusing account of once being accosted by a young Crow Indian about his business on their reservation. When Lowie, attempting to explain the business of anthropology with childlike simplicity, said, "I am here to talk with your old men to find out how they used to hunt and play and dance," the young man—who apparently had never been off the reservation—replied, "Oh, I see, you are an ethnologist" (Lowie, 1959, p. 60).³⁰ This can be contrasted against situations in which community members have no understanding of what an ethnographer is or does and therefore make sense of a researcher's presence through their own cultural frames of reference (Pouwer, 1973; McLaren, 1991). Although Lowie's work on the Crow reservation took place long after the (idealized) first-contact situation, it speaks to the extent that ethnographic researchers were in many cases fixtures of a larger imperial apparatus.

The emphasis on studying small-scale "non-Western" societies—either in the interest of documenting what were erroneously thought to be rapidly disappearing cultures (Hallowell, 1960/2002) or as a means of offering profitable cross-cultural comparisons through presentations of values and practices that were sufficiently distinct from the researcher's own—curtailed ethnographers' interest in fitting in. For such societies were usually located on the frontier of imperial expansion: for nineteenth-century America, they were communities of native peoples in the manifest destinations of the territories to the west; for Europeans (most notably the British), they were in Africa, India, and the islands of the Pacific.

Reflecting on the rational standpoint that, at the time, was considered essential to these cross cultural investigations, Johannes Fabian (2000, p. 7) remarks on the varying amounts of "protective equipment" that aided pseudo-scientific travelers in maintaining physical and intellectual distance. Certainly the "necessities" that researchers take with them into the field and the decisions they make about how to present themselves should be considered legacies of this endeavor. Malinowski's list shows an obvious lack of concern with integrating and perhaps the intention of presenting his colonialist superiority, possibly even to the other Westerners who were already there. He might not be blamed since

the level of integration—or more precisely the level of isolation from the contaminating influence and company of white men—he ultimately aspired to was unprecedented within the Cambridge School.³¹ Malinowski was not interested in presenting himself as a native. He was interested in "wak[ing] up every morning to a day, presenting itself to [him] more or less as it does to a native" (Malinowski, 1922/1966, p. 7). He had no desire to become a Trobriander but rather an intense desire to take on a native standpoint. What he appears to have understated is any consideration for the extent to which his self-presentations hindered his efforts to cease being "a disturbing element in the tribal life" (p. 8).

This is in stark contrast to later ethnographies, particularly in the postcolonial era, where researchers and the communities they study do not look, and in fact might not be, all that different. Today we see more conscious efforts on the part of researchers to present themselves in fashions that facilitate their fitting in, and, one may presume, to conceal those aspects of their personalities or those day-to-day "necessities" that most strikingly mark them as different. For example, in his research among Arab professionals in turn-of-the-twenty-first-century Brazil, John Tofik Karam (2007) took several intentional steps to polish his appearance in the interest of meeting the expectations of the people he worked among, these included upgrading his wardrobe and cutting his dreadlocked hair.³² Comparatively, in my own research among underground hip hop musicians (Harrison, 2009), I very consciously wore my hair in dreadlocks and purchased a book bag displaying a fashionable hip hop label, which helped mark me as someone involved in the scene.

An Intersubjective Science

As research practitioners, ethnographers intrinsically operate in the physical, social, and psychological spaces of the in-between. This position is reflected in ethnography's guiding vantage point, participant observation, which is regularly (although erroneously) equated with the methodology itself. Ideally, the classic ethnographer was at once a participant and an observer. Such liminality extends from personal situatedness to the realm of societal belonging. Although ethnographic writings frequently celebrate instances of researchers being accepted by, and thus belonging to, the communities they study, these relationships are in most cases conditional. In fact, in classic ethnographic discourse it was just as common for a community to be represented as belonging to the researcher (i.e., "my village" or

“my people”) or for there to be suspicions surrounding an ethnographer “going (too far) native.” Even in instances where researchers choose to study the communities they belong to—referred to as native ethnography³³—the acts of conducting research can serve to extract the researcher from their community in meaningful and potentially consequential ways (see Brayboy & Deyhle, 2000). Furthermore, there is a popular anthropological wisdom, which I believe has more than a shred of truth to it, suggesting that those most drawn to the discipline have difficulty fitting in within their own societies.

I mention all this to shed greater contextual light on the interpersonal negotiations that ethnographers must persistently grapple with. The everyday practice of ethnographic participation, observation, inquiry, and engagement marks another zone of in-betweenness where relationships, understandings, and methodological scripts are never settled. In this regard, the ethnographer is a perpetual improviser and social bricoleur, both “adept at performing a large number of diverse tasks” (Lévi-Strauss, 1962, p. 17) and cobbling together a social role out of whatever unexpected rapids the stream of ethnographic experience holds. As such a strict set of prescribed methods simply does not suffice.

Ethnographic research is dialogic, intersubjective, and intrinsically incomplete (Kondo, 1986). Its multiplex methods start from an act of intervention into the fabric of daily life in which the researcher—their presence and behavior—is continuously being interpreted by the fashioners of the social world they wish to examine (Williams, 1996). At times this negotiation of observation and presentation compels researchers to subordinate certain aspects of their identities (Tsuda, 1998) or to embrace the idea that the research process can be transformative for both ethnographers and members of the communities they work within (D’Amico-Samuels, 1991/1997). Peter McLaren insists that contemporary field researchers must consider the conditions and ends to which they “enter into relations of cooperation, mutuality and reciprocity with those whom [they] research” (1991, p. 150). Questions of who the ethnographer is and what their business is within the community are part and parcel to this process. This can lead to specific inquiries regarding sources of funding and institutional affiliations, which have the potential to betray ethnography’s more benign characteristics.

Technological Rapport

Another kind of “protective equipment” frequently deployed by ethnographers in the field is

the technologies of recording that they take with them into research. In Malinowski’s case, we see the instruments of writing field notes, namely ink, writing pads, and notebooks, as well as wax cylinders for making field recordings.³⁴ For both their material presence and role in data collection and analysis not to mention their use in maintaining communication with the world beyond “the field,” these instruments can significantly affect the depth and texture of ethnographic relations. Even the activity of field note writing (typically) marks participant researchers as different from members of the community where research is conducted. That is, although the researcher might take part in all the same activities as “natives,” at the end of the day—when “natives” retire to do whatever it is they do—the ethnographer goes home to write about culture (Clifford, 1986; Emerson, Fretz, & Shaw, 1995).

Over recent decades this process of documentation has evolved to include possibly more conspicuous technologies (depending on the setting). Malinowski’s Klimax camera was certainly one of his heftier purchases. The introduction of new technological machinery—for instance iPads or do-it-all smart phones—has the potential to disrupt the everyday life today’s ethnographers seek to observe. Erica Brady (1999) explains how, just as the ethnographer of the early twentieth century became a common part of the typically observed community landscape, these technologies of recording should be understood as things that ethnographic subjects respond to and form relationships with, often as a means of serving their own interest (see Menon, 2010). The miniaturization and global proliferation of technologies (Appadurai, 1990) over the course of the twentieth century has made them increasingly more familiar in all “fields.” Even so, their notable introduction into everyday social settings in which one would not typically find them tends to highlight the researcher–subject dichotomy and extraction-of-data agenda in ways that many contemporary ethnographers would rather minimize. In this interest, various strategies are employed. These range from using jottings as a technique of clandestine field note writing to efforts towards familiarizing research subjects with a piece of recording technology by making it available to them for non-research purposes—for example, allowing children to play on one’s laptop computer prior to using it to record an interview or using a camera to take family photos in addition to more intentionally ethnographic ones.³⁵

In an effort to prioritize equitable social relationships over extractive research ones, some ethnographers

choose to participate more and record less. This is done with the awareness that experiences of recording (for instance witnessing an event through a video camera lens) are distinct and atypical forms of participation with potentially distorting effects. Indeed, even Malinowski recommended that:

it is good for the Ethnographer sometimes to put aside that camera, note book and pencil, and to join in himself [*sic*] in what is going on. . . . Out of such plunges into the life of the native. . . . I have carried away a distinct feeling that their behavior, their manner of being, in all sorts of tribal transactions, became more transparent and easily understandable than it had before. [1922/1966, pp. 21–22]

Of course, this dichotomy gets collapsed within paradigmatic outlooks that recognize the researcher as having a role in actively constructing the social environment they study (see the earlier discussion).

At the same time, many sites of contemporary ethnography are increasingly saturated with technologies of recording—such as smart phones that allow for photography, video, and sound recording all at one time—making the activity of recording and the introduction of a technology nothing particularly out of the ordinary. On the surface this ubiquity of recordings may be viewed as benefiting the project of documenting native life without having the documentation process or technologies disturb its rhythms and fabric. However, this simultaneously introduces new sets of issues. These particularly concern the minimization of traditional ethnographic authority, the extent to which ethnographic research and researchers have become surveillable, and possible conflicts and contradictions surrounding who must (and who must not) adhere to institutional regulations. Ultimately, such developments have the potential to augment, jeopardize, and transform the ethnographic project, perhaps all at once.

At the height of anthropology's "crisis of representation" (see "Malinowski's literary (re)turn"), Geertz astutely commented that, traditionally:

[t]he ability of [ethnographers] to get us to take what they say seriously has less to do with either a factual look or an air of conceptual elegance than it has with their capacity to convince us that what they say is a result of their having actually penetrated (or, if you prefer, been penetrated by) another form of life, of having, one way or another, truly "been there." [1988, 4–5]

An historical arc can be drawn starting from an era when ethnographic accounts, by names like Malinowski and Mead, were seldom challenged on the basis that, quite simply, no other trained researcher had *been there* to a period of ethnographic proliferation where multiple researchers had worked within the same societies. Even accounting for the half century between their studies and the shifts in styles of ethnographic reporting, Annette Weiner's (1976) Trobriand Islands ethnography is notably different from Malinowski's (Jolly, 1992). A few years later, Derek Freeman (1983) was attacking Mead's work in Samoa (1928/1961) on the basis of both her methods and findings.³⁶

In addition, during the post-World War II decades, members of what for lack of a better term might be called "traditionally studied communities" began having a greater presence in anthropology.³⁷ Though there had been a long disciplinary history of native community members working closely with ethnographers, and in some cases being encouraged to publish their own work and/or enter the discipline (Lassiter, 2005), initially such key informants were regarded primarily as tools who through their organic insider-ness could get "the inside scoop" (Narayan, 1993, p. 672). In contrast, the native anthropologist who came of age during that latter half of the twentieth century brought with them "a set of theories based on non-Western precepts and assumptions" (D. Jones, 1970, p. 251) with the critical politics of post-colonialism to support them. Even outside these trained professionals, the one-time omniscient voice of the lone ethnographer who had "been there" was additionally challenged by community members who were often Western educated and had both access to the research that had been conducted on them and avenues for talking back.

These late-twentieth-century challenges to ethnographic authority are magnified in the early twenty-first century context of widespread social and data-based documentation, social networking, and what Andrea Fontana and James H. Frey (2000, p. 647) refer to as "interview society." Social media—for instance, a YouTube video of an event that has been posted and commented on for months prior to the time necessary for a peer-reviewed publication—makes it possible for virtually anyone to feel as if they have *been there*.³⁸ As John L. Jackson (2012) has recently pointed out, under many of today's ethnographic conditions it is quite easy to follow a researcher's backstage activities. Furthermore, from blogs to online (customer)

reviews of ethnographic texts, the possibilities for public comment have enabled research subjects, as well as everyday people, to engage in public dialogues about research. In short, modes of ethnographic inquiry and reporting are no longer the exclusive province of trained academics (Holmes & Marcus, 2008), with both the process of research and the scrutiny of research reporting open to wider circles of participants.

Jackson describes the “internet as a mechanism for humbling ethnographic voyeurism” (2012, p. 495). Indeed, the emergence of these new modes of dialogue may mark the future of ethnography, but the multitude of voices and the potential for rhetoric (particularly among those with little to no social research background) to trump careful reflection and grounded analysis within the public domain may signal the amplification of what some already regard as an unproductive methodological quagmire.

That academic ethnographers, on the basis of their training, disciplinary identities, and institutional affiliations are required to follow not only important ethical principles but also institutional regulations—most notably in the form of institutional review board (IRB) compliance—which often appear to be more interested in protecting the institution from lawsuits than in protecting human subjects (Lincoln & Tierney, 2004), creates further complications in an age when the conducting and broadcasting of personal studies is so pervasive. Following the 1970s “Belmont Report” (1979), IRBs were set up to “ensure freedom from harm for human subjects, to establish the likelihood of beneficence for a larger group (of similar research participants), and to ensure that subjects’ consent to participate in research is fully and authentically informed” (Lincoln, 2005, p. 174). Where human subjects’ protections were initially directed towards research in health, they were soon applied to all interactive research on people. Among qualitative researchers there have always been question regarding IRB regulations’ applicability to studies as benign as oral histories or as unpredictable (i.e., difficult to outline in an IRB protocol) as ethnography,³⁹ as well as concerns about the ability of IRB members—most of whom come from the “harder sciences”—to understand and appreciate what ethnographers do. One constant case for comparison, which perhaps most effectively brings to light many of the grievances of contemporary ethnographers operating in environments of ubiquitous social documentations and media, is with journalists who in many ways operate similarly to qualitative researchers but are

not bound to the same ethnical principals or, more importantly, regulatory constraints.

Malinowski “Checks Out” the Trobriands

Malinowski’s regulatory constraints seem to have been few. He appears to have arrived at his ultimate ethnographic destination—the Trobriand Island of Kiriwina—somewhat serendipitously. What started as a one-month stop along the way to New Guinea’s northern coast—“to get an idea of what was going on [in the Trobriands],” he reported to Seligman (who presumably wanted him to go elsewhere), assuring him that the stay was only temporary (Stocking, 1992, p. 249)—resulted in “about two years” (Malinowski, 1922/1966, p. xvi) of field research. Yet this escape from colonially infested Mailu to the uncontaminated Trobriands was not as isolated as the “off the verandah” legend and Malinowski himself portray it. Early in *Argonauts’* famous first chapter, Malinowski outlines the *proper conditions for ethnographic work*:

It must be far enough away [from the company of other white men] not to become a permanent milieu in which you live and from which you emerge at fixed hours only to “do the village.” It should not even be near enough to fly to at any moment for recreation. *For the native is not the natural companion to a white man*, and after you have worked with him [*sic*] for several hours . . . you will naturally hanker after the company of your own kind. But if you are alone in a village beyond reach of this, you go for a solitary walk for an hour or so, return again and then quite naturally seek out the natives’ society, this time as a relief from loneliness, just as you would any other companionship. [pp. 6–7—emphasis added]

Stocking (1992) refers to Malinowski’s “aleness” among the Trobrianders as “relative rather than ‘absolute’” (p. 251). Should he have had a hankering, Malinowski could seek the company of his “own kind” just a few miles away. At the time Malinowski arrived on Kiriwina looking to pitch his tent, the largest Trobriand Island had both a hospital and jail; moreover, its resident magistrate had recently “persuaded” the Kiriwinians to line the paths of the island with 120,000 coconut trees by “imposing stiff penalties for failure to do so” (Stocking, 1992, p. 249). Seligman had already conducted some preliminary fieldwork there and, as Michael W. Young explains, the Trobriands had developed quite a reputation among colonial observers for its unique virtues—not the least of which surrounded the burgeoning popular image

of its “chiefly aristocracies and exotic dancers” as “part noble savage[s], part licentious sybarite[s]”⁴⁰ (quoted in Stocking, 1992, p. 249). One of these early observers was travel writer Beatrice Grimshaw, who nominated Kiriwina as “among the most civilized” places in British Papua New Guinea (M. W. Young, 2004, p. 380). Malinowski has been credited for shifting the anthropological lens from searching for and trying to represent pure cultural forms to understanding societies in the context of colonially induced change (Kluckhohn, 1943; Fardon, 1990). Yet from his impetus to get away from missionaries to the appeal of “Trobriand beauties,” Malinowski’s efforts to extol the virtues of his new methodology appear to be lodged in the allure, albeit a fabricated one, of the pure and untouched exotic.

Alternative “Fields”

Traditionally the ethnographic “field” has been conceived of as remote, non-Western, and to some degree exotic. This was largely a remnant of evolutionary anthropology’s emphasis on comparative (cross-cultural) analysis through holistic examinations of small-scale societies that differed significantly from the West. Yet there are important ethnographic traditions, mostly coming out of sociology, that were notably closer to home. W. E. B. DuBois’s late nineteenth century resident study of Black life in Philadelphia, published as *The Philadelphia Negro* (1899/1973), should be considered one of the earliest examples of urban ethnographic study.⁴¹ Though much of DuBois’s research consisted of detailed questionnaires to residents of Philadelphia’s Seventh Ward, his taking up residence “in the heart of the community to be studied” (Aptheker, 1973, p. 6), his regular house to house visits to virtually all the homes in the ward, and his propensity to align with the Black people of Philadelphia and, at times, stand in militaristic opposition to what was at best a stance of paternalistic benevolence held by the project’s sponsors, retrospectively marks the Philadelphia study as profoundly ethnographic. DuBois would go on to do similar field research throughout the South while at Atlanta University (1898; 1903/1996).

Far and away the most celebrated ethnographic traditions practiced outside of anthropology came from a collection of researchers associated with the University of Chicago department of sociology. The “Chicago School,”⁴² in a general sense, formed around the combined influences of Malinowskian fieldwork methodologies and German

phenomenological theory (J. S. Jones, 2010). Through their conceptualization of urban life as an assemblage of “natural areas” or “little communities,” researchers affiliated with the Chicago School, under the direction and/or influence of scholars like Robert E. Park, W. I. Thomas, E. W. Burgess, and later Everett Hughes and Herbert Blumer (Becker, 1999; Vidich & Lyman, 2000) imagined the city as a social laboratory through which to examine secular differences—primarily oriented around ethnicity and various forms of “civic otherness.” Between the 1920s and the early 1960s, the Chicago School released a series of ethnographic studies of specific aspects of urban life. Among the most notable were Nels Anderson’s (1923/1961) sympathetic account of the life of the hobo, Frederick Thrasher’s (1927) pioneering work on the urban geography of gangs, Louis Wirth’s (1928) historically informed study of the social isolation of ghetto life among Jewish immigrants, several important studies of Black urban life by E. Franklin Frazier (1932; 1939; 1957) and St. Claire Drake & Horace Clayton (1945/1993), and William Foot Whyte’s “participant observation” among Italian American youth residing in Boston’s North End (1943/1981). Despite their more proximate ethnographic settings, most of these works conformed to the anthropological tradition of otherizing by focusing on “urban groups whose ways of life were below or outside the purview of the respectable middle class” (Vidich & Lyman, 2000, p. 49).⁴³ Indeed, when Howard Becker described the virtues of the “Chicago way” as having “all the romance of anthropology but [you] could sleep in your own bed and eat decent food” (1999, p. 8), we can imagine a romance different from Malinowski’s with Annie Brunton and all things British, and rather resembling the intrigues which drew him to Kiriwina or, for that matter, might draw a Swedish tourist to attempt personal studies in 1970s Harlem.

Other notable studies that employed “the approach of the cultural anthropologist” to what could be described as closer-to-home communities in more than just a geographic sense include Helen and Robert Lynd’s (1929/1956) study of a compact, homogenous, representative American city—“*Middletown*,” also known as Muncie, Indiana (see also Lynd & Lynd, 1937); August B. Hollingshead’s “typical midwestern community,” “*Elmtown*” (1949/1975); and W. Loyd Warner’s *Yankee City Series* (see Warner, 1963). Despite the classic place of these middle-of-the-road American ethnographic studies in sociological history (Gillin, 1957), both the Lynds’ study of Muncie and Warner’s “Yankee

City,”—which was known to be Newburyport, Massachusetts—received considerable criticism.⁴⁴

One of the more remarkable critiques of the Middletown studies came from Dr. Hillyer Hawthorne Straton, minister of the First Baptist Church of Muncie and a neighbor of one of the families that was prominently featured in the Lynds’ study. Straton’s ten-page, typewritten manuscript, written in 1937 and eventually published by Robert S. La Forte and Richard Himmel (1983), I believe, is consistent with many of the later “native criticisms” of anthropology. Straton chides Robert Lynd for “fail[ing] to live up to... [the] standard of ‘[t]he social scientist,’” citing a local columnist comment that “[The Lynds] came here with a preconceived notion of what Middletown should be.... Blind to everything else” (La Forte & Himmel, 1983, p. 255). He is particularly critical of the Lynds “propensity for anything that is radical, ‘new-dealish,’ or liberal” (p. 261) and in one telling passage questions the credentials of a critic who hailed the book for its sociological accuracy, arguing “How he knows is a puzzle for he has never *been here*” (p. 255—emphasis added). The critical lens brought to many of these early-to-mid twentieth century ethnographic studies of middle America anticipated the critiques from abroad that emerged as more “traditional” ethnographic subjects gained knowledge of how they were being represented and had the platforms and impetuses to say something about it.⁴⁵

Disappearing “Fields”

Several of the previously outlined historical developments that impacted relationships between ethnographers and members of the communities they study also worked to collapse the once comfortable division between “home” and “the field.” Time and space compressions (Harvey, 1991), accelerated by heretofore unconceivable levels of global interconnectness and telecommunications ubiquity exposed the lines separating the field, the academy, and everyday life as artificially imposed classifications (Wilk, 2011). Whereas previous ethnographic conventions foregrounded the significance of place—especially when activated through the classic “arrival story”—as essential to establishing the identity and authority of ethnographer as having “been *there*,” which had to be *somewhere*,⁴⁶ by the close of the last century, innovations in how ethnography was being conceptualized, particularly within anthropology, sought to dislocate and deconstruct the traditional notion of a discreet ethnographic “field” (Gupta & Ferguson, 1997). George Marcus (1995; 1998), for example, advocated mobile, multi-site ethnography

as a way of both rethinking methods and theories within globalized contexts and accounting for life ways that were fundamentally embedded within global systems (see also Appadurai, 1990; Stoller, 1997; Hannerz, 1998). In doing so, Marcus was particularly attentive to the strides that had been made within interdisciplinary fields like media studies, cultural studies, science and technology studies, and migration/diaspora studies.⁴⁷

Certainly the notion of a traditional, fixed “field”—itself a product of a colonial worldview—obscured many of the realities of contemporary fieldwork. Thus, many scholars (including several cited earlier) argue that clinging to such spatialized understandings is not only limiting but potentially nonproductive (Caputo, 2000, p. 29). Politically, the notion of a traditional “field” produces and sustains the role of academia and other at-home institutions as the “exclusive site[s] of shaping, directing, and informing the research agenda” (Rogers & Swadener, 1999, p. 437); the “out there” field remains as the place where those directives get carried out. In challenging this history, Akhil Gupta and James Ferguson assert that ethnography’s once well-established sense of location “becomes a liability when notions of ‘here’ and ‘elsewhere’ are assumed to be features of geography, rather than sites constructed in fields of unequal power relations” (1997, p. 35).

A decade into the twenty-first century, we see not only a blurring of distinctions between home and the field but, for many researchers, corresponding collapses between research and everyday life. Whereas quite recently these disappearing physical and mental spaces were thought to engender a schizophrenic existence (Hoodfar, 1994; see also Caputo, 2000), many ethnographers today, schooled in the vocabulary and conception of multitasking, would agree with Richard Wilk’s assertion that ethnography “takes the unruly business of life through a series of operations which produce an orderly narrative”:

It is not so much a stage as a process, and in reality it is always going on, because we are never simply recording what we see like cameras or voice recorders. We are interpretive instruments, and we are engaging with ethnography when we move any experience from our senses to our pen or keyboard. [Wilk, 2011, p. 24]

An Ethnographer of Ethnographic Practice

In 1922 when *Argonauts of the Western Pacific* was published, it was hailed by none other than Frazier himself as a “remarkable record of anthropological

research” by someone who had “lived as a native among the natives” (J. G. Frazier, 1922/1966, p. vii). For his part, Malinowski was exceedingly deliberate in foregrounding his methodological “innovations.” Despite mixed reviews, most notably some unfavorable ones coming out of England (Leach, 1965/2000a), the myth of Malinowski—as the first field researcher to voluntarily remove himself from colonial quarters, (essentially) cut off all ties with “civilization,” and immerse himself in the world of savages as a methodological imperative for understanding both their world and worldview—soon took legs. His oft-quoted summation, found on the penultimate paragraph of *Argonauts*’ first chapter, stated that the ultimate goal of the ethnographer was “to grasp the native’s point of view, his [*sic*] relation to life, to realize *his* vision of *his* world” (Malinowski, 1922/1966, p. 25). The prescriptive methods for doing this included long-term residence by a trained researcher, learning the local language rather than relying on interpreters, collecting as much data as possible on as wide a range of activities as possible—from the spectacular and ceremonial to the everyday and mundane—and taking copious field notes, and, when possible, partaking in social activities as a “participant-observer.” From all that I have outlined already, it should be apparent that Malinowski’s status as the “inventor” of these practices is disputable if not improbable. But more than anyone in England at the time, he took up the challenge of theorizing them through practice and was, furthermore, immodest in broadcasting his achievements. Together Malinowski’s prescriptions amounted to a methodological manifesto (Strathern, 1987, p. 258; see also Stocking, 1992, p. 62) that championed contextualization, holism,⁴⁸ and the distinction between ideal and actual behavior as signaling the capacity for agency within social structures.

In this respect, Malinowski’s title as the progenitor of ethnography is in some ways legitimate. Where scholars like Rivers and Marett were forthright in producing ideas regarding the correct methods for conducting qualitative research across cultures (see “Malinowski encounters the Cambridge School”), Malinowski more so than any Cambridge School scholar before him formulated his ideas through involving himself in activities of participant observation. In other words, his understandings of proper ethnography were experientially informed in the same way that ethnography as a methodology requires experiential realizations.

* * *

In the early pages of *Argonauts*—dedicated to “Subject, Method, and Scope”—Malinowski (1922/1966) made several prescient dictates that re-emerged during the late-twentieth-century ascendance of postmodern, poststructural ethnographic practices and orientations. These included:

- *Methodological transparency*: “an ethnographer, who wishes to be trusted, must show clearly and concisely . . . which are his [*sic*] own direct observations, and which the indirect information that form the bases of his account.”⁴⁹ (p. 15)
- *Researcher subjectivity and (his solution) the importance of keeping a diary*: “As to the actual method of observing and recording in fieldwork these *imponderabilia of actual life and of typical behavior*, there is no doubt that the personal equation of the observer comes in here more prominently, than in the collection of crystalised ethnographic data . . . An ethnographic diary, carried on systematically throughout the course of one’s work in a district would be an ideal instrument for this sort of study.” (pp. 20–21—emphasis original)
- *Embodied knowledge cultivated through engaging the rhythm of research*: In order to “get . . . the hang of tribal life” (p. 5), “I had to learn how to behave and to a certain extent, I acquired ‘the feeling’ for native good and bad manners. With this, and with the capacity of enjoying their company and sharing some of their games and amusements, I began to feel that I was indeed in touch with the natives, and this is certainly the preliminary condition of being able to carry on successful field work.” (p. 8)

Aside from the unintended publication of his *Diary* (1967/1989), which made previously veiled aspects of his field experiences transparent, I hesitate to champion Malinowski as a researcher who practiced all that he preached. Nevertheless, students of ethnography would be wise to note that these important aspects of how ethnography has been conceived of and conducted were articulated by Malinowski only after his informative experience conducting fieldwork.

Malinowski’s Literary (Re)turn

Richard Fardon notes how following a period—which he dates to the 1970s—when emerging trends in critical and radical ethnography treated Malinowski as “definitively superseded or encompassed” (1990, p. 573), a new wave of scholarship, much of it coming out of the United States,

resurrected his significance. For this next generation of ethnographers, Malinowski's value, or more precisely the value of his "charter myth" (M. W. Young, 1988, p.1), lay in the braided inheritances of the Malinowskian method of research, theory of culture, and style of ethnographic reporting (Fardon, p. 574). The most recognized of these "Malinowskian children" (Geertz, 1988) were collectively cast under the label "postmodern ethnographers" with their craft deemed, alternately, "the new ethnography," "reflexive ethnography," "critical ethnography," or simply "postmodern ethnography."⁵⁰ Though the postmodern label, which has been criticized for obscuring more than it says (Pool, 1991), was not always embraced by those who felt it imposed on them, these scholars generally shared a number of orientations to their ethnographic practice, including an interest in deconstructing, decentering, and juxtaposing the coherence of established ways of knowing (Fardon, 1992, p. 25); a reflexive outlook on the position of the researcher relative to the community of study; concern for the constructed nature of ethnographic authority (Clifford, 1983); and attention to language, texture, and form in modes (primarily literary) of ethnographic representation (Clifford, 1986).

These paradigmatic shifts, which significantly impacted how ethnography today is thought of and practiced, have been credited to various late-twentieth-century "moments" including the publication of Malinowski's field diaries (1967/1989), important interventions from feminists and indigenous researchers (Mascia-Lees, Sharpe, and Cohen, 1989; Wolf, 1996; Harrison, 1997),⁵¹ as well as the arrival of seminal works such as Dell Hymes' *Reinventing Anthropology* (1972) and Geertz's *The Interpretation of Cultures* (1973). In contrast to this revolutionary moment model, Nash and Wintrob (1972) document how, as early as the 1950s, within anthropology an ethnographic self-consciousness emerged that challenged the discipline's naively empirical aspirations towards attaining "full-fledged scientific status." Several significant works—such as Claude Lévi-Strauss's autobiographical *Tristes Tropiques* (1955/1992) and later Gerald Berriman's *Behind many Masks* (1962)—reflected the integration of symbolic interactionist thinking into conceptions of research as process. They credit these shifts to: (a) global forces that resulted in the crumbling of the colonial regime's that anthropology had come of age under and the creation of globally-informed and post-colonially-critical (traditional) ethnographic subjects; and (b) changes

within the discipline of anthropology, including multiple studies of the same culture and a greater range of people and "personality types" becoming ethnographers (p. 530).

The ascension of the postmodern—which reached its zenith in 1986 around the publication of Marcus and Michael Fischer's *Anthropology as Cultural Critique* (1986) and Clifford and Marcus's edited volume *Writing Culture* (1986)—coalesced around a political stance concerned with questioning the terms of Western hegemony, an appreciation for the (inter)performative nature of ethnographic research and the (inter)subjective nature of ethnographic analysis, and a focus on ethnographies as texts (Marcus & Cushman, 1982).

Literal Postmodern Politics

Responding to what Mascia-Lees et al. (1989, p. 8) describe as "the need to claim a politics in order to appeal to an anthropological audience," the harbingers of postmodernism adopted (or appropriated) critical stances previously propagated by feminist, (to some extent, native⁵²) and indigenous ethnographers. Their insufficiency in crediting these positioned influences was striking given that so many of the key scholars associated with postmodernism were white males. This critique was most poignantly raised by feminist scholars who observed that "[l]ike European explorers discovering the New World, [postmodernists] perceive a new and uninhabited space where, in fact, feminists have long worked" (Mascia-Lees et al., p. 14). Indeed, where women and native ethnographers have always occupied marked positions along the axes of gender and ethnicity/race, white male researchers, as unmarked, have historically enjoyed the privilege of claiming objectivity and, quite notably, had their claims accepted by their audiences (Alsop, 2004). Postmodern skepticism about the constructed nature of truth claims coincided with a recognition of researcher subjectivity and research serendipity that was, for lack of a better way of putting it, "old news" within feminist and native ethnographic traditions. Both traditions had long questioned the assumption of political allegiance on the basis of common identity ascriptions (see Kondo, 1986; Narayan, 1993), thus compelling their adherents to critically examine the politics and experiences of fieldwork. Far from detached scholars, feminist and native ethnographers recognized their role in shaping the social worlds they participant-observed and described (Geertz, 1988). Such revelatory acknowledgements—not from the margins of ethnographic

practice but, with the rise of postmodernism, coming from its mainstream—supplied the platform for more collaborative, participatory action-based, and arts-based approaches that were to follow (Lassiter, 2005; Finley, 2005; Leavy, 2009).

Writing in the Postmodern Momentum

The most distinguishing aspect of this new ethnography—or the topic that has received the most attention—is the emphasis on the rhetorical processes involved with ethnographic production and, ultimately, the view of ethnographies as writerly projects. This literary turn was not without precedent.⁵³ Malinowski certainly thought of himself as a writer. Writing just after the “founding father” of ethnography’s death, Clyde Kluckhohn speculated that Malinowski’s “capacity for expression” would be one of the key things upon which his reputation would rest (1943, p. 209).⁵⁴ Indeed Clifford (1986) in arguing the partial and constructed nature of truth claims, and advancing the artistic dimensions of ethnography as a project profoundly situated between systems of meaning making, invoked Malinowski on the very first page of his seminal text. Even though the once-dominant aspirations for “hard science” status—marked by formalized methods leading to timeless truths—had been waning for decades, this nod to the humanities and the constructed and interpretive nature of all research was viewed by many as a “crisis” in the field.

Ethnography constructs culture through texts of contexts, which to a certain degree are valued based on their effective presentations. Arguing for what she called an anthropology of “persuasive fictions,” Marilyn Strathern suggested that ethnographers impact imaginations through relationships *internal to the text*: “the kind of relationship that is set up between writer and reader and writer and subject matter” (1987, p. 256). Stephen Tylor expressed it somewhat differently in asserting that “the critical function of ethnography derives from the fact that it makes its own contextualization part of the question” (1986, p. 139). Inspired by this wisdom, my conviction for some time now has been that ethnography, both as research and representational practice, operates in an adverbial mode (see Hammersley, 2008). It contextualizes transmutable and transposable social processes through transcriptions of the dynamic social interactions of community members and researchers. As such, the experiences of ethnographic fieldwork are (re)constructed through the process of writing first field notes (Emerson et al.,

1995) and later ethnographic monographs. Such recognition, of the mediated expressions of social processes and meanings, through acts of composition (literal and otherwise), has sprouted into a tremendous range of experimental ethnographic forms and new political possibilities—thus leaving ethnography’s horizons promising and bright.

His(torical) Legacies

Constructing a complete picture of Malinowski—the man, the field researcher, and the scholar—presents special difficulties, not the least because he was a creative intellectual with “an open and lively mind” (Flis, 1988, p. 123) whose scholarly career can be characterized as much by evolution as by stasis (Murdock, 1943). He furthermore had a penchant for flamboyance in both representing himself and the world around him. Part of this involved embracing the great storytellers’ wisdom that the context of a telling dictates the text of the tale. In this vein, it would not be too much to characterize Malinowski as having a loose interpretation of the “facts” regarding his own personal history, which he would strategically adjust to delight or in some other way influence his audience (M. W. Young, 2004). He was a master of the sketchy, revisionist memoir, which, combined with an erratic temperament that made even his journal entries and personal correspondences knavishly unreliable (Rapport, 1997), resulted in an enigmatic and elusive biography fitting of mythic status.

Malinowski was obviously aware of the pioneering nature of his work—or at the very least the potential to frame it that way—and quite concerned with his legacy. He was in essence what sociologist Gary Fine (1996) would refer to as a self-entrepreneur of his own reputation. Fine’s notion of *reputation entrepreneurs*—that is, “self-interested custodians” of someone’s historical reputation (p. 1162)—is useful for contextualizing Malinowski’s historical import and for making sense of how and why the myths surrounding him have been so enduring. As an analytic concept, reputation entrepreneurship is premised on a constructionist model of history that frames it as the outcome of sociopolitical struggles over power, prestige, and resources (Fine, 2001, p. 8). Fine specifically investigates the role of social agents in shaping the collective memory and settling discourses that surround historical figures. This can involve recognition within one’s field—in Malinowski’s case, anthropology and other scholarly fields that position ethnography at or near their core—and renown outside of it.

In addition to his achievements and how he represented them, Malinowski also laid the groundwork for future custodians of his reputation despite his untimely death from a heart attack at age fifty-eight.⁵⁵ For example, his propensity to keep journals provided the source materials for future biographers—although it is widely believed that his *Diary* (1967/1989) was never intended for publication. Young (2004) recounts how during his days in Leipzig, Malinowski exhorted himself to “Keep a diary!”; adding, “Everything that passes through me must leave a lasting trace” (p. 131). His published *Diary* similarly includes statements to this effect. In 1926 Malinowski wrote that myths “record singularly great achievements... redound to the credit of some individual and his [*sic*] descendants or of a whole community; and hence they are kept alive by the ambition of those whose ancestry they glorify” (1926/1948, p. 106). The extent to which this was true for a lot of Malinowski’s student-descendants was evident by their support of him following the controversial publication of his diaries in 1967.⁵⁶

Had Frank Hamilton Cushing had better reputation entrepreneurs, or been more organized (Brady 1999) and less prone to making enemies (Kolianos & Weisman, 2005), he might hold a status comparable with Malinowski’s. In the United States, where the objects of anthropological study—minimally defined by William S. Willis Jr. as “dominated colored peoples... living outside the boundaries of modern white societies” (1972, p. 123)—were closer at hand, research expeditions along the order of Torres had a longer history. Thirty-five years before Malinowski, Cushing had “developed” his own “reciprocal method” of field research (Mark, 1980, p. 123), when he decided to forsake his position as the Smithsonian Institute representative on the 1879 Bureau of (American) Ethnology’s first-ever southwestern expedition, in order to take up residence with the Zuñi Indians. Apparently, after becoming frustrated “at how little he could learn as an outsider” camping outside the pueblo, he “soon abandoned the tents of his colleagues and... moved in with the Indians” (Green, 1979, p. 5). Cushing lived among the Zuñi for four and half years, during which time he dressed like a Zuñi, was given a Zuñi name, became proficient in the language, took an active part in both ceremonial events and daily life, was adopted into the Dogwood clan, became a member of the tribal council, and was initiated into the Priesthood of the Bow (Pandey, 1972; Hinsley, 1983). Dubbing him the “original participant observer,” Jesse Green adds:

Cushing was the first anthropologist to have actually lived with his subjects over an extended period—and the only man in history entitled to sign himself, as he once did at the end of an official letter, “1st War Chief of Zuñi, U.S. Asst. Ethnologist.” [1979, p. 5–6]

This list of legendary feats may look somewhat different if subject to the same scrutiny as Malinowski’s.⁵⁷ Yet clearly Cushing was involved in a project that in many respects—duration of field stay, wardrobe (see any of the handful of classic photos of Malinowski in the field), formal recognition of community roles, and even acculturation, since it has been suggested that Cushing “felt more at home among the Zuñi than among his own people” (Pandey 1972, p. 322; cf. Malinowski 1967/89)—outpaced ethnography’s recognized founder.⁵⁸

What is perhaps most special about ethnography as a research tradition is its propensity to perpetually and critically assess, and at times reinvent, its methodological, theoretical, and epistemological foundations. More than anything else, what marks the ethnographer as distinct from researchers who engage in (seemingly) identical methods and activities of qualitative field research (or participant-observations) are the sensibilities that led them to research, inform them during its unanticipatable courses of experiences, and, ultimately, sustain meaningful legacies thereafter.

Conclusion

Today’s ethnographers inherit the burdens of Malinowskian methodological precepts but are privileged in their ability to construct their own projects in strategic juxtaposition to those that came before them. In Malinowski’s example, both legendary and personal, the metaphors of travel and narratives of revision enact and sustain discourses that are crucial to understanding ethnography’s journey through a century of practice over epistemological, theoretical, and methodological grounds.

Among the several functions that Malinowski attributed to myths and legends, his claim that they open up historical vistas (1926/1948, p. 107) is perhaps the most apt point to close on. Mythic narratives “reflect the circumstances and perspectives of their narrators” and provide context for contemporary commentary (Fardon, 1990, p. 570). Malinowski then, through his status as ethnography’s “most mythicized” figure (Geertz, 1988, p. 75), serves as a beacon for whatever future turns ethnography’s journey into its second century as a

professionalized practice takes. His legend supplies knowledge of where modern ethnography emerged from, highlighting both its enduring value and what has thankfully been left to the past, and simultaneously inspires the need for constant criticality, revision, and above all else, contextual awareness of how far this ethnographic field has yet to go.

Future Directions

- What can historical methodological documents teach us about the development and evolution of ethnography (and about the attitudes, political views, and underlying epistemological assumptions of researchers during a particular period)?

- What are the limitations of field notes and other forms of on-the-spot ethnographic record keeping? As with tape recorders or video cameras, in some instances, might field note documentation be viewed as negatively impacting ethnographic relationships? Can ethnography exist without field notes? What recent technological innovations or modes of ethnographic inquiry and analysis could potentially substitute for them?

- Should ethnographers, on the whole or within specific disciplines, have a collective position on institutional review board compliance? Is it fundamental to what ethnographers do, or is it an unnecessary encumbrance that the increasing numbers of ethnographers outside the academy (and “everyday” ethnographers) do not have to deal with?

- In a context of ubiquitous media interconnectedness, viral news streams, and big data, how must ethnography adjust to issues of timely publishing, accountability, and the erosion of ethnographic authority in a highly mediated, data-based “interview society?”

- As the lines between ethnography and everyday life become increasingly fuzzy, what new modes of ethnographic understanding and representation should be acknowledged and embraced?

- In ethnography’s post-postmodern reformulations and trajectories, how should ethnographers map the boundaries of the field (epistemologically and in terms of the various interests which ethnographic study can serve)?

- Ethnography’s foundations are in writing culture, yet historically ethnographers are deeply implicated in the project of literatizing non-literate societies. Given this paradox, what non-literal forms of ethnographic representations might a

contemporary, critical, and historically informed ethnographic project take? How can we move beyond *writing culture*?

Notes

1. For instance, Norman K. Denzin and Yvonna S. Lincoln identify “at least eight historical moments” in qualitative research history; writing in 2008 (p. 3), they list these as: the traditional (1900–1950), the modernist (1950–1970), blurred genres (1970–1986), the crisis of representation (1986–1990), the postmodern (1990–1995), the postexperimental (1995–2000), the methodologically contested present (2000–2004), and the fractured future (2005–). While I see value in their effort to assign broad themes to various time periods, I am less comfortable with the accelerated momentum of their model. To define the four year period of 2000–2004 as an “historical moment” on par with the first fifty years of the twentieth century strikes me as peculiar—something like a historiographic version of the old social evolutionist claims that non-literate peoples had been living the same way for the last thousand years. More to the point, to place six “historical moments” between qualitative research as practiced in 1948 and that practiced in 2008, from where I sit, misleadingly magnifies the impression of how far it has come.
2. Arthur J. Vidich and Stanford M. Lyman view this conflation as unwise and unserviceable, arguing that the ethnographic “data gathering process can never be described in its totality because . . . [it is] part of an ongoing social process that in its minute-by-minute and day-to-day experience defies recapitulation” (2000, p. 38).
3. Several very good overviews of ethnographic qualitative field research methods exist, including Hammersley & Atkinson (1995), Bernard (1995), Bailey (2007), and Emerson et al. (1995).
4. The classic definition-of-culture example comes from Alfred L. Kroeber and Clyde Kluckhohn (1952), who compiled 162 different definitions of the term.
5. *Ethno* is derived from the Greek *ethnos*, which refers to “people, nation, class, caste, tribe; a number of people accustomed to live together;” and *graphy* is derived from the Greek *graphia*, meaning “description of.” These etymological definitions came from the Online Etymology Dictionary: <http://www.etymonline.com/> (Retrieved July 16, 2012). Similar breakdowns can be found in Jones (2010).
6. Of course, Malinowski had already received a doctorate from Jagiellonian University in Cracow before he went to England (Ellen, Gellner, Kubica, & Mucha, 1988), but because that degree is typically listed as in physics and mathematics, it is regarded as incidental to his later work.
7. See for example Stocking (1983a), M. W. Young (1988), Geertz (1988), and Kuper (1996).
8. By one popular account, Malinowski’s status as an “enemy alien” (Wayne, 1985, p. 533) prevented him from returning to Europe (see Kuper, 1996; J. S. Jones, 2010). By another—first relayed to me as an undergraduate—Malinowski’s journey to the southwest Pacific was engineered in part to dodge the outbreak of war in Europe. To the extent that this alleges an avoidance of military service, it seems untrue since owing to his health troubles, most notably issues with his eyesight, Malinowski was deemed unfit to serve (M. W. Young, 2004, p. 38).

9. Regarding class, Malinowski's daughter Helena Wayne (1985) writes that both of her paternal grandparents belonged to a class that to her knowledge had "no exact equivalent" in Europe—"between landed gentry and nobility, but certainly not aristocracy" (p. 529). The story of young Malinowski being read *The Golden Bough*—which is contradicted by at least one testimony from Malinowski himself regarding his first "read[ing] [emphasis added] this great work" (Leach, 1965/2000a, p. 26)—can be traced to a 1923 letter written to Frazier (cited in Stocking, 1983b, p. 93). It is clear that Józefa Malinowska read a good deal to her son during his secondary-school years and beyond when trouble with his eyesight forced him out of school and to spend significant time with "his eyes bandaged" (Wayne, p. 530). By one account, she even forbade him to read, opting instead to "read everything to him herself" (M. W. Young, 2004, p. 38).
10. Stocking (1992) also cites these "preadolescent experiences at the cultural margins of Europe" as inspiring young Malinowski's "romantic fascination with the culturally exotic;" adding that his father's interest in folklore (see below) and Malinowski's perspective of having grown up in a "subjugated nation" may have also contributed to his turn towards anthropology (p. 241).
11. Young (2004) has suggested that Malinowski's opportunities to work with Wundt might have been truncated by the latter's age and career stage, not to mention his responsibilities as university rector.
12. Robert Redfield writes in his introduction to Malinowski's *Magic, Science, and Religion*, "Malinowski's gift was double: it consisted both in the genius given usually to artists and in the scientist's power to see and to declare the universal in the particular" (1948, p. 9).
13. By some accounts of the Malinowski myth, it was his sickness that caused him to break from his path to science (Kuper, 1996, p. 9). To the extent that this may be partially true—and both his extracurricular readings and Mediterranean travels *could* be construed as a product of illness—it might be extended to also include love-sickness.
14. Of course, this is a highly simplified explanation. For a thorough discussion of the various paradigms and epistemologies surrounding qualitative inquiry, see Lincoln & Guba (2000) and Schwandt (2000).
15. I use "ethnology" to reference the more theoretically informed, historically speculative, and comparative form of researching (mostly) non-literate societies that dominated the emerging field of anthropology during the late nineteenth century and first decades of the twentieth (see Radcliffe-Brown, 1952). Ethnology was "less intensive" than ethnography and often involved "armchair" theorists who adhered to evolutionist models of understanding human diversity. Initially Malinowski called his work ethnology (Firth, 1988). However, by the 1922 publication of *Argonauts of the Western Pacific*, he was clearly referring to it as "ethnography."
16. Prior to leaving Leipzig, Malinowski had already begun writing several ethnological projects including what would become his first book, *The Family among the Australian Aborigines* (see Barnes, 1963). In addition to his enthusiasm and notable intellect, these works enabled Malinowski to make an immediate impression on his eventual mentors.
17. Edward Burnett Tylor's (1871) classic definition of culture as "that complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and habits acquired by man [*sic*] as a member of society" (p. 1) is still widely used and taught today.
18. There are countless stories of arbitrary, serendipitous, and unforeseen circumstances that led ethnographers to particular fieldwork topics and destinations. Two of the more celebrated within American anthropology are: (1) Margaret Mead's path to studying adolescence in American Samoa, which resulted from a negotiation between her desire to study culture change in the Tuamotu Islands and her advisor Franz Boas's desire to have her study adolescence among American Indians (see Mead, 1972); and (2) the story of Lewis Henry Morgan, who came to work with Iroquois leaders after a chance meeting with a young Seneca, Ely Parker, in an Albany New York bookstore (see Lassiter, 2005).
19. There is also evidence suggesting that Haddon may have secured a travel grant for Malinowski (M. W. Young, 2004, p. 245).
20. Much of Papua New Guinea, including the Trobriand Islands, was under Australian control. The rest of it was controlled by Germany. Stocking (1992, p. 242) hints at the possibility that, with the outbreak of war, Malinowski also had to negotiate this evolving imperial scramble.
21. Malinowski had one of his most fruitful periods of early research during a time when the missionary couple he stayed with, the Savilles, left Mailu for an extended period of time. In 1915 he wrote that he found this experience working among the natives "incomparably more intensive than work done from white men's settlements" (quoted in Stocking, 1992, p. 246); and again in 1922 he wrote, "it was not until I was alone in the district that I began to make some headway" (Malinowski, 1922/1966, p. 6).
22. Malinowski's continued use of "savage" throughout his career has been, at times, presented as evidence of deep-seated racism. During this time, however, the term was a common descriptor for non-Western peoples. Its association to cultural evolutionism could certainly be used to help make the cases that Malinowski was a career long evolutionist (see Kuper, 1996, p. 8).
23. Shortly after arriving in Mailu, Saville sent a letter to his brother in England in which he listed his ten "laws in dealing with Mailu-speaking natives" (or what Stocking [1992] refers to as his "ten commandments" [p. 246]); they went as follows: "(1) Never play the fool with a native; (2) Never speak to a native for the sake of speaking to him [*sic*]; (3) Swear at a native when he is alone; (4) Never call a native, send someone for him or go inadvertently to him; (5) Never *touch* a native, unless to shake hands or thrash him; (6) Always let a native see you mean what you say; (7) Never let a native see you believe his word right away, he never speaks the truth; (8) Rarely argue with a native and then only when he is alone; (9) Warn once, afterwards proceed to action; (10) Don't try to be funny, a native can never see a joke. He possesses one joke and that is beastly talk" (M. W. Young, 1988, p. 44).
24. The note, found among Haddon's papers, was typed and, intriguingly, neither signed nor dated—Young (2004, p. 357) is nonetheless "almost certain" that it was written by Saville.
25. Indeed, in the opening pages of *Argonauts of the Western Pacific*—the major publication introducing his New Guinea/Trobriand fieldwork and announcing his revolutionary

- method—Malinowski describes the beginnings of his field research on Mailu as “making [his] first entry into the village... in the company of his white cicerone” (presumably Saville) and later returning, where after a few exchanges of “compliments in pidgin-English” and “some tobacco changing hands” he “tried then to *proceed to business*” (1922/1966, pp. 4–5—emphasis added). Young (2004) confirms that “some work” was done during this “first week” on the island (p. 332).
26. In fact, J. L. Myers describes Rivers’ contributions to the 1912 edition of *Notes and Queries* as “a revelation to all but an inner circle of colleagues” and “setting a standard of workmanship in the field” (1923, p. 15). Would Malinowski, who went on to be the recognized setter of the next new standard, have been among that inner circle of colleagues? Stocking, for one, definitively names Malinowski as the last member of the “Cambridge School” to get into the field (1983b, p. 82). If by 1912 Malinowski was not a member of Rivers’ inner circle, he would have beyond any doubt been only one degree removed.
 27. See for instance Deloria (1969/1988), Willis (1972), Asad (1973), Owusu (1978), Magubane & Faris (1985), R. Rosaldo (1989), Smith (1999/2012), A. A. Young (2008).
 28. This is only a smattering of what was included. For the complete list and a discussion of its significance, see M. W. Young (2004, pp. 264–267).
 29. In a fascinating discussion, James Urry (1972) outlines how *Notes and Queries on Anthropology* was specifically marketed to colonialists to help mitigate the consequences of cross-cultural disagreements and misunderstandings. He concludes that, at the dawn of the twentieth century, “political and economic motives for the collection of ethnographic materials were becoming as important as the scientific” (p. 49).
 30. Lowie supplies an exclamation point to the story by recounting how, the following year, a New York City election official stood “completely nonplussed” after being told that Lowie’s occupation was ethnology; “[h]e evidently lacked the educational advantages of the Crow reservation,” Lowie concluded (1959, p. 60).
 31. Several North American researchers, most notably Frank Hamilton Cushing (Pandey, 1972; Green, 1979) and Boas (Cole, 1983), had previously achieved this level of integration.
 32. See Karam (2007, p. 18–19); some of the details of this account were also confirmed through personal email correspondence (August 20, 2012).
 33. See D. Jones (1970), Nakhleh (1979), Hau’ofa (1982), Ohnuki-Tierney (1984), and Narayan (1993).
 34. Curiously in the list that M. W. Young (2004) presents there is no mention of a phonograph recorder. Wax cylinders did not work well in the tropics and, as Young notes, only six cylinders (of six dozen shipped) of sound recordings survived.
 35. Thanks to Lakshmi Jayaram and Ali Colleen Neff for pointing out these specific practices to me.
 36. Also compare same-culture studies conducted by Redfield (1930) against Lewis (1951), Dollard (1937) against Powdermaker (1939), as well as Mead (1935) against Fortune (1939).
 37. Nash & Wintrob (1972, p. 531) credit the “assertions of independence by native people” in a general sense—outside of native anthropology in particular—with unsettling the self-assuredness of the Western colonial view of non-Western people.
 38. I caution that all modes of recording—including video camera—have certain biases of perspective and limitations. Nevertheless, for many audiences—and particularly Western audiences conditioned to privilege vision over other sensory input (M. Jackson, 1989, p. 6)—seeing is believing.
 39. This is by no means a one-way debate. Although many ethnographers would be more than happy to not have to deal with IRBs, some feel that by not requiring IRB approval, ethnographers would be further marginalized as unscientific and/or not *real* research (Lincoln, 2005).
 40. Young (2004) elaborates on Malinowski’s preoccupation with the “salacious details” of Trobriand sex life including what was likely a rather unnerving correspondence with Annie Brunton regarding the “sensual temptations” of Kiriwinian young women (pp. 402–405).
 41. In fact, one could quite straightforwardly make the case for the “Sage of Great Barrington” (as DuBois came to be known) as the inventor of modern ethnography.
 42. Howard Becker is critical of this designation, arguing that “‘Chicago’ was never the unified chapel...[or] unified school of thought” that many believe it to have been (1999, p. 10).
 43. This can also be seen in the ethnographies conducted by white sociologists of African American communities during the integrationist period of the 1960s (A. A. Young, 2008).
 44. On the basis of their distortions and lack of scientific rigor (Mills, 1942; Pfauts & Duncan, 1950; Madge, 1962; Colson, 1976; Frank, 1977), oversights (Thernstrom, 1964; Lassiter et al., 2004), and their presentation of ideal types of community members as opposed to portraying genuine personalities (Goldschmidt, 1950; Ingersoll, 1997). If such critical reception followed the publication Hollingshead’s studies of *Elmtown Youth* (1949), it seems to have been less publicized, most likely owing to the fact that, unlike Middletown and Yankee City, Elmtown’s true identity remained hidden.
 45. More recently some urban ethnographers have focused their attention of elite institutions—i.e., “studying up”; examples of this research include Latour (1987), Cassell (1991), Karam (2007), Foshier (2009), and Ho (2009).
 46. Even if, customarily, the researcher-as-person would then disappear into “scientific omniscience” (Coleman & Collins, 2006, p. 1).
 47. See Clifford (1994), Friedland & Boden (1994), Downey & Dumit (1995), and Marcus (1996). For some very good recent examples of transnational ethnographies, see Pribilsky (2007) and Zheng (2010).
 48. Malinowski (1922/1966) specifically said that “[o]ne of the first conditions of acceptable ethnographic work certainly is that it should deal with the totality of all social, cultural, and psychological aspects of a community, for they are so interwoven that not one can be understood without taking into consideration all the others” (p. xvi). This idea of anthropology as a holistic science continues to be reiterated in the introductory chapters of most discipline textbooks.
 49. Such transparency might seem rather pedestrian by today’s standards, but, in its historical context, insisting on these types of divulgements was a noteworthy gesture.

50. Representative examples of this work include Rabinow (1977), Myerhoff (1978), Crapanzano (1980), M. Rosaldo (1980), Taussig (1980), and Friedrich (1987).
51. For examples of such work from the feminist tradition, see Rosaldo & Lamphere (1974), Reiter (1975), and Daniels (1983); from the indigenous or native ethnography tradition, see Jones (1970), Owusu (1978), and Nakhleh (1979).
52. Inspired by Narayan's (1993) insights, I distinguish between native and indigenous ethnographers on the basis of the former being an ascribed identity and the latter being a political stance.
53. In his Introduction to *Writing Culture*, Clifford (1986) lists Geertz, Victor Turner, Mary Douglas, Lévi-Strauss, Leach, Mead (1928/1961), Ruth Benedict, as well as Malinowski as forerunners of this ethnographic tradition. I would resolutely add Zora Neale Hurston (1935/1990; 1942/1991).
54. His publications were noticeable and memorable for their poetics. The titles of his monographs alone make the case, including the dignified splendor of *Argonauts of the Western Pacific*; the crude promotional-ism of *The Sexual life of Savages and Sex and Repression in Savage Society*, which Stephen Hugh-Jones and James Laidlaw (2000) describe as "fairly low gimmicks" (p. 17); and the mystical intrigue of *Coral Gardens and Their Magic*.
55. Fine (1996) cites institutional placement as one of the key factors in enabling reputation building and sustainment. Beyond his position at the London School of Economics and his paramount role in establishing it as the leading center for anthropology in Europe, through his outstanding lectures and excellent mentorship (Kluckhohn, 1943) Malinowski cultivated a generation of scholars—among his academic progeny were some of the biggest names in twentieth century anthropology—who would continue to sing his praises for years to come.
56. For a good discussion of this, see Firth's (1989) "Second Introduction 1988" to the republication of Malinowski's *Diary*.
57. And very much like Malinowski, Cushing was not beyond strategically constructing his own legend (see Green, 1979, p. 25 n. 5; Koianos & Weisman, 2005).
58. Other prominent candidates for "original participant observer" include Malinowski's American anthropological counterpart, Franz Boas (see Rohner, 1969; Cole, 1983); Alice Cunningham Fletcher, who first traveled to Nebraska in 1881 in the interest of studying the life of Omaha women and ended up "traveling with the Omahas for weeks at a time, learning their customs and listening to their fears" about being taken advantage of by the American government (Mark, 1980, p. 67); Nikolai Mikouho-Maclay, the Russian fieldworker who in 1871 found himself "virtually alone among previously uncontacted and totally 'untouched' groups" on the northern coast of New Guinea (Stocking, 1992, p. 222); and Lewis Henry Morgan, whose *League of the Ho-de-no-sau-nee* (1851) has been referred to as "the first 'true ethnography'" (Lassiter, 2005, p. 30).
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The Purposes, Practices, and Principles of Autoethnographic Research

Carolyn Ellis and Tony E. Adams

Abstract

This chapter details the authors' approach to understanding and practicing autoethnography. It begins by defining autoethnography and describing its history and emergence within qualitative social research and within psychology. It then proposes general guiding principles for those seeking to do autoethnography, principles such as using personal experience, acknowledging existing research, understanding and critiquing cultural experience, using insider knowledge, breaking silence, and maneuvering through pain, confusion, anger, and uncertainty. It continues with a discussion of autoethnography as a process and as a product, and a method that can take a variety of representational forms. After offering ways to evaluate and critique autoethnography, it concludes with a discussion of autoethnography as an orientation to the living of life and an approach that has the potential of making life better—for the writer, reader, participant, and larger culture.

Key Words: Autoethnography, qualitative research, personal narrative, storytelling, ethical research, insider knowledge, identity politics, interactive interviewing, reflexivity, collaborative witnessing

In this chapter, we detail our approach to understanding and practicing autoethnography. We begin by defining autoethnography and describing its history and emergence within qualitative research and within psychology. We then propose general guiding principles for those seeking to do autoethnography, which include using personal experience, acknowledging existing research, understanding and critiquing cultural experience, using insider knowledge, breaking silence, and maneuvering through pain, confusion, anger, and uncertainty. We continue with a discussion of autoethnography as a process and as a product, one that can take a variety of representational forms. After offering ways to evaluate and critique autoethnography, we conclude with a discussion of autoethnography as an orientation to the living of life and an approach that has the potential for making life better—for the writer, reader, participant, and larger culture.

What Is Autoethnography?

Autoethnography refers to research, writing, stories, and methods that connect the autobiographical and personal to the cultural, social, and political. This approach considers personal experience as an important source of knowledge in and of itself, as well as a source of insight into cultural experience. As Ellis (2004) notes, autoethnographers “look inward, exposing a vulnerable self that is moved by and may move through, refract, and resist cultural interpretations,” and, simultaneously, focus “outward on social and cultural aspects of their personal experience” (p. 37; see Ellis, 2009*a*; Ellis, Adams, & Bochner, 2011). Autoethnographers use reflexivity to illustrate intersections between self and society, the particular and the general, the personal and the political (Berry & Clair, 2011). They also recognize and respect a researcher's relationships with others (Ellis, 2007), treat research as a socially conscious

act (Holman Jones, 2005a), and help humanize emotionally sterile research processes (Ellis, 1991).

Autoethnography implies connection: the stories we write connect self to culture; the way we research and write these stories blends social science methods with the aesthetic sensibilities of the humanities, ethnographic practices with expressive forms of art and literature, and research goals of understanding with practical goals of empathy, healing, and coping. We write concrete stories about our lives because we think that the stories of a *particular* life can provide a useful way of knowing about *general* human experience. These stories also offer insight into the patterned processes in our interactions and into the constraints of social structures. As well, *telling* and *listening* to stories and comparing our stories to those of others are how we learn, cope, and make our way in society.

Claiming the conventions of literary writing, autoethnography features concrete action, emotion, embodiment, self-consciousness, and introspection portrayed in dialogue, scenes, characterization, and plot. Autoethnography can take a variety of forms, including short stories, poetry, performance, new media, art, and multivoiced work, such as collaborative autoethnography (Chang, Ngunjiri, & Hernandez, 2012), co-constructed narrative (Bochner & Ellis, 1995), and collaborative witnessing (Ellis & Rawicki, 2013; in press). Additionally, autoethnography can be used in a variety of ways, from positioning oneself in the text as the researcher to being a participant to being a focus of research. We elaborate on many of these ideas throughout this chapter.

History of Autoethnography

Heider (1975) employed the term “autoethnography” to describe a study in which cultural members give their own accounts about their culture. Goldschmidt (1977) noted that “all ethnography” is “self-ethnography” in that it reveals personal investments and particular kinds of analysis, and he used autoethnography to investigate anthropology’s position in and relevance to the academy and in society (p. 294). Hayano (1979) used “auto-ethnography” to describe anthropologists who “conduct and write ethnographies of their ‘own people’” (p. 99) and researchers who choose a “field location” tied to one of their own identities or group memberships. Although these views of autoethnography foreground distinctions of insider–outsider, the move toward the personal is implied, with Heider making a case for the value of cultural members telling their

own stories, Goldschmidt arguing that traces of the personal are present in all ethnographic work, and Hayano describing the importance of a researcher’s identities and connection with similarly identified others.

Although the term “autoethnography” was not employed often during the 1980s, sociologists, anthropologists, communication scholars, and others doing oral interpretation, performance ethnography, and feminist research began writing and advocating for forms of personal narrative, subjectivity, and reflexivity in research (see Benson, 1981; Conquergood, 1986; Crapanzano, 1980; Denzin, 1989; Clifford & Marcus, 1986; Oakley, 1981; Pacanowsky, 1988; Reinhartz, 1984; Shostak, 1981; Van Maanen, 1988; Zola, 1982). These scholars were interested in the importance of storytelling and enactments of culture, and they progressively became engaged by the personal traces in ethnographic practice. Rejecting the idea that ethnographers should hide behind or perpetuate an aura of objectivity and innocence, these researchers began including themselves as part of what they studied, often writing stories about the research process and sometimes focusing on their own experience. At the end of the decade, literary and cultural critics began to apply the term “autoethnography” to work that explored the interplay of the introspective, personally engaged self with cultural descriptions mediated through language, history, and ethnographic explanation (see Deck, 1990; Lionnet, 1989).

The 1990s saw the emergence of more emphasis on personal narratives and the continuation of the autoethnographic movement that crossed many social scientific disciplines. I (CE) published one book (*Final Negotiations*; Ellis, 1995a) and more than two dozen essays about autoethnography, and I co-edited two books about the use of personal experience in research—*Investigating Subjectivity* (with Michael Flaherty; Ellis, 1992) and *Composing Ethnography* (with Art Bochner; Ellis, 1996a). Bochner (1994; 1997) published essays about the importance of personal stories and their relationship to theory, and, together, the two of us began editing the *Ethnographic Alternatives* book series, all of which illustrated how and why personal experience should be used in research. Other key works from this decade included Reed-Danahay’s *Auto/Ethnography* (1997) and the first *Handbook of Qualitative Research* (Denzin & Lincoln, 1994), which contained chapters on personal experience and research (Clandinin & Connelly, 1994) and writing as a method of inquiry (Richardson, 1994).

Also important were Goodall's *Casing a Promised Land* (1989), Behar's *The Vulnerable Observer* (1996), Richardson's (1997) *Fields of Play*, and Pelias's *Writing Performance* (1999). All of us (along with many others!) helped carve out a special place for emotional and personal scholarship, and the term "autoethnography" soon became the descriptor of choice.

The first decade of the twenty-first century saw the publication of the second and third editions of the *Handbook of Qualitative Research* (Denzin & Lincoln, 2000; 2005a), both of which included numerous references to personal ethnography, personal experience, personal narrative, personal writing, autobiography, and reflexivity, as well as specific chapters about autoethnography (Ellis & Bochner, 2000; Holman Jones, 2005a). I (CE) published two additional books about autoethnography (Ellis, 2004; 2009a) and two more co-edited collections about autoethnography (Bochner & Ellis, 2002; Bartlett & Ellis, 2009). Art and I also started *Writing Lives*, a second book series about autoethnography published by Left Coast Press. In this decade, there were many more notable books (e.g., Alexander, 2006; Chang, 2008; Goodall, 2001; 2006; Holman Jones, 2007; Pelias, 2004; Poulos, 2009; Tillmann-Healy, 2001), essays (e.g., Adams, 2006; 2008; Adams & Holman Jones, 2008; Anderson, 2006; Berry, 2006; 2007; 2008; Boylorn, 2006; 2008; Crawley, 2002; Holman Jones, 2005b; Jago, 2002; Pelias, 2000; Pineau, 2000; Spry, 2001; Tillmann, 2009; Wall, 2006; 2008), and special issues of journals about autoethnography, reflexivity, and personal narrative (e.g., Boyle & Parry, 2007; Ellis & Bochner, 1996b; Gingrich-Philbrook, 2000; Hunt & Junco, 2006; Warren & Berry, 2009). Furthermore, in 2005, Norman Denzin began the International Center for Qualitative Inquiry and the International Congress of Qualitative Inquiry, an organization and a conference that recognized the importance of reflexivity and personal experience in research.

Currently, in the second decade of this century, excitement about autoethnography continues to flourish. Denzin and Lincoln (2011) published the fourth edition of the *Handbook of Qualitative Research*—a collection that, similar to the two previous editions, includes numerous references to ethnography, personal experience, and reflexivity, as well as two chapters about autoethnography (Pelias, 2011a; Spry, 2011). There continue to be more books (e.g., Adams, 2011; Boylorn, 2013; Boylorn & Orbe, 2014; Chang & Boyd, 2011; Denzin,

2014; Gergen & Gergen, 2012; Diversi & Moriera, 2011; Pelias, 2011b; Spry, 2011; Tamas, 2011), essays (e.g., Boylorn, 2011; Foster, 2010; Fox, 2010; Jago, 2011; Holman Jones, 2011; Holman Jones & Adams, 2010), and special issues of journals devoted to autoethnography (e.g., Adams & Wyatt, 2012; Berry & Clair, 2011; Myers, 2012a; Ngunjiri, Hernandez, & Chang, 2010). The International Congress of Qualitative Inquiry is entering its tenth year. Finally, and certainly notable, are the four volume set, *Autoethnography*, edited by Pat Sikes (2013), and the *Handbook of Autoethnography* (2013), which we (TEA and CE) have edited with leading author Stacy Holman Jones. Along with Sikes's collection of reprinted articles and chapters, our *Handbook* of new chapters further legitimates the approach, offers practical advice for using personal experience in research, and poses future possibilities for doing autoethnographic work.

Autoethnography and Psychology

Although coming later to the qualitative revolution than other disciplines, psychologists have increasingly begun to embrace qualitative methods (Marecek, Fine, & Kidder, 1997; Wertz, 2011). Along with this embrace has come an increased interest in autoethnography. Given the emphasis in psychology on the mind and the self, autoethnography would seem like a good methodological fit. So why the delay? Perhaps this should not be surprising, given psychology's desired separation from the humanities and its preferred identity as a science (Wertz, 2011). Additionally, psychology has had complex and contradictory responses to introspection and self-observation in terms of evaluating its scientific rigor, reliability, and measurability (see Ellis, 1991; McIlveen, 2008; Polkinghorne, 2005; Schultz & Shultz, 2012). Nevertheless, some interpretive psychologists—whether they refer to themselves as autoethnographers or not—have embraced autoethnographic practices for some time. Some have composed autoethnographies and others have examined the use of personal narratives in research. Psychologist Amia Lieblich, for example, authored *Conversations with Dvora* (1997), a book about the imagined conversations between herself and an early modern woman writer, and *Learning about Lea* (2003), which is both a biography of Lea Goldberg, a poet, and about Lieblich's personal journey and discovery of Goldberg. Ruthellen Josselson (1996; 2011) has composed personal stories about herself as a researcher, her feelings

about the research process, and the issues that arise in doing research with others. Psychologist George Rosenwald (1992) has examined autobiographical stories and self-understanding (see also Rosenwald & Ochberg, 1992), and psychologist Dan McAdams and other colleagues from psychology have explored personal narratives and life stories (see McAdams, Josselson, & Lieblich, 2006).

There are also many others in psychology who have begun to write autoethnographically and to refer to what they do as autoethnography. For example, vocational psychologist McIlveen (2007) used autoethnography to examine his career counseling process. Health psychologist Smith (2004) autoethnographically examined eating disorders. Community psychologist Langhout (2006) employed autoethnography to look at issues of race, class, and gender in a research project. Du Preeza (2008) examined autoethnography as an example of reflexive practice that brought her to and through her research. Psychologist Tessa Muncey wrote *Creating Autoethnographies* (2010), a practical guide that details the steps for doing an autoethnographic project. Jane Speedy (2013) and her colleagues and students (e.g., Martin et al., 2011) and other psychotherapists and counselors now embrace autoethnography as well.

As well, other supportive psychologists help open up spaces for autoethnographic work. For example, Ken and Mary Gergen celebrate autoethnographic writing and performance in their book, *Playing with Purpose: Adventures in Performative Social Science* (2012) and also in other publications (Gergen & Gergen, 2000; Gergen & Gergen, 2002), and they often do autoethnographic performances. Although they themselves do not do autoethnography, psychologists Günter Mey and Katja Mruck from Germany published a chapter on autoethnography (Ellis, Adams, & Bochner, 2010) in their *Handbook of Qualitative Research in Psychology* and reprinted the chapter in their online journal, *Forum: Qualitative Social Research* (Ellis et al., 2011). For the past two years, there has been a “Day in Qualitative Psychology” at the International Congress for Qualitative Research that includes several autoethnographic presentations. In 2012, a session there, entitled “Critical ‘I’ and Qualitative Psychology,” included three autoethnographic papers (Benjamin, 2012; Benozzo & Bell, 2012; Trostin, 2012).

We predict that this interest in autoethnography, along with the entirety of qualitative methods, will increasingly move into mainstream psychology. This

is evidenced by the new American Psychological Association’s “Society for Qualitative Inquiry in Psychology” (started by Kenneth Gergen, Ruthellen Josselson, and Mark Freeman), as well as by the new handbooks, journals, textbooks, and conferences in psychology that feature qualitative work (see Wertz, 2011). The spread of qualitative work, including autoethnography, will happen for many of the same practical and social reasons that it has occurred in other disciplines.

Why Autoethnography? Why Now?

Although we provide an overview of the history of autoethnography in terms of those who have been involved, the history does not illustrate specifically why autoethnography came to exist. We are aware that this movement has developed alongside the cultural emphasis on self and self-revelation expressed in such popular cultural phenomena as reality TV, the self-help movement, new media where folks blog and tell their stories, and the popularization of memoirs and autobiographies. Although we see a strong connection from autoethnographies to memoirs and autobiographies (thus, the “auto” in autoethnography) in terms of the emphasis on telling one’s story, we view our work as more analytic and scholarly than what is shown on reality TV and in most blogs. Furthermore, our goals are broader than those of the self-help movement, which is more focused on and committed to individual change as the sole outcome of their work. As autoethnographers, we emphasize interpretation and reflection, and we attempt to compare, normalize, and understand how folks experience emotions, bodies, and thought. We investigate the ways that authors complicate recovery processes (Tamas, 2011) or being survivors (Spry, 2011). We have a heightened concern about ethics, and we also examine the influence of culture, politics, and power relations on personal experience. Many of these aspects often are neglected by more popular forms of self-expression, especially reality TV, which tends to sensationalize the personal, perpetuate the victim status, forego ethical considerations, focus on individual concerns at the expense of cultural concerns, and reinforce the oppressive structures of capitalism that contribute to victimization (Rothe, 2011).

Although this cultural movement of telling one’s story in memoirs and healing one’s self in the self-help movement may have served to bring attention to what we do, we do not think this is a primary reason for why autoethnography has emerged in many academic contexts. In this section, we describe why

we think this happened, and, in particular, focus on three interrelated conditions that contributed to the emergence and solidification of autoethnography as an approach to research: (1) the growing appreciation for qualitative research and personal storytelling in academia, (2) a greater recognition of research ethics, and (3) the influx of women and minorities into academia and the continuing emergence and importance of identity politics.

The Growing Appreciation for Qualitative Research

In the 1970s and 1980s, concerns mounted that quantitative, social scientific research could not solve all social problems, was inadequate for capturing the particulars of social experience, and, in many ways, adhered to invasive and unethical procedures for studying and representing others. A “crisis of representation” occurred—a moment when scholars questioned strongly the objectives of traditional research. Such objectives included the goal of seeking universal Truth, especially with regard to social relations; the disregard of stories and storytelling in human life; a bias against affect and emotion; and a neglect of the ways in which social positions (e.g., race, sex, age, sexuality) influence how persons research, write, read, and evaluate. This lack of emphasis on feelings, chaos, and nonrationality, as well as on personal involvement in research and the use of subjectivity and first-person voice, paved the way for the emergence of a greater appreciation for qualitative research—research grounded in quality (not quantity!) and research that tends to embrace, or at least be more cognizant of, ethical and humane ways to study others.

Autoethnography emerged within qualitative research for many of these same reasons, although it has responded to these troubles to a greater extreme than traditional qualitative work. As with much of the interpretive side of qualitative research, autoethnography is a partial response to the crisis of representation; it emerged to dismiss any possibility of universal Truth; recognize the importance of storytelling (Bochner, 2002) and the existence of messy, emotional, and leaky bodies (Lindemann, 2010); and to counter use of colonialist and invasive ethnographic practices, such as going into and studying a culture, leaving to write about—represent—this culture, and disregarding what the representation might do to cultural members (see DeLeon, 2010; Ellis et al., 2011; Wall, 2006).

Autoethnography also emerged to address aspects of *social* life that were neglected by *social*

scientists. For instance, much of my (CE) work with autoethnography grew out of my awareness of the deficiencies of traditional social science research for dealing with day-to-day realities of chronic illness and relational process (see Ellis, 1998a). To get to the essence of what I wanted to examine meant violating the taken-for-granted conventions of social science research and writing, breaching the separation of subject and researcher, and disrupting the traditional idea of generalizability across cases. To understand life as lived, especially intimate life involving relationships and death, I had to disclose details of my private life that are usually hidden and to highlight emotional experience, all of which challenged the “rational actor” model of social performance (p. 52). If our task as researchers, as *social* scientists, is to study people, the creators of “social life,” then we should try to include as much of the person as possible and not relegate parts of our lives and our selves to the periphery.

An Emphasis on Ethics

The second condition that allowed for the emergence of autoethnography was the growing instances of ethical violations in the social sciences during the past fifty years. Of historical importance are the abuses brought on by the Milgram experiments of the 1960s and the Tuskegee syphilis study that took place from the 1930s until the 1970s, two primary cases that contributed to the emergence of institutional review boards (IRBs) within the United States. Coupled with these concerns were ethical considerations in traditional ethnographic practices about the possible exploitation of the people being studied.

Stanley Milgram (1963; 1964), a social psychologist at Harvard, conducted an experiment to investigate the unquestioned “destructive obedience” that occurred in the Holocaust (Milgram, 1964, p. 848). He wanted to figure out how millions of victims were slaughtered from 1933 to 1945 by people who were only, supposedly, obeying orders from authorities (Milgram, 1963, p. 371). Milgram designed a study to test obedience, which illustrated how everyday people would succumb to the perceived authority of a researcher and harm others upon the researcher’s command. Although Milgram’s intentions were commendable, other scholars raised significant questions about his research implementation (see Baumrind, 1964). For instance, other researchers asked: What gives a researcher the right to make people “sweat, tremble, stutter, bite their lips, groan, and dig their fingernails into their flesh”

for the purposes of knowledge (p. 375)? Does the researcher have any concern for and responsibility toward participants for whom their research motivates seizures and cultivates “serious embarrassment” (p. 375)? Do the benefits of social scientific inquiry, of achieving understanding by examining situations “in which the end is unknown,” justify the use of risk among participants (Milgram, 1964, p. 849)?

The Tuskegee syphilis study, which investigated different kinds of treatments for curing syphilis, also illuminated numerous ethical issues of traditional research processes. White researchers solicited only poor, African-American men infected with syphilis and treated them as “‘subjects, not patients; clinical material, not sick people’” (Heller, cited in Thomas & Quinn, 1991, p. 1501). Researchers showed a “minimal sense of personal responsibility and ethical concern” throughout the project, especially since effective treatments for syphilis were found but never revealed to the participants (p. 1501). Such deception among researchers has made some people suspicious about any kind of social science research. For instance, as Thomas and Quinn observe, “strategies used to recruit and retain participants in the [Tuskegee] study were quite similar to those being advocated for HIV education and AIDS risk reduction programs today” (p. 1500). Thus, some African-American groups are leery of this education and these programs, fearing they might be a continuation of racial eradication supported (or at least not objected to) in the past by white people doing research on African-American populations.

Concerns were not limited to biomedical and social psychological social science research practices. Within ethnography, questions arose about researchers who entered a culture, observed and interviewed cultural members, and then left to write their articles and books. Often, researchers did not maintain contact with members and unselfconsciously used the information they obtained solely for their own personal gain (e.g., fulfilling academic responsibilities, monetary rewards, and academic reputation; see Rupp & Taylor, 2011). The emergence of feminist understandings of research (Keller, 1995) and postcolonialism (Smith, 1999) made such ethnographic practices appear suspicious and questionable: What gives a researcher a right to do this? Is the ethnographer taking advantage of vulnerable, different, and “exotic” populations? What responsibility does the ethnographer have to the people studied? What gives a researcher the authority to represent and speak on behalf of others (Alcoff, 1991)?

A Concern with Identity Politics

A third condition that promoted the emergence of autoethnography was the influx of women and minorities into academia and the heightened attention to identity politics. Within the United States, most noticeably since the mid-twentieth century, significant backlash occurred to considering whiteness, maleness, classism, heterosexuality, Christianity, and able-bodiedness as the norms to which all people were compared or as norms that implicitly informed how research was represented and valued. For example, Lorde (1984) argues that the valuing of prose and the devaluing of poetry by traditional social scientific researchers comes from racist, classist, and sexist evaluations. Not everyone has the time, ability, financial resources, or desire to use prose to express their knowledge, nor is prose the only way knowledge can, or should, be expressed. Furthermore, orthodox researchers privileged objectivity, uncompromising rigor, and debate—characteristics typically considered to be masculine—over subjectivity, emotion, and care—characteristics typically viewed as feminine (Keller, 1995; Pelias, 2011*b*). Some researchers also continued to presume language’s neutrality, advocate the use of the “generic he,” and neglect the racist characteristics of everyday speech, such as the use of “black” to describe anything that is evil, unacceptable, and undesirable and “white” to describe anything that is safe, honest, and pure (Moore, 1976). Some writers, such as Berger (1972), critiqued how women were represented in art and research, especially how these representations cultivated harmful stereotypes about and perpetuated ignorance toward women. Other writers raised questions about research that maintained heterosexual assumptions and ideas about commitment, partnership, and family life (Foster, 2008; Frye, 1983; van Gelder, 1998).

Such concerns motivated significant changes in research design and evaluation. It became more difficult for a researcher to remain unaware of or silent about human difference, or to promote racist, sexist, classist, heterosexist, and able-bodied beliefs. If researchers failed to take into account human difference, they risked being considered uninformed and being called to account for such absence (see Keller, 1995; McIntosh, 1995). These concerns brought into question decades and centuries of extant research; much of the research now needed to be revised to include the recognition of others. Furthermore, these concerns did not (and should not) preclude white, male, upper-class, heterosexual, and/or able-bodied persons from doing

research, but only encourage them to reflexively account for the possible ways in which their identities impact what and how they see and write, as well as who and how they study. It has become much more important for researchers to reflexively situate themselves in the text and say explicitly and to the best of their ability how they arrived at the outcome of a study and show their awareness of the ethical issues that might arise when representing others.

* * *

The three conditions that contributed to the emergence of autoethnography are interrelated and inseparable. Concerns about research being an invasive and oppressive colonialist enterprise are directly connected with the ethics of researching and representing others. Storytelling is an important way of knowing for many communities, and the ethical dilemmas brought on by social science practices raised questions about the legitimacy of social scientific inquiry. Performance studies scholars—scholars who long have valued storytelling, narrative, and the body—often recognize the ways in which identity is manifest in bodies. The ethics of the Tuskegee syphilis project also included dilemmas in identity politics, particularly when African-American populations became leery of researchers, especially white researchers. Furthermore, as Thomas and Quinn (1991) argue, the end of the Tuskegee syphilis study was heavily influenced by the rise of the civil rights and Black Power movements, movements tied to certain populations/identities.

Autoethnography thus emerged partly to help accommodate the space of navigating difference and to acknowledge how and why identities matter. Consequently, autoethnography is often considered a feminist (Allen & Piercy, 2005), queer (Holman Jones & Adams, 2010), and indigenous (Tomaselli, 2003; Tomaselli, Dyll, & Francis, 2008) approach to research, one that recognizes past treatments (abuses) of research participants and instead tries to advocate for more humane treatments of selves and others in research. The self—the “auto” part of autoethnography—is central because the investigator is explicitly or implicitly part of the studied group and will not leave or cut ties to the group being represented. As such, it is assumed that researchers will take more or better responsibility in representing others. Being a member of or closely connected to the studied culture helps alleviate ethical concerns about access and colonization. Rather than speaking on behalf of others, autoethnographers may focus on their own personal experience to illuminate

nuances of cultural experience, although this would occur without a claim that the researcher’s experience represents the experience of all members of the cultural group. Rather than a retreat from fieldwork or studying others, autoethnography tries in its practice to honor and respect those being studied and to work alongside and with them rather than to invade and do research on them (see Rawicki & Ellis, 2011). At the same time, autoethnography has its own particular ethical considerations brought on by its practices, design, and subject matter, which we will discuss later in the chapter.

Given all of these concerns, the question arises: What must we take into account to do autoethnography well?

Guiding Principles for Autoethnography

We offer the following principles as a possible roadmap when one considers doing an autoethnographic project or to use when reading and contemplating the personal narratives of others. The first two principles—the use of personal experience and a familiarity with existing research—are two features that cut across almost all autoethnographic work. The remaining five elements—using personal experience to describe and critique cultural experience; taking advantage of and valuing insider knowledge; breaking silence, (re)claiming voice; healing and maneuvering through pain, confusion, anger, and uncertainty; and writing accessible prose—are more specific goals, advantages, and rewards for using autoethnography in research.

An Emphasis on Personal Experience

All autoethnographies include personal experience, although it may occupy different roles depending on the form and scope of a project. In embracing personal experience, social scientists take on the dual identities of academic and personal selves in order to tell stories about some aspect of their experience. For instance, in reflexive autoethnographies, researchers may write about their own experiences along a continuum, starting from their own life story or how they got interested in the phenomenon being examined, to studying their experience as part of a culture, to being researchers who examine a particular culture. In indigenous autoethnographies, researchers who are natives of cultures that have been marginalized or exoticized by others write about and interpret their native cultures for themselves and others. In complete-member research, researchers explore groups of which they already are members or in which they become full members during the research

process. In all cases, including when the focus is on the other, autoethnographers observe the participation between themselves and the people they study (Reed-Danahay, 2001; Tedlock, 1991).

Familiarity with Existing Research

Autoethnographers show familiarity with existing research on a topic, although this work may be included and/or referred to in different ways. For instance, if I [TEA] write about my experiences with coming out as gay, I should know what other people have said about this experience. I would then frame my discussion within what others have said or write my story in a way that adds insight into existing research. Or consider my [CE] essay “Maternal Connections” (1996). Although I do not cite existing daughter–mother research, I write to offer a description of a caregiving encounter and a counter-story to dominant stories about caregiving as a burden. I tell a story about caregiving as love, one that is not given enough credence in existing research. Offering readers a new way to think about their experiences with caregiving, this story invites readers to enter the experience and feel it with body and emotion, as well as with head and intellect. Writing in this way required being familiar with the caregiving literature, although I did not cite it or explicitly reference it.

Using Personal Experience to Describe and Critique Cultural Experience

Stein (2010) says the “best ethnographic work” tells the “story of lives lived in specific social and historical contexts and draws readers in, helping them to understand their own hopes and fears and personal and political investments” (p. 567; see Goodall, 2001). Critical ethnographers agree with Stein that they tell stories, and add that they also evaluate these stories. As Thomas (1993) says, critical ethnography is “conventional ethnography with a political purpose,” a method that facilitates “social consciousness and societal change,” aids “emancipatory goals,” and negates “repressive” cultural influences (p. 4; see DeLeon, 2010). Although the telling of stories itself can be a critical act in that description can generate knowledge and knowledge can be powerful, critical ethnographers explicitly work toward cultural change (see Boylorn & Orbe, 2014; Denzin, Lincoln, & Smith, 2008).

As part ethnography, autoethnography describes cultural beliefs and practices and helps audiences understand “hopes and fears and personal and political investments” (Stein, 2010, p. 567). For instance, Herrmann (2005) describes

what it can mean to be a son as well as the fears and feelings of father abandonment. Jago (2002; 2011) describes how depression can be lived and how it can impact relationships with others. I (CE) describe the process of caring for an aging parent, and, in so doing, show how the love between a mother and daughter can look and feel (see Ellis, 1996; 2001). I (TEA) describe characteristics of and everyday struggles with coming out of the closet—the process by which those with same-sex attraction reveal this attraction to others (see Adams, 2011). The power of these accounts rests on an author’s ability to use personal hopes, fears, and investments to provide complex and engaging descriptions of cultural life.

Some researchers consider autoethnography an inherently critical approach—a method that describes *and* critiques a person’s experiences on behalf of promoting social change, and a method that not only disrupts norms of representation but also treats research as a socially—and relationally conscious—act (Ellis, 2007; Holman Jones, 2005*a*; Spry, 2011). For instance, Pineau (2000) and Defenbaugh (2011) describe and critique medical practice and error, while Denzin (2011) concentrates on interpretations of Native Americans and US history; Boylorn (2011), Marvasti (2006), and Myers (2008) address instances of racism in everyday conversation, and Diversi and Moreira (2010) highlight hegemonic characteristics of academic writing and research. The power of these accounts is each author’s ability to describe and critique harmful aspects of cultural life.

Given autoethnographers’ critical edge, there is a tendency to tell stories about tragic events and painful experiences in order to promote awareness and change (see Myers, 2012*a*). Pleasant and comfortable experiences, such as a birthday party, graduation, or church service, may not require awareness or change; for some, these experiences may already be pleasant and comfortable. However, for others, a birthday party might be viewed as promoting capitalism; a graduation as involving grandiose, class-laden discourse; or a church service as representing or advocating sexist, racist, and/or homophobic practices. In those cases, autoethnography might provide an avenue for promoting awareness of and suggested change in these practices.

Elsewhere, I (CE) have argued that social change happens one person at a time (Ellis, 2002). In autoethnographic writing, collective action is connected to personal biography and emotionality, and abstract collective change can be represented by personal stories of actors.

Taking Advantage of and Valuing Insider Knowledge

Throughout the history of ethnography, ethnographers have debated the benefits and consequences of being insiders in and outsiders to the cultures being studied. The method of participant observation—of taking part in the culture but distancing oneself enough to credibly and “objectively” observe this culture—is a hallmark of ethnographic practice, as are concerns about the dangers of “going native” and establishing “over-rapport” with cultural members (Atkinson, Coffey, & Delamont, 2003). Being close but not too close to the people we study, finding ways to create (and manipulate) trust among different others and then exiting the field post research, are often taken-for-granted principles of ethnographic practice.

With autoethnography, however, the relationship between insiders and outsiders is fluid and untenable. More specifically, given that researchers use personal experiences to study cultural identities or experiences that have affected them, autoethnographers always are insiders (or closely related) to the groups and cultures they describe. This is helpful for several reasons. First, in adhering to philosophies of feminism, queer theory, and indigenous research, autoethnographers must carefully and respectfully represent the culture in which they study, especially given that they will be affected by the representation. For instance, instead of a more traditional ethnographer writing about people with depression, the autoethnographer who writes about living with depression often has a personal investment in how depression is represented (see Jago, 2002; 2011). Likewise, an autoethnographer who defines herself as a black, southern, rural woman will care about how she represents this culture (Boylorn, 2013).

Second, insiders or members of a culture will have different kinds of knowledge of the culture than will strangers or outsiders to the culture (see Droogsma, 2007; Marvasti, 2006; Wood, 1992). We do not suggest that this knowledge is better, more truthful, or more complete. We understand that outsiders sometimes can observe taken-for-granted acts and beliefs or distinguish patterns that cultural members may not see. But there also are benefits of being an insider to a cultural group: an insider can talk about the everyday feelings and negotiations of a cultural identity or experience, as well as intentions and motivations that might otherwise be unavailable or inaccessible to observers.

For instance, through providing everyday feelings, internal conversations, witnessed negotiations, and relational conversations grounded in concrete

detail, I (CE) provide insight into the experiences of having a minor bodily stigma to which outsiders, for the most part, would not be privy (Ellis, 1998*b*). In writing about bulimia, Tillmann-Healy (1996) notes, “I can show you a view no physician or therapist can, because, in the midst of an otherwise ‘normal’ life, I experience how a bulimic *lives and feels*” (p. 80). In these examples, autoethnographers are able to explore emotional trauma without worrying about invading respondents’ personal experiences, revealing what they might prefer remain private, or worrying about doing emotional harm to other vulnerable participants. Likewise, these writers do not have to fear losing control of their words and experiences to another researcher. As in all autoethnographies, however, the storyteller and related loved ones may be made vulnerable by what is revealed (Ellis, 2004).

Breaking Silence, (Re)Claiming Voice

With autoethnography, cultural members now have a way to tell and justify telling their personal stories, particularly within academic contexts; no longer must they rely on others to speak on their behalf. Although textual gatekeepers still exist for those who can and do write (e.g., editors, reviewers, artistic directors), autoethnographers, when compared to more traditional research norms, have more choices as to what to publish and what compromises they will make to a text (Chatham-Carpenter, 2010; Wall, 2006; 2008). Thus, another tenet of autoethnography is the ability for the autoethnographer to represent oneself complexly and justly, simultaneously recognizing that there are things that others know about us that we may never know (Mead, 1934), or that we may not know yet (Ellis, 2009*a*).

Autoethnography also allows a researcher to break silence and reclaim voice by adding nuanced personal perspectives to and filling experiential “gaps” (Goodall, 2001) in traditional research—research that often disregards emotions (Ellis, 1991), perpetuates canonical narratives (Bochner, 2002), and promotes hegemonic beliefs and practices (Pathak, 2010). For instance, Ronai (1996) describes her experience of living with a “mentally retarded” mother—experience often left out of texts on cognitive impairment, family communication, and parenting. Defenbaugh (2011) writes about her experience with inflammatory bowel disease in order to “give voice to those who have been silenced by dominant discourses” about chronic illnesses (p. 13). Berry (2007), Crawley (2002), and Eguchi

(2011) voice concerns about cultural norms of sex, gender, and sexuality. In critiquing traditional academic practices, Diversi and Moreira (2010) not only describe their experiences navigating the (white, masculine, colonialist) academy, but also stress the importance of using alternative writing strategies to represent cultural phenomena. Thus, autoethnography offers the possibility for researchers to describe their experiences of hegemonic beliefs and practices, experiences often disregarded in extant research.

Healing and Maneuvering Through Pain, Confusion, Anger, Uncertainty

Writing (and performing) is a way of knowing cultural experiences—a way to learn about social phenomena differently (see Colyar, 2008; Richardson, 1994; Spry, 2011). Through writing, we can make sense of a repetitive or problematic cultural experience and have the possibility of venting our frustrations or at least making these frustrations known to others. Through writing, we have the potential to heal (DeLeon, 2010; hooks, 1991), seek freedom (Chatham-Carpenter, 2010), and work through emotions such as anger, pain, and confusion, and, in so doing, better cope with these emotions (Berry, 2006). Through writing, we can try to understand or make sense of the pain of losing a brother (Ellis, 1993) or missing a father (Adams, 2006; 2012), the anger of feeling prejudice and being judged inappropriately (Boylorn, 2006; 2011), or the uncertainty of living with the memory of a father who has passed away (Bochner, 1997; 2012; Patti, 2012).

Another tenet of autoethnography thus involves the possibility of learning about oneself and healing through writing and, in the process, alleviating anger, pain, and confusion, and coming to feel better. This is not to say that writing will serve as conclusive therapy or that with writing recovery is possible or always desirable (Tamas, 2011; 2012). But for some people and for some topics, writing can permit an autoethnographer to work through negative feelings and/or uncertainty about a cultural experience or a particular cultural identity.

Writing Accessible Prose

In writing about bulimia, Lisa Tillmann-Healy (1996) notes that physicians and therapists tend to “write from a dispassionate third-person stance that preserves their position as ‘experts’” and, in so doing, “keep readers at a distance” (p. 80). Instead, Tillmann-Healy uses autoethnography to write from an “emotional first-person stance that highlights

[her] multiple interpretive positions” with bulimia and invites readers to “come close” and experience the world of bulimia for themselves (p. 80).

Tillmann-Healy is one of many scholars who express concerns about traditional, academic writing. bell hooks (1991) also notes that “the only work deemed truly theoretical”—and, consequently, truly valuable in academic contexts—is often “highly abstract, jargonistic, difficult to read, and containing obscure references that may not be at all clear or explained” (p. 4). Eric Mykhalovskiy (1996) characterizes the traditional work of academics as “insular and isolated, as if cut off from the lives and experiences of people outside the academy” and only read “by a handful of other academics” (p. 137). Ron Pelias (2000) describes his frustrations on looking through a new issue of *Communication Monographs*—a traditional, heavily quantitative, esoteric communication journal. The articles, Pelias says, are

located in a paradigmatic logic you find less than convincing. You read the abstracts and shake your head, not because you are confused by the content, but because you cannot understand how the scientific model continues to thrive in the [Communication] discipline given the number of arguments that show why the heart needs to accompany the head. (p. 223)

Pelias then describes putting the unread issue on his bookshelf, “along side other unread *Monographs*” (p. 223).

Another principle of autoethnography is to make meaning and knowledge available to more people than a select, academically trained few. I (CE) have described this previously as one of the “great rewards” of the method (Ellis, 2004, p. 35). I can count on one hand how many people ever wrote to me about my more orthodox social science work, but I have gotten hundreds of responses to my autoethnographic stories about loss and identity from those who have had similar experiences. I (TEA) also receive many responses from people who, after reading my stories about coming out, ask for advice about how to love their queer selves or queer others and from people who, after reading stories about my troubled familial relationships, want to learn more about disrupting harmful relational systems. For the autoethnographer, this means creating texts that do not contain “obscure references” (hooks, 1991, p. 4) or that leave our emotions and engagement with the heart (Pelias, 2000), texts that do not “keep readers at a distance” (Tillmann-Healy, 1996,

p. 80) or make others feel “insular and isolated” (Mykhalovskiy, 1996, p. 137).

Autoethnographers accomplish increased accessibility through using innovative techniques to represent experience, rather than following traditional academic writing forms (Ronai, 1995) or relying solely on prose (Adams, 2008; Lorde, 1984). They welcome and value nontextual, performative instances of research (Leavy, 2009; Pelias, 2011*b*; Pineau, 2000; Spry, 2011), as well as innovative representational forms, such as poetry (Boylorn, 2006; 2008), art (Minge, 2007), and music (Bartlett & Ellis, 2009). Embracing innovative, dynamic, and nontraditional ways to do and represent research helps transcend emotionally sterile and intellectually inaccessible academic walls.

Given these principles, how should one begin to do and write autoethnography?

Autoethnography as a Process

In this section, we discuss how to start an autoethnography, appropriate questions to ask, and key principles to consider. In particular, we focus on four ways that an autoethnographic project might begin: (1) from epiphanies or personal struggles, (2) from common experiences, (3) from dilemmas or complications in doing traditional fieldwork, and/or (4) for the purpose of adding to existing research.

Sometimes, the start of an autoethnographic project may be informal and personal, such as my (TEA) personal narrative about telling my father that I was gay (Adams, 2006). A personal struggle might extend into a more formal project about a community of others who share this struggle, as happened when I wrote about coming out of the closet (Adams, 2011). At other times, an autoethnographer might include herself as a participant in writing about another community, as I (CE) did with my collaborative interviews with Holocaust survivors (Rawicki & Ellis, 2011) or write about her experiences of researching a community, as I did after my research in an isolated fishing community (Ellis, 1995*b*).

Some autoethnographies begin with an epiphany (Denzin, 1989)—an event after which life never seems quite the same; an event that often generates pain, confusion, anger, and/or uncertainty, or that has made a person feel immensely vulnerable; an event, often a turning point, that changes the perceived and often desired trajectory of life. Some examples include the death of a sibling (Ellis, 1993), the disclosure of sexuality (Adams, 2011) or religion (Myers, 2012*b*), or, after numerous years of schooling, failing

to find a stable academic job (Herrmann, 2012). In these accounts, writers begin by describing and analyzing their personal experiences with the epiphany, focusing in particular on revealing embedded cultural politics and telling what the experience means for themselves and others.

Both of us have written numerous accounts of personal struggles (Ellis, 1993; 1995*a*; 1998*a*; 2009*b*; Adams, 2006; 2011). We often begin autoethnographic research with personal experiences riddled with pain, confusion, anger, and/or uncertainty; experiences that just don't make much sense and that seem to significantly alter our perceived trajectory about how life should work; experiences that we think about often and desperately want to understand and cope with; and experiences that illustrate interpersonal and social problems that need to be addressed.

For instance, I (TEA) structured my book—*Narrating the Closet: An Autoethnography of Same-Sex Attraction* (Adams, 2011)—around experiences that troubled me and that I knew I had to cope with in order to live the life I wanted to live. One epiphany for me was realizing that my intimate attraction toward other people could be classified as “gay.” Before this realization, throughout much of my youth, I tried to envision whom I would marry, when this might happen, and how many children I might have. As a teenager, I made many efforts to date women and find my perfect partner—a “wife.” When I began to embrace my attraction to men and recognize that marriage to a woman would be a sham, I realized that such an act would harm not only me but also the woman I married. My life changed for the better the moment I recognized and embraced my same-sex attraction; this moment significantly altered how I thought I would live.

Recognizing and embracing my same-sex attraction also required learning what it means to be gay in the United States. One common story about same-sex attraction is that such attraction is tied to coming out of the closet—the moment when a person reveals same-sex attraction to self and others—and about how to reveal the attraction safely, with care and respect. In my investigation, however, I realized that few, if any, stories in the literature told how people entered into the closet. Were they born there? Was the closet constructed around them, discursively, throughout life? Once a person comes out of the so-called closet, does the person ever go back in?

Thus, I embarked on an investigation of how I entered the closet. I started reflecting on my early

experiences of same-sex attraction and noted contradictory moments when I felt some desire for men but others pushed me toward women. I recalled times when I looked at images of men in magazines and catalogs—shirtless images of men with bulging muscles and genitalia—but didn't know what to say about my attraction. Throughout my youth, I learned that mentioning this attraction would bring on ridicule and possible abandonment from friends and family. I started writing through these experiences, spinning out their possible meanings, looking back and reflecting on how my experiences pre-coming out might resonate with others.

In addition, I also interviewed self-identified gay men about their coming out stories, read memoirs of gay men, and analyzed mass-mediated scripts that represented gay male coming out experiences.

During the research process, and after publication of my book on this topic, I came to realize further that my experiences seemed to be applicable to many people who possessed not only same-sex attraction but also a variety of other socially marginal identities; for example, coming out as atheist (Myers, 2012*b*) or as having a particular medical condition (Defenbaugh, 2011). I started with my experience in order to not only highlight cultural processes, but also to speak to others. My doing of autoethnography meant starting with a life-changing experience—an epiphany—as well as moments characterized by conflicting and painful emotions..

Some autoethnographies begin with cultural experiences that seem to perpetually happen or with common occurrences of troubling social behaviors. These experiences might include moments of racism and prejudice in everyday interaction (Boylorn, 2011; Marvasti, 2006), mundane and troubling ascriptions of heterosexuality (Foster, 2008), repetitive feelings of dissonance around the possession of and inability to call attention to a minor bodily stigma (Ellis, 1998*b*), or frequent frustrations with flippant responses to disclosures of a medical disorder (Defenbaugh, 2011). If these experiences only happened once or twice, they might be jarring but not epiphanical; with time and repetition, however, these experiences often become frustrating and indicative of larger cultural problems, and, consequently, worthy of attention.

Some autoethnographies begin by adding personal insights to the fieldwork experience. Although early ethnographers (e.g., Malinowski, 1967) may not have used the term “autoethnography” to describe such accounts, their backstage, behind-the-scene stories that described their feelings about and ethical

dilemmas in the doing of fieldwork (e.g., Barton, 2011; La Pastina, 2006; Stein, 2010) fit the category of autoethnography. Such accounts were—and still sometimes are—published as texts separate from the primary research texts (Heath, 2012).

Likewise, I (CE) published several pieces (Ellis, 1995*b*; 2007; 2009*b*) that discussed ethical dilemmas of writing about the fishing folk in my dissertation study (Ellis, 1986). I raised questions about the way I had conducted the study and about some of the ways ethnography was being done and taught at this time. What do we owe those we study? How should we treat them? How much do they have a right to know about us, both our personal lives and what we are doing in their lives? Are there ways to write about people that honor and empower them?

Some autoethnographies begin when a researcher recognizes critical silences and crucial voids in the existing research on particular topics, and, as such, uses personal experience to fill in these silences. For instance, Herrmann (2012) provides the necessary but raw complications of pursuing an academic career; Ronai (1995; 1996) writes against the absence of personal experience in research on child abuse; Ellis (1995*a*) addresses key silences in aging, dying, and close, personal relationships; Jago (2002; 2011) illustrates the day-to-day feelings of living with depression; and Tamas (2011) critiques the heightened emphasis on recovery among survivors of domestic violence. In these accounts, the authors use their particular knowledge and experience to illustrate problems with and failures in extant research.

Although these may not be the only ways to begin an autoethnographic project, they are the most common. Also note that some of the ways to begin autoethnography may overlap. For instance, a researcher might have an epiphany while doing more traditional, fieldwork research, or an autoethnographer might be aware of an epiphany or common, unsettling experiences and note the absence of any discussion of these events in existing research.

Once a start to a project is determined, the basics of fieldwork apply: take detailed notes, collect relevant texts, read popular press and more traditional research articles about the topic, and consult with others, when possible.

Doing Autoethnographic Fieldwork

If autoethnography consists of autobiography and ethnography, then doing autoethnography means using practices of autobiography or memoir and ethnography. For instance, in ethnography, a researcher usually does fieldwork by finding

a community to study, accessing this community, observing and interviewing members of this community, and then leaving to report observations. Sometimes the researcher has personal connections to this community (e.g., Barton, 2011), and sometimes the researcher does not (e.g., Stein, 2010). Sometimes the researcher will maintain contact with members of the community (e.g., Ellis, 1995*b*; Heath, 2012; Rupp & Taylor, 2011), and sometimes the researcher will not or cannot (e.g., Adelman & Frey, 1994).

But for the autoethnographer, fieldwork is a bit different (see Anderson & Glass-Coffin, 2013). If a researcher writes about personal experience with a cultural identity (e.g., race, class, gender, sexuality, age, and so on), then the field may be difficult to define and may always exist (Barton, 2011). For instance, if I (TEA) write about coming out as a cultural experience, and if I, as a gay man, feel as though I come out to a variety of audiences almost every day, then where do I begin and end my analysis (Adams, 2011)? If a researcher writes about the lives of rural black women and how race can be lived in the United States, and if she identifies as a black woman who spends much of her time in rural contexts, then when does she turn off her observations of and reactions to her raced body (Boylorn, 2006; 2011)? If I (CE) write about the experiences of possessing a minor bodily stigma such as a speech impediment (Ellis, 1998*b*) or others write about excess body hair (Paxton, 2013; Santoro, 2012), are all of our experiences open to analysis? If an author has had an eating disorder since her teens, has she been in the field of eating disorders for that long (Tillmann, 2009)? What happens when the “self and the field become one,” when “ethnography and autobiography” become “symbiotic” (Atkinson et al., 2003, p. 63)?

For the autoethnographer, everyday experience can serve as relevant “data” and everyday life can become part of an ambiguous and ever-changing field. Kleinman (2003) articulates what this may mean methodologically:

Being a fieldworker in my everyday life means that I attend to the social patterns around me, analyze my own actions, and piece together the observations I make and the words I hear. Being a *feminist* fieldworker means that I attend to the subtleties of inequalities (in race, class, gender, sexual orientation, ability, age, etc.), including the ways in which I live out sexist programming. (p. 230)

But when everyday life has the potential for fieldwork, can I include the conversations I have with students, coworkers, and relatives about my

sexuality, race, minor bodily stigma, or eating disorder in my research? Do I need IRB approval or to tell these people that I might write about them in some way at a later time (Tullis, 2013)? Must we identify as researchers always and everywhere?

The Ethics of Doing Autoethnography

Autoethnographers have connections to the communities they study, but the extent of the connections may differ depending on the project. Consequently, autoethnographers must be aware of some key ethical concerns.

Institutional review boards espouse “procedural ethics” (Ellis, 2007) when researching others. These ethics often require obtaining informed consent from the people we study, respecting and protecting vulnerable populations, and guarding access to data. Given an autoethnographer’s use of personal experience, however, relations with the IRB may be tricky: If I am writing about my experience, am I the only person who gives consent? If not, from whom do I have to get consent? Am I part of a vulnerable population? How do I protect my data, whatever I consider it to be—should I destroy my photographs, diaries, and work to forget key memories? As such, some autoethnographic projects may require IRB approval (e.g., projects that include interviews with other people), but other autoethnographic projects—projects that rely on personal narratives (Adams, 2006), are solely focused on the researchers (Ellis, Kiesinger, & Tillmann-Healy, 1997), or involve deceased others (Goodall, 2006)—may or may not, depending on the interpretations of the local IRB (see Tullis, 2013).

Although the IRB may sanction procedural ethics, most fieldwork dilemmas are not often under the purview of the IRB. These dilemmas are complicated and impossible to address with any kind of certainty. Responsible autoethnographers, however, acknowledge what I (CE) call “relational ethics” (Ellis, 2007)—ethics that apply to the people implicated in or represented by autoethnographic works and ethics that apply to studying others with whom we have familiar, friendly, and meaningful connections (e.g., parents, friends, students). For instance, if a daughter does not want to show her mother an essay she wrote about the two of them, then, as an ethical autoethnographer, the daughter should at least acknowledge and make a persuasive case about why she chose not to do so (Ellis, 2001). Or, if an autoethnographer wants to use conversations overheard in a classroom, at a meeting, or on a bus, it might be

necessary to mask identifying details of the people participating in the conversation, especially if the others have no idea that the researcher is writing about them (Adams, 2011; Barton, 2011). Or, if autoethnographers write about their experiences with a community, they must consider if and how they allow the community to respond to their work, especially if members of the community cannot read or understand academic jargon, or if they speak a language different from that spoken by the researchers (Adams, 2008; Tomaselli, 2003). Or, if a researcher does not believe a person or community should know of or needs to approve the autoethnographic work, then the autoethnographer should at least justify why access has not or cannot be granted (see Adams, 2006; Ellis, 2009*b*; Kiesinger, 2002). Regardless of course of action, responsible autoethnographers welcome such ethical considerations, and, at the least, should make sure to acknowledge and justify how they might proceed with a project, especially a project that implicates easily identifiable others.

Furthermore, even if an autoethnographer gets IRB approval to enter the field and study others, there are ethics about leaving the field, too. If I have come in to interview a person about an intimate and controversial topic, what gives me the right to sever that relationship? If I establish a relationship grounded on my need to research, and the other person comes to consider me a friend, am I able to ethically cut ties to this person? I might have approached the relationship in a utilitarian way, trying to figure out how it can best serve me and my project, but that doesn't mean the other person will view the situation similarly. I might have received informed consent from another person, but that does not mean my ethical obligations are satisfied. Closeness to and intimacy with the people we study can offer insights that distanced and impersonal observation cannot, but closeness and intimacy can also facilitate meaningful, and possibly even intimate, connections that make it difficult to aggressively and determinately leave the field. As autoethnographers, we have responsibilities for the relationships we help cultivate, or else we will continue to function as patronizing and elitist members of society and keep the perception of the academic ivory tower intact. Furthermore, given the personal characteristic of autoethnography, the autoethnographer cannot leave the metaphorical field; we cannot easily run away from our identities and experiences, neighbors and colleagues, friends and family.

Autoethnography as Product

Autoethnography can take numerous, inter-related forms. The following are common forms, which also illustrate some of the aforementioned principles of autoethnography (e.g., the use and valuing of insider knowledge, the attempt to make research accessible). We present these as possible kinds, not as definitive representational forms that autoethnographies must resemble.

Personal narratives are the most common form of autoethnography. Here, the autoethnographer tells stories about her or his life (Pelias, 2011; Pineau, 2000) with the hope that others will use these stories to better understand and cope with their lives (Ellis, 2004; 2009*a*). In these first-person accounts, the inner workings of the self are investigated and presented in concrete action, thoughts, and feelings; developed and problematized relationally through dialogue; shown processually in vivid scenes and dramatic plot; and contextualized by history, social structure, and culture, which themselves often operate as unstated subtexts that are dialectically revealed through action, thought, and language (Ellis, 1998*a*, p. 50).

Personal narratives often are the most controversial form of autoethnography, especially if the stories do not include more traditional academic analysis or are not situated among relevant scholarly literature. These are the autoethnographies critics often charge as not being research or as being narcissistic and self-indulgent. Of course, as authors, we reject that assessment for successful autoethnographies, given that we have learned much from personal narratives—our own and others'—about social and cultural life. We suggest that it is narcissistic to think that we are somehow outside our studies and not subject to the same social forces and cultural conditioning as those we study or that somehow our own actions and relationships need no reflexive thought (Ellis, 2004, p. 34).

Layered accounts (Rambo, 2005; Ronai, 1992; 1995; 1996) are texts that assemble fragments of personal experience, memory, extant research, introspection (Ellis, 1991), and other sources of information alongside each other in creative and juxtaposed ways. The primary purpose of layered accounts is to textually represent selves as lived—as fragmented, uncertain, and exposed to different kinds of information at different times. With layered accounts, autoethnographers work as bricoleurs who make textual mosaics of cultural experiences (Denzin & Lincoln, 2005*b*).

Interactive interviews take place when two or more people come together to share their stories

about a cultural identity, experience, or epiphany (Ellis, Kiesinger, & Tillmann-Healy, 1997; Ellis & Berger, 2001). What people learn together in the interview process is valued along with the experiences each person brings to the interview (see also Schoen & Spangler, 2011). Furthermore, interactive interviews allow all participants in the interview process to meaningfully participate, and a distinction between interviewer and interviewee is, in the best interactive interviews, indistinguishable.

Reflexive, dyadic interviews also involve interviews with other people. However, in this form of autoethnography, personal experience is used to complement interview data; there is a focus on the self and the other—an emphasis on the thoughts, feelings, and subject positions of the researcher, as well as on interviewees' stories (Ellis, 2004; Marvasti, 2006). When using reflexive, dyadic interviews, autoethnographers might reflect deeply on the personal experience that brought them to the topic, what they learned about and from themselves and their emotional responses in the course of the interview, and/or how they used knowledge of the self or the topic at hand to understand what the interviewee was saying (Ellis & Berger, 2001, p. 854).

Reflexive, dyadic interviews are different from oral histories in that the researcher not only intentionally contributes to the interview but also notes personal feelings and struggles before, during, and after the interview.

Co-constructed narratives are tales jointly constructed by relational partners used to show multiple perspectives of an epiphany (Ellis & Berger, 2001, p. 859). When doing a co-constructed narrative, two or more people write about their experiences of the agreed upon epiphany, come together to talk about their separate stories and their reactions to the other accounts, and then assemble all of their stories into one collective story (see Alexander, Moreira, & Kumar, 2012; Bochner & Ellis, 1995; Cann & DeMeulenaere, 2012; Hill & Holyoak, 2011). Co-constructive narratives are helpful for illustrating how people “cope with the untidy ambiguities, ambivalences, and contradictions of relationship life” (Ellis, 1998a, p. 50).

Indigenous/native autoethnographies are “self-made portraits” composed by colonized or economically subordinated people (Erikson, 2004, p. 346). Focused on “transforming the conditions of knowledge production” (Bainbridge, 2007, p. 54), these autoethnographies address the workings and abuses of power in culture, research, and representation, and they often try to correct the inaccuracies and

harms of extant research. Indigenous autoethnographies acknowledge the “immediate ecology” of a community, as well as the community’s spiritual practices, stories, rituals, “various forms of literacies in holistic ideographic systems,” and “legendary archetypes” (Battiste, 2008, p. 499), and they disrupt the belief that a (outside) researcher has the right and authority to study marginal others.

All of these forms are interrelated and may overlap. For instance, all use personal narratives, although there is a difference in how personal experiences are integrated into the text (i.e., by itself, or along with others’ experiences). A co-constructed narrative could simultaneously be an indigenous/native autoethnography, an interactive interview might include some characteristics of a co-constructed narrative, and an indigenous/native autoethnography might be structured like a layered account.

Added to these forms are other less common types. *Reflexive autoethnographies* resemble “confessional tales” (Van Maanen, 1988) in that they describe the ways in which a researcher may be implicated by or changed during and after fieldwork (see Barton, 2011; La Pastina, 2006). *Community autoethnographies* involve collaboration with community members to investigate a particular issue (e.g., whiteness; see Toyosaki, Pensoneau-Conway, Wendt, & Leathers, 2009). *Collaborative autoethnographies* (Chang, Ngunjiri, & Hernandez, 2012; Ngunjiri, Hernandez, & Chang, 2010) involve two or more researchers writing and sharing their personal stories and analysis about some issue. And *collaborative witnessing* (Ellis & Rawicki, 2013; in press; Rawicki & Ellis, 2011) involves working with community members in a way that focuses compassionately on their lives, with the idea of developing long-term relationships between the researcher and respondent.

With all research, we story life and experience. We craft it into reports; change identifying characteristics such as time, place, and name; and, for more traditional reports, include an introduction, literature review, method, findings, discussion, and conclusion. Some autoethnographers follow this structure (e.g., Marvasti, 2006), but many autoethnographers story their experience as well as their literature reviews in novel, more evocative ways. This does not mean that experiences are fictionalized or that what is told never happened. Rather, the report is crafted in an engaging way to achieve narrative truth (Bochner, 2002)—the truth of experience—and to strive for accessibility so that the report is read by and useful for many different audiences.

Given this requirement, training in creative writing and storytelling is crucial for autoethnographers. Such training means taking classes in the craft of writing and/or performing; reading memoirs, autobiographies, and books about writing memoirs and autobiographies; working to create thick descriptions of cultural life; providing sufficient and engaging detail to bring readers into the text; and attending to characteristics of good storytelling, such as different uses of voice, plot, character development, and dramatic tension (Caulley, 2008) and other techniques often associated with fiction (Leavy, 2012). This way of representing social life is more representative of life as lived than is the traditional academic form of introduction–literature review–method–findings–analysis–conclusion, a form that reveals more about academic structure and rules than about social life, which is rarely, if ever, organized in such a way.

Evaluation and Critique

Given the focus on personal experience, autoethnographers receive criticisms that other methods do not. For instance, autoethnographers often are critiqued for using too much subjectivity and doing too little fieldwork, and, consequently, for being self-indulgent and narcissistic (Delamont, 2009). Many autoethnographers might do little *traditional* fieldwork—which consists of going out to live with a (strange, different) group, participating in this group, and then leaving to write about this group. But some of their hesitation in this regard may be because they do not want to perpetuate and participate in past, unethical research practices, especially given that the topics they often choose to study are intimate and emotional. Nevertheless, autoethnographers often do interview others and try to do so ethically (e.g., Adams, 2011; Marvasti, 2006). Given the personal nature of a particular project, they may be in the field for much of their lives, much longer than traditional ethnographers (see Barton, 2011). For many autoethnographies, there is much fieldwork to do: analyzing diaries and journals, recalling memories, searching through archives, and talking with friends and family (Goodall, 2006).

Suggesting that autoethnography is self-indulgent and narcissistic fails to recognize the ways in which selves are constituted and implicated by larger cultural systems or that there are connections between selves and these systems. The accusation assumes that researchers are self-contained entities isolated from all others, and, in using their own experiences, they are engaging in “intellectual masturbation”

that somehow occurs absent from or outside of social life (Gobo, 2008). Consequently, criticisms of too much subjectivity and too little fieldwork and of being self-indulgent and narcissistic are just too simple, especially since many of the critics who make these judgments do so because they do not approve of the approach under any circumstances; by their standards and criteria, a good autoethnography could never exist.

Traditional notions of generalizability, validity, and reliability also are inadequate for evaluating autoethnography. Ethnography is the study of a particular culture by a particular researcher who is part of what is studied. As such, generalizing the findings of ethnography or autoethnography across people/cultures or thinking in terms of replicability is dangerous and impossible. An autoethnographer might make some tentative comparisons about others, but should never suggest that others’ experiences are the same. Rather, an autoethnographer often turns to readers to assess generalizability as they determine if a story speaks to them about their experiences or about the lives of others they know. Readers provide theoretical validation by comparing their lives to ours, by thinking about how our lives are similar and different and the reasons for these differences (Ellis, 2004, p. 195). Validity in autoethnography means that our work seeks verisimilitude; it evokes in readers a feeling that the experience described is lifelike, believable, and possible. The validity of an autoethnography can be determined by whether it helps readers communicate with others different from themselves or offers a way to improve the lives of participants and researchers (Ellis, 2004, p. 124).

Along with these revised uses of generalizability, validity, and reliability, we suggest that writers and critics should examine autoethnographies based on the principles of the method that we have offered in this chapter. More specifically, we should expect autoethnographies to (1) use personal experience; (2) have a familiarity with existing research; (3) describe and/or critique cultural experience; (4) illuminate insider knowledge; (5) break silence and reclaim voice about a topic; (6) maneuver through pain, confusion, anger, and/or uncertainty; (7) and be accessible.

Other scholars offer additional criteria for evaluating autoethnographic texts. Laurel Richardson (2000) states that good autoethnography helps us understand cultural life, has “aesthetic merit” in that the text is “artistically shaped, satisfying, complex, and not boring,” and illustrates important facets of the “author’s subjectivity.” Autoethnographers,

Richardson (2009) says, should be “concerned with (1) literary values, (2) narrative thrust, (3) reflexivity, and (4) the ethics of research and representation” (p. 346).

Art Bochner (2000) suggests that autoethnographies should contain “abundant” and “concrete” details, “not only facts but also feelings.” He asks for “structurally complex narratives” that illustrate a vulnerable, honest, and emotional author, and he expects a “tale of two selves”—a “believable journey” that shows how an author is “transformed by crisis” (pp. 270–271).

Ron Pelias (2011*a*) makes a distinction between “flat” and “engaging” autoethnographic texts. A flat text—a text that would be, for us, insufficient as autoethnography—is simple and abstract. It offers “easy and ready answers” in order to “prove itself right,” positions the author as “invisible” and “bodiless,” buries “passion” and “politics,” and “decorates the status quo”; it is a smug text that “proceeds unaware of its moral consequences.” Conversely, an engaging text—a text that would be, for us, an ideal autoethnographic text—provides homage to previous research and works to “further conversation”; it is a structurally complex and tentative text, one that offers “small,” “nervous,” and “cautious” solutions; it is a text that recognizes bodies as “historically, culturally, and individually saturated,” and a text riddled with passion, danger, politics, and uncertainty; it is a text that “speaks of and to the heart,” and tries to recognize the ways in which others are implicated in the work (pp. 666–667).

These authors’ approaches to evaluating autoethnography combine criteria from the social sciences and humanities. Consequently, none of these criteria is rooted solely in traditional social science or in humanistic standards; for autoethnography, evaluative flexibility is a must.

Autoethnography as an Orientation to Research and the Living of Life

We end this chapter with a discussion of the ways in which autoethnography describes an orientation to research and a way of living in addition to being a set of practices and products. First, I (CE) briefly describe my recent study, which demonstrates an autoethnographic orientation that works in a loving, caring, and relationally engaged way to help others to tell their stories and leads into a discussion of autoethnography as an orientation to the living of life.

Collaborative Witnessing

I (CE) somehow always knew I would work with Holocaust survivors. For decades, I had been

interested in and written about loss and grief, usually my own—the loss of my partner from chronic illness (Ellis, 1995*a*), my brother from an accident (Ellis, 1993), and my mother from various illnesses of old age (Ellis, 1996; 2001). I could imagine no richer place to learn more about long-term grief and coping than through talking with Holocaust survivors, people who had lived with trauma for more than sixty years.

In 2009, I began working with the University of South Florida Holocaust and Genocide Center and the Florida Holocaust Museum to interview forty-five survivors living in the Tampa Bay area (<http://guides.lib.usf.edu/content.php?pid=49131&sid=443218#>). I had found out from a friend in university administration that the Museum needed someone to interview survivors and I had jumped at the chance. Not content to do traditional interviews alone, early in this interviewing process I made arrangements to continue meeting with several survivors who showed interest in continuing to talk about their experiences. That was when I met survivor Jerry Rawicki.

I first conducted an initial four-hour interview with Jerry. I was impressed with how deeply he considered my questions and the emotionality and insight with which he told his stories. As of this writing, Jerry and I have been meeting, talking, and writing together for almost five years. We have recorded numerous collaborative sessions thus far and plan to record many more. We have published one short story together (Rawicki & Ellis, 2011) and several articles (Ellis & Rawicki, 2012; 2013; in press). The stories that we write involve months of daily back-and-forth editing, commenting, and decision making, as we figure out how best to convey his experience and gain insight into its meaning. This process involves building trusting and caring relationships that go beyond traditional research and that require an extensive—in my case, a lifelong—commitment to stay in relationship as long as it is possible to do so and welcomed by the participant.

I call this approach “collaborative witnessing.” Collaborative witnessing connects the roles of storyteller and listener so that both come to be narrators together, to know and tell *with* each other in mutual engagement of hearts and minds joined in long-term relationships and dialogic exchange. This approach encourages survivors to take greater ownership of the interpretation and meaning of their testimony and interviewers to have greater latitude for improvisation, questioning, and engagement

with survivors and their stories. As a researcher, I play an active role in the stories that are told by survivors in terms of the questions I ask and the verbal, nonverbal, and emotional responses I give, and Jerry is an integral part of the analytic process. I am a character in Jerry's telling of the story at this present moment—a person he speaks with and tells his story to—a feeling recipient and co-creator who allows myself to enter and experience Jerry's story, and who speaks and listens from a place of my own losses. Thus, we tell our story—his and mine and ours together—but it is his story that allows us insight into the experiences of survivors and adds to our understanding of grief and loss. More significantly, Jerry's well-being is the most important part of the project, as I constantly take into account his current life situation and the ways in which story-telling may affect his life, those to whom he is related, and survivors in general (Ellis & Rawicki, 2012; 2013; in press).

For us, autoethnography is not simply a way of knowing about the world, but also a way of being in the world. An autoethnographic perspective requires living consciously, emotionally, and reflexively. It asks that we not only examine our lives but also examine how and why we think, act, and feel as we do. Autoethnography requires that we observe ourselves observing, that we interrogate what we think and believe, and challenge our own assumptions, asking over and over if we have penetrated as many layers of our own defenses, fears, and insecurities as our project requires. It asks that we rethink and revise our lives, making conscious decisions about who and how we want to be. In the process, it seeks a story that is hopeful, one in which authors ultimately write themselves as survivors of the story they are living.

For us, autoethnography is a relational practice. The approach asks that we not only examine our experiences but also view them in the context of our emerging and ever-changing relationships. Autoethnography asks that we enter the experience of the other as much as we think about the experience of the self. By the other, we mean participants in our ethnographic studies and characters in our personal stories, as well as those who read and hear our stories. Autoethnography requires that we locate ourselves through the eyes of others, that we take others' roles as fully as we can, and consider why, given their histories, locations, and reflexive processes, they act on the world and respond the way they do. Autoethnography requires that we consider alternative points of view and interpretations, being

conscious in our analyses of the role of structure, power, and inequality.

Autoethnography also requires us to think about, think with, and ultimately live with and in the stories we tell and hear from others (see Frank, 1995). Autoethnography asks that we and others strive to understand and cope with our struggles, so that we might be better equipped to “bear witness” to the pain and struggles of others. We might do this by offering our stories to others, including their stories with ours, and/or assisting others in writing their stories. In so doing, our goal is to make life better and offer companionship to those who feel troubled and whose experiences have been so terrible that they may feel alone.

Both of us feel called to do our part in trying to make life better, especially for those who feel unwarranted pain and anguish. For me (TEA), I feel called to tell my stories of the closet and to lessen the harm done by the limiting constructs of “normal” (hetero)sexuality. I do so with the intent to provide companionship, cultivate hope, and make queer people feel as though their lives matter. For me (CE), I feel called to tell my personal stories of loss and grief and to be a secondary witness for Holocaust survivors, to assist in their telling and meaning-making, to listen intimately and respond from my heart to stories that are too terrifying and painful to remember in isolation. Finally, I hope to contribute to stories that might be read, remembered, and retold by future generations hoping to stem the possibility of such tragedies, as the Holocaust, happening again.

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Unstructured and Semi-Structured Interviewing

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Abstract

This chapter gives an introduction to qualitative interviewing in its unstructured and semistructured forms. Initially, the human world is depicted as a conversational reality in which interviewing takes a central position as a research method. Interviewing is presented as a social practice that has a cultural history and that today appears in a variety of different formats. A number of distinctions are introduced, which are relevant when mapping the field of qualitative interviewing between different levels of structure, numbers of participants, media of interviewing, and also interviewer styles. A more detailed exposition of semistructured life world interviewing is offered because this is arguably the standard form of qualitative interviewing today.

Key Words: Interviewing, conversation, unstructured interviews, semistructured interviews, phenomenology, discourse analysis

Qualitative interviewing has today become a key method in the human and social sciences and also in many other corners of the scientific landscape from education to the health sciences. Some have even argued that interviewing has become *the* central resource through which the social sciences—and society—engages with the issues that concern it (Rapley, 2001). For as long as we know, human beings have used conversation as a central tool to obtain knowledge about others. People talk with others in order to learn about how they experience the world, how they think, act, feel, and develop as individuals and in groups, and, in recent decades, such knowledge-producing conversations have been refined and discussed as qualitative interviews.¹

This chapter gives an overview of the landscape of qualitative interviewing, with a focus on its unstructured and semistructured forms. But what are interviews as such? In a classic text, Maccoby and Maccoby defined the interview as “a face-to-face

verbal exchange, in which one person, the interviewer, attempts to elicit information or expressions of opinion or belief from another person or persons” (Maccoby & Maccoby, 1954, p. 449). This definition can be used as a very general starting point, but we shall soon see that different schools of qualitative interviewing have interpreted, modified, and added to such a generic characterization in many different ways.

I begin this chapter by giving an introduction to the broader conversational world of human beings in which interviewing takes place. I then provide a brief history of qualitative interviewing before introducing a number of conceptual and analytical distinctions relevant for the central epistemological and theoretical questions in the field of qualitative interviewing. Particular attention is given to the complementary positions of experience-focused interviewing (phenomenological positions) and language-focused interviewing (discourse-oriented positions).

Qualitative Interviewing in a Conversational World

Human beings are conversational creatures who live a dialogical life. Humankind is, in the words of philosopher Stephen Mulhall, “a kind of enacted conversation” (Mulhall, 2007, p. 58). From the earliest days of our lives, we are able to enter into proto-conversations with caregivers in ways that involve subtle forms of turn-taking and emotional communication. The dyads in which our earliest conversations occur are known to be prior to the child’s own sense of self. We are therefore communicating, and indeed conversational, creatures before we become subjective and monological ones (Trevarthen, 1993).

Of course, we do learn to talk privately to ourselves and hide our emotional lives from others, but this is possible only because there was first an inter-subjective communicative process with others. Our relationships with other people—and also with ourselves—are thus conversational. To understand ourselves, we must use a language that was first acquired conversationally, and we try out our interpretations in dialogue with others and the world. The human self exists only within what philosopher Charles Taylor has called “webs of interlocution” (Taylor, 1989, p. 36). Our very inquiring and interpreting selves are conversational at their core; they are constituted by the numerous relationships we have and have had with other people (Brinkmann, 2012).

Unsurprisingly, conversations are therefore a rich and indispensable source of knowledge about personal and social aspects of our lives. In a philosophical sense, all human research is conversational because we are linguistic creatures and language is best understood in terms of the figure of conversation (Mulhall, 2007). Since the late nineteenth century (in journalism) and the early twentieth century (in the social sciences), the conversational process of knowing has been conceptualized under the name of *interviewing*. The term itself testifies to the dialogical and interactional nature of human life. An interview is literally an *inter-view*, an interchange of views between two persons (or more) conversing about a theme of mutual interest (Kvale & Brinkmann, 2008). *Conversation* in its Latin root means “dwelling with someone” or “wandering together with.” Similarly, the root meaning of *dialogue* is that of talk (*logos*) that goes back and forth (*dia-*) between persons (Mannheim & Tedlock, 1995, p. 4).

Thus conceived, the concept of conversation in the human and social sciences should be thought

of in very broad terms and not just as a specific research method. Certainly, conversations in the form of interviewing have been refined into a set of techniques—to be explicated later—but they are also a mode of knowing and a fundamental ontology of persons. As philosopher Rom Harré has put it: “The primary human reality is persons in conversation” (Harré, 1983, p. 58). Cultures are constantly produced, reproduced, and revised in dialogues among their members (Mannheim & Tedlock, 1995, p. 2). Thus conceived, our everyday lives are conversational to their core. This also goes for the cultural investigation of cultural phenomena, or what we call social science. It is fruitful to see language, culture, and human self-understanding as emergent properties of conversations rather than the other way around. Dialogues are not several monologues that are added together but the basic, primordial form of associated human life. In the words of psychologist John Shotter:

[W]e live our daily social lives within an ambience of conversation, discussion, argumentation, negotiation, criticism and justification; much of it to do with problems of intelligibility and the legitimation of claims to truth. (Shotter, 1993, p. 29)

The pervasiveness of the figure of conversation in human life is both a burden and a blessing for qualitative interviewers. On the one hand, it means that qualitative interviewing becomes a very significant tool with which to understand central features of our conversational world. In response to widespread critiques of qualitative research that it is too subjective, one should say—given the picture of the conversational world painted here—that qualitative interviewing is, in fact, the most *objective* method of inquiry when one is interested in qualitative features of human experience, talk, and interaction because qualitative interviews are uniquely capable of grasping these features and thus of being adequate to their subject matters (which is one definition of objectivity).

On the other hand, it is also a burden for qualitative interviewers that they employ conversations to study a world that is already saturated with conversation. If Mulhall is right that humankind is a kind of enacted conversation, then the process of studying humans by the use of interviewing is analogous to fish wanting to study water. Fish surely “know” what water is in a practical, embodied sense, but it can be a great challenge to see and understand the obvious, that with which we are so familiar

(Brinkmann, 2012). In the same way, some interview researchers might think that interviewing others for research purposes is easy and simple to do because it employs a set of techniques that everyone masters by virtue of being capable of asking questions and recording the answers. This, however, is clearly an illusory simplicity, and many qualitative interviewers, even experienced ones, will recognize the frustrating experience of having conducted a large number of interviews (which is often the fun and seemingly simple part of a research project) but ending up with a huge amount of data, in the form of perhaps hundreds or even thousand pages of transcripts, and not knowing how to transform all this material into a solid, relevant, and thought-provoking analysis. Too much time is often spent on interviewing, whereas too little time is devoted to preparing for the interviews and subsequently analyzing the empirical materials. And, to continue on this note, too little time is normally used to reflect on the role of interviewing as a knowledge-producing social practice in itself. Due to its closeness to everyday conversations, interviewing, in short, is often simply taken for granted.

A further burden for today's qualitative interviewers concerns the fact that interviewees are often almost too familiar with their role in the conversation. As Atkinson and Silverman argued some years ago, we live in an *interview society*, where the self is continually produced in confessional settings ranging from talk shows to research interviews (Atkinson & Silverman, 1997). Because most of us, at least in the imagined hemisphere we call the West, are acquainted with interviews and their more or less standardized choreographies, qualitative interviews sometimes become a rather easy and regular affair, with few breaks and cracks in its conventions and norms, even though such breaks and cracks are often the most interesting aspects of conversational episodes (Roulston, 2010; Tanggaard, 2007).

On the side of interviewers, Atkinson and Silverman find that "in promoting a particular view of narratives of personal experience, researchers too often recapitulate, in an uncritical fashion, features of the contemporary interview society" in which "the interview becomes a personal confessional" (Atkinson & Silverman, 1997, p. 305). Although the conversation in a broad sense is a human universal, qualitative interviewers often forget that the social practice of research interviewing in a narrower sense is a historically and culturally specific mode of interacting, and they too often construe "face-to-face interaction" as "the primordial, natural setting

for communication," as anthropologist Charles Briggs has pointed out (Briggs, 2007, p. 554).

As a consequence, the analysis of interviews is generally limited to what takes place during the concrete interaction phase with its questions and responses. In contrast to this, there is reason to believe that excellent interview research does not simply communicate a number of answers to an interviewer's questions (with the researcher's interpretive interjections added on), but includes an analytic focus on what Briggs has called "the larger set of practices of knowledge production that makes up the research from beginning to end" (Briggs, 2007, p. 566). Just as it is crucial in quantitative and experimental research to have an adequate understanding of the technologies of experimentation, it is similarly crucial in qualitative interviewing to understand the intricacies of this quite specific knowledge-producing practice, and interviewers should be particularly careful not to naturalize the form of human relationship that is a qualitative research interview and simply gloss it over as an unproblematic, direct, and universal source of knowledge. This, at least, is a basic assumption of the present chapter.

The History of Qualitative Interviewing

This takes us directly to the history of qualitative interviewing because only by tracking the history of how the current practices came to be can we fully understand their contingent natures and reflect on their roles in how we produce conversational knowledge through interviews today.

In one obvious sense, the use of conversations for knowledge-producing purposes is likely as old as human language and communication. The fact that we can pose questions to others about things that we are unknowledgeable about is a core capability of the human species. It expands our intellectual powers enormously because it enables us to share and distribute knowledge between us. Without this fundamental capability, it would be hard to imagine what human life would be like. It is furthermore a capacity that has developed into many different forms and ramifications in human societies. Already in 1924 could Emory Bogardus, an early American sociologist and founder of one of the first US sociology departments (at the University of Southern California) declare that interviewing "is as old as the human race" (Bogardus, 1924, p. 456). Bogardus discussed similarities and differences between the ways that physicians, lawyers, priests, journalists, detectives, social workers, and psychiatrists conduct

interviews, with a remarkable sensitivity to the details of such different conversational practices.

Ancient Roots

In a more specific sense, and more essentially related to qualitative interviewing as a scientific human enterprise, conversations were used by Thucydides in ancient Greece as he interviewed participants from the Peloponnesian Wars to write the history of the wars. At roughly the same time, Socrates famously questioned—or we might say *interviewed*—his fellow citizens in ancient Athens and used the dialogues to develop knowledge about perennial human questions related to justice, truth, beauty, and the virtues. In recent years, some interview scholars have sought to rehabilitate a Socratic practice of interviewing, not least as an alternative to the often long monologues of phenomenological and narrative approaches to interviewing (see Dinkins, 2005, who unites Socrates with a hermeneutic approach to dialogical knowledge) and also in an attempt to think of interviews as practices that can create a knowledgeable citizenry and not merely chart common opinions and attitudes (Brinkmann, 2007*a*). Such varieties of interviewing have come to be known as dialogic and confrontational (Roulston, 2010, p. 26), and I return to these later.

Psychoanalysis

If we jump to more recent times, interviewing notably entered the human sciences with the advent of Sigmund Freud's psychoanalysis, emerging around 1900. Freud is famous for his psychoanalytic theory of the unconscious, but it is significant that he developed this revolutionary theory (which, in many ways, changed the Western conceptions of humanity) through therapeutic conversations, or what he referred to as the *talking cure*. Freud conducted several hundred interviews with patients that used the patients' free associations as a conversational engine. The therapist/interviewer should display what Freud called an "even-hovering attention" and catch on to anything that emerged as important (Freud, 1963).

Freud made clear that research and treatment go hand in hand in psychoanalysis, and scholars have more recently pointed to the rich potentials of psychoanalytic conversations for qualitative interviewing today (see Kvale, 2003). For example, Wendy Hollway and Tony Jefferson have developed a more specific notion of the interview that is based on the psychoanalytic idea of "the defended subject" (Hollway & Jefferson, 2000). In their eyes, interviewees "are motivated *not* to know certain aspects

of themselves and...they *produce* biographical accounts which avoid such knowledge" (p. 169). This, obviously, has implications for how interviewers should proceed with analysis and interpretation of the biographical statements of interviewees and is a quite different approach to interviewing compared to more humanistic forms, as we shall see.

Many human and social scientists from the first half of the twentieth century were well versed in psychoanalytic theory, including those who pioneered qualitative interviewing. Jean Piaget, the famous developmental researcher, had even received training as a psychoanalyst himself, but his approach to interviewing is also worth mentioning in its own right. Piaget's (1930) theory of child development was based on his interviews with children (often his own) in natural settings, frequently in combination with different experimental tasks. He would typically let the children talk freely about the weight and size of objects, or, in relation to his research on moral development, about different moral problems (Piaget, 1932/1975), and he would notice the manner in which their thoughts unfolded.

Jumping from psychology to industrial research, Raymond Lee, one of the few historians of interviewing, has charted in detail how Piaget's so-called clinical method of interviewing became an inspiration for Elton Mayo, who was responsible for one of the largest interview studies in history at the Hawthorne plant in Chicago in the 1920s (Lee, 2011). This study arose from a need to interpret the curious results of a number of practical experiments on the effects of changes in illumination on production at the plant: it seemed that work output improved when the lighting of the production rooms was increased but also when it was decreased. This instigated an interview study, with more than 21,000 workers being interviewed for more than an hour each. The study was reported by Roethlisberger and Dickson (1939), but it was Mayo who laid out the methodological procedures in the 1930s, including careful—and surprisingly contemporary—advice to interviewers that is worth quoting at length:

1. Give your whole attention to the person interviewed, and make it evident that you are doing so.
2. Listen—don't talk.
3. Never argue; never give advice.
4. Listen to:
 - (a) what he wants to say
 - (b) what he does not want to say
 - (c) what he cannot say without help

5. As you listen, plot out tentatively and for subsequent correction the pattern (personal) that is being set before you. To test this, from time to time summarize what has been said and present for comment (e.g., “is this what you are telling me?”). Always do this with the greatest caution, that is, clarify in ways that do not add or distort.

6. Remember that everything said must be considered a personal confidence and not divulged to anyone. (Mayo, 1933, p. 65)

Many approaches to and textbooks on interviewing still follow such guidelines today, often forgetting, however, the specific historical circumstances under which this practice emerged.

Nondirective Interviewing

Not just Piaget, but also the humanistic psychologist Carl Rogers had influenced Mayo and others concerned with interviewing in the first half of the twentieth century. Like Freud, Rogers developed a conversational technique that was useful both in therapeutic contexts (so-called client-centered therapy), but also in research interviews, which he referred to as the “non-directive method as a technique for social research” (Rogers, 1945). As he explained, the goal of this kind of research was to sample the respondent’s attitudes toward herself: “Through the non-directive interview we have an unbiased method by which we may plumb these private thoughts and perceptions of the individual.” (p. 282). In contrast to psychoanalysis, the respondent in client-centered research (and therapy) is a client rather than a patient, and the client is the expert (and hardly a “defended subject”). Although often framed in different terms, many contemporary interview researchers conceptualize the research interview in line with Rogers’s humanistic, nondirective approach, valorizing the respondents’ private experiences, narratives, opinions, beliefs, and attitudes.

As Lee recounts, the methods of interviewing developed at Hawthorne in the 1930s aroused interest among sociologists at the University of Chicago, who made it part of their methodological repertoire (Lee, 2011, p. 132). Rogers himself moved to Chicago in 1945 and was involved in different interdisciplinary projects. As is well known, the so-called Chicago School of sociology was highly influential in using and promoting a range of qualitative methods, not least ethnography, and it also spawned some of the most innovative theoretical developments in the social sciences, such as symbolic interactionism (e.g., Blumer, 1969).

As the Rogerian nondirective approach to interviewing gained in popularity, early critiques of this technique also emerged. In the 1950s, the famous sociologist David Riesman and his colleague Mark Benney criticized it for its lack of interviewer involvement (the nondirective aspects), and they warned against the tendency to use the level of “rapport” (much emphasized by interviewers inspired by therapy) in an interview to judge its qualities concerning knowledge. They thought it was a prejudice “to assume the more rapport-filled and intimate the relation, the more ‘truth’ the respondent will vouchsafe” (Riesman & Benney, 1956, p. 10). In their eyes, rapport-filled interviews would often spill over with “the flow of legend and cliché” (p. 11), since interviewees are likely to adapt their responses to what they assume the interviewer expects from them (see also Lee, 2008, for an account of Riesman’s surprisingly contemporary discussion of interviewing). Issues such as these, originally raised more than fifty years ago, continue to be pertinent and largely unresolved in today’s interview research.

Classic Studies on Authoritarianism, Sexuality, and Consumerism

The mid-twentieth century witnessed a number of other large interview studies that remain classics in the field and that have also shaped public opinion about different social issues. I mention three examples here of such influential interview studies to show the variety of themes that have been studied through interviews: on authoritarianism, sexuality, and consumerism.

After World War II, there was a pressing need to understand the roots of anti-Semitism, and *The Authoritarian Personality* by the well-known critical theorist Adorno and co-workers controversially traced these roots to an authoritarian upbringing (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950). Their study was based on interviews and employed a combination of open qualitative interviews and much more structured questionnaires to produce the data. Although important knowledge of societal value may have been produced, the study has nonetheless been criticized on ethical grounds for using therapeutic techniques to get around the defenses of the interviewees in order to learn about their prejudices and authoritarian personality traits (Kvale & Brinkmann, 2008, p. 313).

Another famous interview study from the same period was Kinsey’s *Sexual Behavior in the Human Male* (Kinsey, Pomeroy, & Martin, 1948). The research group interviewed about 6,000 men for an

hour or more about their sexual behaviors, which generated results that were shocking to the public. In addition to the fascinating results, the book contains many interesting reflections on interviewing, and the authors discuss in great detail how to put the interviewees at ease, assure privacy, and how to frame the sequencing of sensitive topics (the contributions of Adorno and Kinsey are also described in Platt, 2002). As Kinsey put it in the book:

The interview has become an opportunity for him [the participant] to develop his own thinking, to express to himself his disappointments and hopes, to bring into the open things that he has previously been afraid to admit to himself, to work out solutions to his difficulties. He quickly comes to realize that a full and complete confession will serve his own interests. (Kinsey et al., 1948, p. 42)

The movie *Kinsey*, from 2004, starring Liam Neeson, is worth seeing from an interviewer's point of view because it shows these early interviewers in action.

As a third example, it can be mentioned that qualitative interviewing quickly entered market research in the course of the twentieth century, which is hardly surprising as a consumer society developed (Brinkmann & Kvale, 2005). A pioneer was Ernest Dichter, whose *The Strategy of Desire* (1960) communicates the results of an interview study about consumer motivation for buying a car. Interestingly, Dichter describes his interview technique as a "depth interview," inspired both by psychoanalysis and also by the nondirective approach of Rogers. Market and consumer research continue to be among the largest areas of qualitative interviewing in contemporary consumer society, particularly in the form of focus groups, and, according to one estimate, as many as 5 percent of all adults in Great Britain have taken part in focus groups for marketing purposes, which certainly lends very concrete support to the thesis that we live in an "interview society" (Brinkmann & Kvale, 2005).

Contemporary Conceptions of Qualitative Interviewing

Along with the different empirical studies, academics in the Western world have produced an enormous number of books on qualitative interviewing as a method, both in the form of "how to" books, but also in the form of more theoretical discussions. Spradley's *The Ethnographic Interview* (1979) and Mishler's *Research Interviewing: Context and Narrative* (1986) were two important early

books, the former being full of concrete advice about how to ask questions and the latter being a thorough theoretical analysis of interviews as speech events involving a joint construction of meaning.

Also following from the postmodern philosophies of social science that emerged in the 1980s (e.g., Clifford & Marcus, 1986; Lyotard, 1984), in the past couple of decades there has been a veritable creative explosion in the kinds of interviews offered to researchers (see Fontana & Prokos, 2007), many of which question both the idea of psychoanalysis as being able to dig out truths from the psyche of the interviewee and that the nondirective approach to interviews can be "an unbiased method," as Rogers had originally conceived it.

Roulston (2010) makes a comprehensive list of some of the most recent postmodern varieties of interviewing and also of more traditional ones (I have here shortened and adapted Roulston's longer list):

- *Neo-positivist* conceptions of the interview are still widespread and emphasize how the conversation can be used to reveal "the true self" of the interviewee (or the essence of her experiences), ideally resulting in solid, trustworthy data that are only accessible through interviews if the interviewer assumes a noninterfering role.
- *Romantic* conceptions stress that the goal of interviewing is to obtain revelations and confessions from the interviewee facilitated by intimacy and rapport. These conceptions are somewhat close to neo-positivist ones, but put much more weight on the interviewer as an active and authentic midwife who assists in "giving birth" to revelations from the interviewee's inner psyche.
- *Constructionist* conceptions reject the romantic idea of authenticity and favor an idea of a subject that is locally produced *within* the situation. Thus, the focus is on the situational practice of interviewing and a distrust toward the discourse of data as permanent "nuggets" to be "mined" by the interviewer. Instead, the interviewer is often portrayed as a "traveler" together with the interviewee, with both involved in the co-construction of whatever happens in the conversation (Kvale & Brinkmann, 2008).
- *Postmodern* and *transformative* conceptions stage interviews as dialogic and performative events that aim to bring new kinds of people and new worlds into being. The interview is depicted as a chance for people to get together and create new possibilities for action. Some transformative

conceptions focus on potential decolonizing aspects of interviewing, seeking to subvert the colonizing tendencies that some see in standard interviewing (Smith, 1999). In addition, we can mention feminist (Reinharz & Chase, 2002) and collaborative forms of interviewing (Ellis & Berger, 2003) that aim to practice an engaged form of interviewing that focuses more on the researchers' experience than in standard procedures, sometimes expressed through autoethnography, an approach that seeks to unite ethnographical and autobiographical intentions (Ellis, Adams, & Bochner, 2011).

It goes without saying that the overarching line of historical development laid out here, beginning in the earliest years of recorded human history and ending with postmodern, transformative, and co-constructed interviewing, is highly selective, and it could have been presented in countless other ways. I have made no attempt to divide up the history of qualitative interviewing into historical phases because I believe this would betray the criss-crossing lines of inspiration from different knowledge-producing practices. Socrates as an active interviewer inspires some of today's constructionist and postmodern interviewers (as we shall see), whereas Freud and Rogers—as clinical interviewers—in different ways became important to people who use interviewing for purposes related to marketing and the industry. Thus, it seems that the only general rule is that no approach is never completely left behind and that everything can be—and often is—recycled in new clothes. This should not surprise us, because the richness and historical variability of the human conversational world demand that researchers use different conversational means of knowledge production for different purposes.

An Example of Qualitative Interviewing

Before moving on, here I introduce an example of what a typical qualitative interview may look like, taken from my own research, to illustrate more concretely what we are talking about when we use the term “qualitative interviewing.”

The following excerpt is from an interview I conducted about ten years ago. It was part of a research project in which I studied ethical dilemmas and moral reasoning in psychotherapeutic practice. The project was exploratory and sought an understanding of clinical psychologists' own experiences of ethical problems in their work. The excerpt in Box 14.1 is not meant to represent an ideal interview, but rather to illustrate a common choreography that is

inherent in much qualitative interviewing across the different varieties.

These few exchanges of questions and answers follow a certain conversational flow common in qualitative interviews. This flow can be divided into (1) *question*, (2) *negotiation of meaning* concerning the question raised and the themes addressed, (3) *concrete description* from the interviewee, (4) the interviewer's *interpretation* of the description, and (5) *coda*. Then the cycle can start over with a new question, or else—as in this case—further questions about the same description can be posed.

The sequence begins when I pose a question (1) that calls for a concrete description, a question that seems to make sense to the interviewee. However, she cannot immediately think of or articulate an episode, and she expresses doubt concerning the meaning of one of the central concepts of the opening question (an “ethical dilemma”). This happens very often, and it can be quite difficult for interviewees (as for all of us) to describe concretely what one has experienced; we often resort to speaking in general terms (this characterizes professionals in particular, who have many general scripts at their disposal to articulate). There is some negotiation and attunement between us (2), before she decides to talk about a specific situation, but even though this is interesting and well described by the interviewee (3), she ends by returning (in what I call the coda) to a doubt about the appropriateness of the example. Before this, I summarize and rephrase her description (4), which she validates before she herself provides a kind of evaluation (5). After this, I have a number of follow-up questions that ask the interviewee to tell me more about the situation before a new question is introduced, and a similar conversational flow begins again.

The uncertainty of the interviewee about her own example around (2) illustrates the importance of assuring the interviewee that he or she is the expert concerning personal experience. The interviewer should make clear that, in general, there are no right or wrong answers or examples in qualitative interviewing and that the interviewer is interested in anything the interviewee comes up with. It is very common to find that participants are eager to be “good interviewees,” wanting to give the researcher something valuable, and this can paradoxically block the production of interesting stories and descriptions (although it did not in the present case).

In this case, a key point of the study became the term “ethical dilemma” itself; a term that is currently a nodal point in a huge number of different

Box 14.1 An interview on ethics and psychotherapy

At the time of the interview, the interviewee was in her early fifties and had been a practicing psychologist for about twenty-five years. The interview was conducted in Danish, and I have translated it into English myself.

After some introductory remarks and an initial briefing, I, the interviewer (SB), go straight to a question that I had prepared in advance and ask the interviewee (IE) for a description of a concrete ethical dilemma (the numbers in square brackets refer to elements of the conversation that are addressed in the text):

SB: (1) First, I'd like to ask you to think back and describe a situation from your work as a psychologist in which you experienced an ethical dilemma...or a situation that in some way demanded special ethical considerations from you.

IE: (2) Actually, I believe I experience those all the time. Well...I believe that the very fact that therapeutic work with other people demands that you keep...I don't know if it is a dilemma—that's what you asked about, right?—well, I don't know if it's a dilemma, but I think I have ethical considerations all the time. Considerations about how best to treat this human being with respect are demanded all the time...with the respect that is required, and I believe that there are many ethical considerations there. Ahm...When you work therapeutically you become very personal, get very close to another human being, and I think that is something you have to bear in mind constantly: How far are you allowed to go? How much can you enter into someone else's universe? But that is not a dilemma, is it?

SB: I guess it can be. Can you think of a concrete situation in which you faced this question about how close you can go, for example?

IE: (3) Yes, I can. I just had a...a woman, whose husband has a mental disorder, or he has had a severe personality disorder, so their family life is much affected by this. And she comes to me to process this situation of hers, having two small children and a husband, and a system of treatment, which sometimes helps out and sometimes doesn't. And it is very difficult for her to accept that someone close to her has a mental disorder or is fragile, it's actually a long process. She is a nurse and family life has more or less been idyllic before he...before the personality disorder really emerged. So it is extremely difficult for her to accept that this family, which she had imagined would be the place for her children to grow up, is not going to be like that. It is actually going to be very, very different. And she tries to fight it all the time: "It just *might* be...if only...I guess it will be..." And it is *never* going to be any different! And there lies a dilemma, I think: How much is it going to be: "This is something you *have* to face, it is *never* going to be different!" So I have to work to make her pose the question herself: "What do you think? How long time...What are your thoughts? Do you think it will be different? What do they tell you at the psychiatric hospital? What is your experience?" And right now she is getting closer to seeing...I might fear that it ends in a divorce; I am not sure that she can cope with it. But no one can know this. I think there is a dilemma here, or some considerations about how much to push and press forward.

SB: (4) Yes, the dilemma is perhaps that you—with your experience and knowledge about these matters—can see that the situation is not going to change much from its current state?

IE: It certainly won't.

SB: And the question is...

IE: ...how much I should push, for she does actually know this intellectually. (5) We have talked about it lots of times. But emotionally she hasn't...she doesn't have the power to face it. One day I told her: "I don't think you develop, I don't think anything happens to you, before you accept emotionally that he is not going to change." I put her on the spot and she kept evading it and so on, but it... "You don't accept it; I can tell that you don't accept it. You understand it intellectually, but you still hope that it passes." I pushed her a lot then. But I don't know if this is an ethical dilemma, I am not sure...

discourses with many different meanings, and it was thus interesting to hear the respondents' immediate understandings of the term. Their widespread uncertainty concerning the referents of the term (which was shared by the interviewer!) was not only understandable, but actually conducive to developing my ideas further about (professional) ethics as something occurring in a zone of doubt rather than certainty (as otherwise stressed by some of the standard procedural approaches to ethics).

When I first set out to conduct this study, I had something like a neo-positivist conception of interviewing in mind, in Roulston's sense, believing that there were certain essential features connected to the experience of ethically difficult situations. When working further with the theme, and after learning from my interviewees, I gradually grew suspicious of this idea, and I also came to appreciate a more constructionist conception of interviewing, according to which the interview situation itself—including the interviewer—plays an important role in the production of talk.

Other things to note about the example in Box 14.1 include the asymmetrical distribution of talk that can be observed between the two conversationalists: the interviewer poses rather short questions, and the interviewee gives long and elaborated answers. This is not always so (some respondents are more reluctant or simply less talkative), but this asymmetry has been highlighted as a sign of quality in the literature on qualitative interviewing (e.g., Kvale & Brinkmann, 2008). There is also quite a bit of dramatization in the interviewee's talk in the excerpt; for example, when she uses reported speech to stage a dialogue between herself and her client, which signals that she is capable with words and a good storyteller. On the side of the interviewer, we see that no attempts are made to contradict or question the interviewee's account, and the part of the interview quoted here thus looks quite a bit like that recommended by Mayo in the 1930s and by later nondirective interviewers: the interviewer listens a lot and does not talk much, he does not argue or give advice, and he plots out tentatively (in [4]) what the interviewee is saying, which is commented on and verified (cf. Mayo, 1933, p. 65).

Different Forms of Qualitative Research Interviews

The semistructured, face-to-face interview in Box 14.1 is probably very typical, but it merely represents one form an interview may take, and there is a huge variety of other forms. Each form has certain

advantages and disadvantages that researchers and recipients of research alike should be aware of. I here describe how qualitative interviews may differ in terms of structure, the number of participants in each interview, different media, and also different interviewer styles.

Structure

It is common to draw a distinction between structured, semistructured, and unstructured interviews. This distinction, however, should be thought of as a continuum ranging from relatively structured to relatively unstructured formats. I use the word "relatively" because, on the one end of the continuum, as Parker (2005) argues, there really is no such thing as a completely structured interview "because people always say things that spill beyond the structure, before the interview starts and when the recorder has been turned off" (p. 53). Utterances that "spill beyond the structure" are often important and are even sometimes the key to understanding the interviewee's answers to the structured questions. One line of criticism against standardized survey interviewing actually concerns the fact that meanings and interpretive frames that go beyond the predetermined structure are left out, with the risk that the researcher cannot understand what actually goes on in the interaction.

We might add to Parker's argument that there is also no such thing as a completely unstructured interview because the interviewer always has an idea about what should take place in the conversation. Even some of the least structured interviews, such as life history interviews that only have one question prepared in advance (e.g., "I would like you to tell me the story of your life. Please begin as far back as you remember and include as many details as possible"), provide structure to the conversation by framing it in accordance with certain specific conversational norms rather than others. Another way to put this is to say that there are no such things as nonleading questions. All questions lead the interviewee in certain directions, but it is generally preferable to lead participants only to talk about certain *themes*, rather than to specific *opinions* about these themes.

So, it is not possible to avoid structure entirely nor would it be desirable, but it is possible to provide a structure that is flexible enough for interviewees to be able to raise questions and concerns in their own words and from their own perspectives. Anthropologist Bruno Latour has argued that this is one definition of objectivity that human and social

science can work with, in the sense of “allowing the object to object” (Latour, 2000). Latour pinpoints a problem in the human and social sciences related to the fact that, for these sciences and unlike in the natural sciences “nothing is more difficult than to find a way to render objects able to object to the utterances that we make about them” (p. 115). He finds that human beings behave too easily as if they had been mastered by the researcher’s agenda, which often results in trivial and predictable research that tells us nothing new. What should be done instead is to allow research participants to be “interested, active, disobedient, fully involved in what is said about themselves by others” (p. 116). This does not imply a total elimination of structure, but it demands careful preparation and reflection on how to involve interviewees actively, how to avoid flooding the conversation with social science categories, and how to provoke interviewees in a respectful way to bring contrasting perspectives to light (Parker, 2005, p. 63).

In spite of this caveat—that neither completely structured nor completely unstructured interviews are possible—it may still be worthwhile to distinguish between more or less structure, with semistructured interviews somewhere in the middle as the standard approach to qualitative interviewing.

STRUCTURED INTERVIEWS

Structured interviews are employed in surveys and are typically based on the same research logic as questionnaires: standardized ways of asking questions are thought to lead to answers that can be compared across participants and possibly quantified. Interviewers are supposed to “read questions exactly as worded to every respondent and are trained never to provide information beyond what is scripted in the questionnaire” (Conrad & Schober, 2008, p. 173). Although structured interviews are useful for some purposes, they do not take advantage of the dialogical potentials for knowledge production inherent in human conversations. They are passive recordings of people’s opinions and attitudes and often reveal more about the cultural conventions of how one should answer specific questions than about the conversational production of social life itself. I do not address these structured forms in greater detail in this chapter.

UNSTRUCTURED INTERVIEWS

At the other end of the continuum lie interviews that have little preset structure. These are, for example, the life story interview seeking to

highlight “the most important influences, experiences, circumstances, issues, themes, and lessons of a lifetime” (Atkinson, 2002, p. 125). What these aspects are for an individual cannot be known in advance but emerge in the course of spending time with the interviewee, which means that the interviewer cannot prepare for a life story interview by devising a lot of specific questions but must instead think about how to facilitate the telling of the life story. After the opening request for a narrative, the main role of the interviewer is to remain a listener, withholding desires to interrupt and sporadically asking questions that may clarify the story. The life story interview is a variant of the more general genre of narrative interviewing about which Wengraf’s (2001) *Qualitative Research Interviewing* gives a particularly thorough account, focusing on biographical-narrative depth interviews. These need not concern the life story as a whole, but may address other, more specific storied aspects of human lives, building on the narratological insight that humans experience and act in the world through narratives. Narratives, in this light, are a root metaphor for psychological processes (Sarbin, 1986). With the more focused narrative interviews, we get nearer to semistructured interviews as the middle ground between structured and unstructured interviews.

SEMISTRUCTURED INTERVIEWS

Interviews in the semistructured format are sometimes equated with qualitative interviewing as such (Warren, 2002). They are probably also the most widespread form of interviews in the human and social sciences and are sometimes the only format given attention to in textbooks on qualitative research (e.g., Flick, 2002). Compared to structured interviews, semistructured interviews can make better use of the knowledge-producing potentials of dialogues by allowing much more leeway for following up on whatever angles are deemed important by the interviewee; as well, the interviewer has a greater chance of becoming visible as a knowledge-producing participant in the process itself, rather than hiding behind a preset interview guide. And, compared to unstructured interviews, the interviewer has a greater say in focusing the conversation on issues that he or she deems important in relation to the research project.

One definition of the qualitative research interview (in a generic form, but tending toward the semistructured format) reads: “It is defined as an interview with the purpose of obtaining descriptions

of the life world of the interviewee in order to interpret the meaning of the described phenomena” (Kvale & Brinkmann, 2008, p. 3). The key words here are *purpose*, *descriptions*, *life world*, and *interpretation of meaning*:

- *Purpose*: Unlike everyday conversations with friends or family members, qualitative interviews are not conducted for their own sake; they are not a goal in themselves, but are staged and conducted to serve the researcher’s goal of producing knowledge (and there may be other, ulterior goals like obtaining a degree, furthering one’s career, positioning oneself in the field, etc.). All sorts of motives may play a role in the staging of interviews, and good interview reports often contain a reflexive account and a discussion of both individual and social aspects of such motives (does it matter, for example, if the interviewer is a woman, perhaps identifying as a feminist, interviewing other women?). Clearly, the fact that interviews are conversations conducted for a purpose, which sets the agenda, raises a number of issues having to do with power and control that are important to reflect on for epistemic as well as ethical reasons (Brinkmann, 2007*b*).

- *Descriptions*: In most interview studies, the goal is to obtain the interviewee’s descriptions rather than reflections or theorizations. In line with a widespread phenomenological perspective (explained more fully later), interviewers are normally seeking descriptions of how interviewees experience the world, its episodes and events, rather than speculations about why they have certain experiences. Good interview questions thus invite interviewees to give descriptions; for example, “Could you please describe a situation for me in which you became angry?” “What happened?” “How did you experience anger?” “How did it feel?” (of course, only one of these questions should be posed at a time), and good interviewers tend to avoid more abstract and reflective questions such as “What does anger mean to you?” “If I say ‘anger,’ what do you think of then?” “Why do you think that you tend to feel angry?” Such questions may be productive in the conversation, but interviewers will normally defer them until more descriptive aspects have been covered.

- *Life world*: The concept of the life world goes back to the founder of phenomenology, Edmund Husserl, who introduced it in 1936, in his book *The Crisis of the European Sciences* to refer

to the intersubjectively shared and meaningful world in which humans conduct their lives and experience significant phenomena (Husserl, 1954). It is a prereflective and pretheorized world in which anger, for example, is a meaningful human expression in response to having one’s rights violated (or something similar) before it is a process occurring in the neurophysiological and endocrinological systems (“before” should here be taken in a logical, rather than temporal, sense). If anger did not appear to human beings as a meaningful experienced phenomenon in their life world, there would be no reason to investigate it scientifically because there would, in a sense, be nothing to investigate (since anger is primarily identified as a life world phenomenon). In qualitative research in general, as in qualitative interviewing in particular, there is a primacy of the life world as experienced, as something prior to the scientific theories we may formulate about it. This was well expressed by Maurice Merleau-Ponty, another famous phenomenologist, who built on the work of Husserl:

All my knowledge of the world, even my scientific knowledge, is gained from my own particular point of view, or from some experience of the world without which the symbols of science would be meaningless. The whole universe of science is built upon the world as directly experienced [i.e., the life world], and if we want to subject science itself to rigorous scrutiny and arrive at a precise assessment of its meaning and scope, we must begin by re-awakening the basic experiences of the world of which science is the second order expression. (Merleau-Ponty, 1945/2002, p. ix)

Objectifying sciences give us second-order understandings of the world, but qualitative research is meant to provide a first-order understanding through concrete description. Whether interview researchers express themselves in the idiom of phenomenology, or use the language of some other qualitative paradigm (discourse analysis, symbolic interactionism, ethnomethodology, etc.), they most often decide to use interviews to elicit descriptions of the life world—or whatever term the given paradigm employs: the interaction order (to speak with Erving Goffman, an exponent of symbolic interactionism), the immortal ordinary society (to speak with Harold Garfinkel, the founder of ethnomethodology), or the set of interpretative

repertoires that make something meaningful (to speak with Jonathan Potter and Margaret Wetherell, significant discursive psychologists).²

• *Interpret the meaning:* Even if interviewers are generally interested in how people experience and act in the world prior to abstract theorizations, they must nonetheless often engage in interpretations of people's experiences and actions as described in interviews. One reason for this is that life world phenomena are rarely transparent and "monovocal" but are rather "polyvocal" and sometimes even contradictory, permitting multiple readings and interpretations. Who is to say what someone's description of anger signifies? Obviously, the person having experienced the anger should be listened to, but if there is one lesson to learn from twentieth-century human science (ranging from psychoanalysis to poststructuralism) it is that we, as human subjects, do not have full authority concerning how to understand our lives because we do not have—and can never have—full insight into the forces that have created us (Butler, 2005). We are, as Judith Butler has argued, authored by what precedes and exceeds us (p. 82), even when we are considered—as in qualitative interviews—to be authors of our own utterances. The interpretation of the meanings of the phenomena described by the interviewee can favorably be built into the conversation itself (as I tried to do at point (4) in the excerpt in Box 14.1) because this will at least give the interviewee a chance to object to a certain interpretation, but it is a process that goes on throughout an interview project.

In my opinion, too rarely do interview researchers allow themselves to follow the different, polyvocal, and sometimes contradictory meanings that emerge through different voices in interviewee accounts. Analysts of interviews are generally looking for *the* voice of the interviewee, thereby ignoring internal conflicts in narratives and descriptions. Stephen Frosh has raised this concern from a discursive and psychoanalytic perspective, and he criticizes the narrativist tendency among qualitative researchers to present human experience in ways that set up coherent themes that constitute integrated wholes (Frosh, 2007). Often, it is the case that the stories people tell are ambiguous and full of gaps, especially for people "on the margins of hegemonic discourses" (p. 637). Like Butler, Frosh finds that the human subject is never a whole, "is always riven with partial drives, social discourses that frame available modes of experience, ways of being that

are contradictory and reflect the shifting allegiances of power as they play across the body and the mind" (p. 638). If this is so, it is important to be open to multiple interpretations of what is said and done in an interview. Fortunately, some qualitative approaches do have an eye to this and have designed ways to comprehend complexity; for example, the so-called *listening guide* developed by Carol Gilligan and co-workers and designed to listen for multiple voices in interviewee accounts (for a recent version of this approach, see Sorsoli & Tolman, 2008).

To sum up, the "meanings" that qualitative interviewers are commonly looking for are often multiple, perspectival, and contradictory and thus demand careful interpretation. And there is much controversy in the qualitative communities concerning whether meanings are essentially "there" to be articulated by the interviewee and interpreted by the interviewer (emphasized in particular by phenomenological approaches) or whether meanings are constructed locally (i.e., arise dialogically in a process that centrally involves the interviewer as co-constructor, as stressed by discursive and constructionist approaches). Regardless of one's epistemological standpoint, it remains important for interviewers to make clear, when they design, conduct, and communicate their research, how they approach this thorny issue because this will make it much easier for readers of interview reports to understand and assess what is communicated.

I have now introduced a working definition of the relatively unstructured and semistructured qualitative research interview and emphasized four vital aspects: such interviews are structured by the interviewer's *purpose* of obtaining knowledge; they revolve around *descriptions* provided by the interviewee; such descriptions are commonly about *life world phenomena* as experienced; and understanding the meaning of the descriptions involves some kind of *interpretation*. Although these aspects capture what is essential to a large number of qualitative interview studies now and in the past (and likely many in the future as well), it is important to stress that all these aspects can be and have been challenged in the methodological literature.

In relation to qualitative interviewing, as in qualitative research in general, there is never one correct way to understand or practice a method or a technique because everything depends on concrete circumstances and on the researcher's intentions when conducting a particular research project. This does not mean that "anything goes" and that nothing is

never better than something else, but it does mean that what is “better” is always relative to what one is interested in doing or knowing. The answer to the question “What’s the proper definition of and approach to qualitative interviewing?” must thus be: “It depends on what you wish to achieve by interviewing people for research purposes!” Unfortunately, too many interview researchers simply take one or another approach to interviewing for granted as the only correct one and forget to reflect on the advantages and disadvantages of their favored approach (sometimes they are not even aware that other approaches exist). These researchers thus proceed without properly theorizing their means of knowledge production.

Individual and Group Interviews

It is not only the interviewer’s agenda and research interests that structure the interaction in an interview. Unsurprisingly, the number of participants also plays an important role. As the history of interviewing testifies, the standard format of qualitative interviewing is with one person interviewing another person. This format was illustrated in the example in Box 14.1, and although this chapter is not about group interviews, I briefly mention these to illustrate how they differ from conventional forms of qualitative interviewing.

GROUP INTERVIEWS

There is an increasing use of group interviews. These have been in use since the 1920s but became standard practice only after the 1950s, when market researchers in particular developed what they termed “focus group interviews” to study consumer preferences. Today, focus groups dominate consumer research and are also often used in health, education, and evaluation research; they are in fact becoming increasingly common across many disciplines in the social sciences.

In focus groups, the interviewer is conceived as a “moderator” who focuses the group discussion on specific themes of interest, and she or he will often use the group dynamic instrumentally to include a number of different perspectives on the given themes (Morgan, 2002). Often, group interviews are more dynamic and flexible in comparison with individual interviews, and they may be closer to everyday discussions. They can be used, for example, when the researcher is not so much interested in people’s descriptions of their experiences as in how participants discuss, argue, and justify their opinions and attitudes.

The standard size for a focus group is between six to ten participants, led by a moderator (Chrzanowska, 2002). Recently, qualitative researchers have also experimented with groups of only two participants (sometimes referred to as “the two-person interview,” although there are literally three people if one counts the interviewer), mainly because it makes the research process easier to handle than with larger groups, where people will often not show up. The moderator introduces the topics for discussion and facilitates the interchange. The point is not to reach consensus about the issues discussed but to have different viewpoints articulated about an issue. Focus group interviews are well suited for exploratory studies in little-known domains or about newly emerging social phenomena because the dynamic social interaction that results may provide more spontaneous expressions than occur in individual interviews.

INDIVIDUAL INTERVIEWS

Individual interviews with one interviewer and one interviewee may sometimes be less lively than group interviews, but they have a couple of other advantages: First, it is often easier for the interviewer in one-on-one interviews to lead the conversation in a direction that is useful in relation to the interviewer’s research interests. Second, when studying aspects of people’s lives that are personal, sensitive, or even taboo, it is preferable to use individual interviews that allow for more confidentiality and often make it easier for the interviewer to create an atmosphere of trust and discretion. It is very doubtful, to take a rather extreme example, that Kinsey and his colleagues could have achieved the honest descriptions of sexual behaviors from their respondents had they conducted group rather than individual interviews. And there are obviously also certain themes that simply demand one person telling a story without being interrupted or gainsaid by other participants, such as in biographical research.

Although late-modern Western culture now looks on the individual, face-to-face interview as a completely common and natural occurrence, we should be very careful not to naturalize this particular form of human relationship, as I emphasized earlier. Briggs (2007) has argued that this form of relationship implies a certain “field of communicability,” referring to a socially situated construction of communicative processes (p. 556). This construction is an artefact of cultural-historical practices and is placed within organized social fields that produce different roles, positions, relations, and forms of

agency that are frequently taken for granted. There are thus certain rights, duties, and a repertoire of acts that open up when entering the field of communicability of qualitative interviewing—and others that close down. Much about this field of communicability may seem trivial—that the interviewer asks questions and the interviewee answers, that the interviewee conveys personal information that he or she would not normally tell a stranger, that the interviewee is positioned as the expert on that person's own life, and so on—but the role of this field in the process of knowledge production is very rarely addressed by interview researchers. We too seldom stop and consider the “magic” of interviewing—that a stranger is willing to tell an interviewer so many things about her life simply because the interviewer presents herself as a researcher. Rather than naturalize this practice, we should defamiliarize ourselves with it—like ethnographers visiting a strange “interview culture”—in order to understand and appreciate its role in scientific knowledge production.

Interviewing Using Different Media

Following from Briggs's analysis of the communicability of interviewing, it is noteworthy that the otherwise standardized format of “face-to-face interaction” was named as late as the early twentieth century by the sociologist Charles Horton Cooley but was since constructed as “primordial, authentic, quintessentially human, and necessary” (Briggs, 2007, p. 553). It is sometimes forgotten that the face-to-face interview, as a kind of interaction mediated by this particular social arrangement, also has a history. Other well-known media employed in qualitative interviewing include the telephone and the internet, and here we briefly look at differences among face-to-face, telephone, and internet interviews.

FACE-TO-FACE INTERVIEWS

In face-to-face interviews, people are present not only as conversing minds, but as flesh-and-blood creatures who may laugh, cry, smile, tremble, and otherwise give away much information in terms of gestures, body language, and facial expressions. Interviewers thus have the richest source of knowledge available here, but the challenge concerns how to use it productively. In most cases, how people look and act is forgotten once the transcript is made, and the researcher carries out her analyses using the stack of transcripts rather than the embodied interaction that took place. This is a problem especially when a research assistant or someone other than the interviewer transcribes the interview because, in that

case, it is not possible to note all the nonverbal signs and gestures that occurred. If possible, it is therefore preferable for the interviewer herself to transcribe the conversations, and it is optimal to do so relatively soon after the conversations are over (e.g., within a couple of days) because this guarantees better recollection of the body language, the atmosphere, and other nontranscribable features of the interaction.

TELEPHONE INTERVIEWS

According to Shuy (2002), the telephone interview has “swept the polling and survey industry in recent years and is now the dominant approach” (p. 539). It often follows a very structured format. In a research context, the use of telephone conversations was pioneered by conversation analysts, who were able to identify a number of common conversational mechanisms (related to turn-taking, adjacency pairs such as questions–answers, etc.) from the rather constricted format that is possible over the telephone. The constricted format may in itself have been productive in throwing light on certain core features of human talk.

Shuy emphasizes a number of advantages of telephone interviewing, such as reduced interviewer effects (important in structured polling interviews, for example), better interviewer uniformity, greater standardization of questions, greater cost-efficiency, increased researcher safety (Shuy, 2002, p. 540), and—we might add—better opportunities for interviewing people who live far from the interviewer. In qualitative interviewing, however, it is not possible (nor desirable) to avoid these “interviewer effects” because the interviewer herself is the research instrument, so only the latter couple of points are relevant in this context. However, Shuy also highlights some advantages of in-person interviewing versus telephone interviewing, such as more accurate responses due to contextual naturalness, greater likelihood of self-generated answers, more symmetrical distribution of interactive power, greater effectiveness with complex issues, more thoughtful responses, and the fact that such interviews are better in relation to sensitive questions (pp. 541–544). The large majority of interviews characterized as “qualitative” are conducted face-to-face, mainly because of the advantages listed by Shuy.

INTERNET INTERVIEWS

E-mail and chat interviews are varieties of internet interviewing, with e-mail interviewing normally implying an asynchronous interaction in time, with the interviewer writing a question and then waiting

for a response, and chat interviews being synchronous or occurring in “real time” (Mann & Stewart, 2002). The latter can approach a conversational format that resembles face-to-face interviews, with its quick turn takings. When doing online ethnographies (e.g., in virtual realities on the internet), chat interviews are important (see Markham, 2005, on online ethnography). One advantage of e-mail and chat interviews is that they are “self-transcribing” in the sense that the written text itself is the medium through which researcher and respondents express themselves, and the text is thus basically ready for analysis the minute it has been typed (Kvale & Brinkmann, 2008, p. 149).

Disadvantages of such interview forms are related to the demanded skills of written communication. Not everyone is sufficiently skilled at writing to be able to express themselves in rich and detailed ways. Most research participants are also more comfortable when talking, rather than writing, about their lives and experiences. However, as the psychiatrist Finn Skårderud has pointed out, there are some exceptions here, and Skårderud emphasizes in particular that internet conversations can be useful when communicating with people who have problematic relationships to their bodies (e.g., eating disorders). For such people, the physical presence of a problematic body can represent an unwanted disturbance (Skårderud, 2003).

In concluding on the different media of interviewing, it should be emphasized that all interviews are mediated, even if only by the spoken words and the historical arrangement of questioning through face-to-face interaction, and there is no universally correct medium that will always guarantee success. Interviewers should choose their medium according to their knowledge interests and should minimally reflect on the effects of communicating through one medium rather than another. That said, most of the themes that qualitative interviewers are interested in lend themselves more easily to face-to-face interviewing because of the trust, confidentiality, and contextual richness that this format enables.

Different Styles of Interviewing

We have now seen how interviews may differ in terms of structure, number of participants, and media. Another crucial factor is the style of interviewing; that is, the way the interviewer acts and positions herself in the conversation. In relation to this, Wengraf (2001) has introduced a general distinction between “receptive” interviewer styles and assertive styles (or strategies, as he calls them),

with the former being close to Carl Rogers’s model of psychotherapy and the latter being more in line with active and Socratic approaches to interviewing (both of which were addressed earlier). Here, I describe these in greater detail as two ends on a continuum.

Receptive Interviewing

According to Wengraf, a receptive style empowers informants and enables them to have “a large measure of control in the way in which they answer the relatively few and relatively open questions they are asked” (Wengraf, 2001, p. 155). Much of what was said earlier on the historical contributions of Elton Mayo and Carl Rogers and on semistructured life world interviewing addressed the receptive style in a broad sense; this is often thought of as self-evidently correct, so that no alternatives are considered. Therefore, I devote more space to articulate the somewhat more unusual assertive style, which is attracting more and more attention today.

Assertive Interviewing

Wengraf states that an assertive style may come close to a legal interrogation and enables the interviewer “to control the responses, provoke and illuminate self-contradiction, absences, provoke self-reflexivity and development” (2001, p. 155), perhaps approaching transformative conceptions of interviewing to use Roulston’s terminology mentioned earlier.

A well-known and more positive exposition of the assertive style was developed by Holstein and Gubrium in their book on *The Active Interview* (Holstein & Gubrium, 1995). They argued that, in reality, there is not much of a choice because interviews are unavoidably interpretively active, meaning-making practices, and this would apply even when interviewers attempt a more receptive style. In this case, however, their role in meaning-making would simply be more elusive and more difficult to take into account when analyzing interview talk. A consequence of this line of argument is that it is preferable for interviewers to take their inevitable role as co-constructors of meaning into account rather than trying to downplay it.

Discourse analysts such as Potter and Wetherell (1987) have also developed an active, assertive practice of interviewing. In a classic text, they describe the constructive role of the interview researcher and summarize discourse analytic interviewing as follows:

First, variation in response is as important as consistency. Second, techniques, which allow diversity rather than those which eliminate it

are emphasized, resulting in more informal conversational exchanges and third, interviewers are seen as active participants rather than like speaking questionnaires. (Potter & Wetherell, 1987, p. 165)

Variation, diversity, informality, and an active interviewer are key, and the interview process, for Potter and Wetherell, is meant to lead to articulations of the “interpretative repertoires” of the interviewees, but without the interviewer investigating the legitimacy of these repertoires in the interview situation or the respondent’s ways of justifying them. This is in contrast to Socratic and other confronting variants of active interviews, which are designed not just to map participants’ understandings and beliefs, but also to study how participants justify their understandings and beliefs.

To illustrate concretely what a confrontative assertive style looks like, we turn to a simple and very short example from Plato’s *The Republic*, with Socrates as interviewer (discussed in Brinkmann, 2007a). The passage very elegantly demonstrates that no moral rules are self-applying or self-interpreting but must always be understood contextually. Socrates is in a conversation with Cephalus, who believes that justice (*dikaiosune*)—here “doing right”—can be stated in universal rules, such as “tell the truth” and “return borrowed items”:

“That’s fair enough, Cephalus,” I [Socrates] said. “But are we really to say that doing right consists simply and solely in truthfulness and returning anything we have borrowed? Are those not actions that can be sometimes right and sometimes wrong? For instance, if one borrowed a weapon from a friend who subsequently went out of his mind and then asked for it back, surely it would be generally agreed that one ought not to return it, and that it would not be right to do so, not to consent to tell the strict truth to a madman?”

“That is true,” he [Cephalus] replied.

“Well then,” I [Socrates] said, “telling the truth and returning what we have borrowed is not the definition of doing right.” (Plato, 1987, pp. 65–66)

Here, the conversation is interrupted by Polemarchus who disagrees with Socrates’ preliminary conclusion, and Cephalus quickly leaves to go to a sacrifice. Then Polemarchus takes Cephalus’s position as Socrates’ discussion partner and the conversation continues as if no substitution had happened.

The passage is instructive because it shows us what qualitative interviewing normally is *not*. Socrates violates almost every standard principle

of qualitative research interviewing, and we can see that the conversation is a great contrast to my own interview excerpt in Box 14.1. Socrates talks much more than his respondent, he has not asked Cephalus to “describe a situation in which he has experienced justice” or “tell a story about doing right from his own experience” or a similar concretely descriptive question probing for “lived experience.” Instead, they are talking about the definition of an important general concept. Socrates contradicts and challenges his respondent’s view. There is no debriefing or attempt to make sure that the interaction was a pleasant experience for Cephalus, the interview is conducted in public rather than private, and the topic is not private experiences or biographical details, but justice, a theme of common human interest, at least of interest to all citizens of Athens.

Sometimes, the conversation partners in the Platonic dialogues settle on a shared definition, but more often the dialogue ends without any final, unarguable definition of the central concept (e.g., justice, virtue, love). This lack of resolution—*aporia* in Greek—can be interpreted as illustrating the open-ended character of our conversational reality, including the open-ended character of the discursively produced knowledge of human social and historical life. If humankind is a kind of enacted conversation, to return to my opening remarks in this chapter, the goal of social science is perhaps not to arrive at “fixed knowledge” once and for all, but to help human beings improve the quality of their conversational reality, to help them know their own society and social practices, and debate the goals and values that are important in their lives (Flyvbjerg, 2001).

Interviews can be intentionally assertive, active, and confronting (good examples are found in Bellah, Madsen, Sullivan, Swidler, & Tipton, 1985, who explicitly acknowledge a debt to Socrates), but the assertive approach can also be employed post hoc as a more analytic perspective. Consider, for example, the excerpt in Box 14.2 from a study by Shweder and Much (1987), discussed in detail by Valsiner (2007, pp. 385–386). The interview is set in India and was part of a research project studying moral reasoning in a cross-cultural research design. Earlier in the interview, Babaji (the interviewee) has been presented with a variant of the famous Heinz dilemma (here called the Ashok dilemma), invented by moral developmental psychologist Lawrence Kohlberg to assess people’s moral capabilities (Kohlberg, 1981): a man (Heinz/Ashok) has a wife who is ill and will die if he does not steal

some medicine from a pharmacist (who refuses to sell the medicine at a price that the man can afford). According to Babaji's Hinduism, stealing is not permitted, and the interview unfolds from there (see Box 14.2).

According to Valsiner (2007), we see in the interview how the interviewer (Richard Shweder), in a very active or assertive way, does everything he can to persuade Babaji to accept the Western framing of the dilemma and see the tension between stealing for a moral reason and stealing as an immoral act. But Babaji fails to, or refuses to, see the situation as a dilemma and first attempts to articulate other possibilities in addition to stealing/not stealing (viz. give shamanistic instructions) before finally suggesting that Ashok sells himself in order to raise the money. As such, the interview flow is best understood as an active and confrontational encounter between two quite different worldviews that are

revealed exactly because the interviewer acts in a confronting, although not disrespectful, way.³

Furthermore, the excerpt illustrates how cross-cultural interviewing can be quite difficult—but also extremely interesting—not least when conducted in “noninterview societies” (Ryen, 2002, p. 337); that is, in societies where interviewing is not common or recognized as a knowledge-producing instrument. All qualitative interviewing is a collaborative accomplishment, but this becomes exceedingly visible when collaborating cross-culturally.

Analytic Approaches to Interviewing

Before closing this chapter, I give a very brief introduction to different perspectives on how to analyze interviews. Obviously, I cannot here cover the immense variety of phenomenological, discursive, conversation analytic, feminist, poststructuralist, psychoanalytic perspectives, so instead I present

Box 14.2 An interview on Hindu morality

Interviewer: Why doesn't Hindu dharma permit stealing?

Babaji: If he steals, it is a sin—so what virtue is there in saving a life. Hindu dharma keeps man from sinning.

Interviewer: Why would it be a sin? Isn't there a saying “On must jump into fire for others”?

Babaji: That is there in our dharma—sacrifice, but not stealing.

Interviewer: But if he doesn't provide the medicine for his wife, she will die. Wouldn't it be a sin to let her die?

Babaji: That's why, according to the capacities and powers which God has given him, he should try to give her shamanistic instructions and advice. Then she can be cured.

Interviewer: But, that particular medicine is the only way out.

Babaji: There is no reason to necessarily think that that particular drug will save her life.

Interviewer: Let's suppose she can only be saved by that drug, or else she will die. Won't he face lots of difficulties if his wife dies?

Babaji: No.

Interviewer: But his family will break up.

Babaji: He can marry other women.

Interviewer: But he has no money. How can he remarry?

Babaji: Do you think he should steal? If he steals, he will be sent to jail. Then what's the use of saving her life to keep the family together. She has enjoyed the days destined for her. But stealing is bad.

Our sacred scriptures tell that sometimes stealing is an act of dharma. If by stealing for you I can save your life, then it is an act of dharma. But one cannot steal for his wife or his offspring or for himself. If he does that, it is simply stealing.

Interviewer: If I steal for myself, then it's a sin?

Babaji: Yes.

Interviewer: But in this case I am stealing for my wife, not for me.

Babaji: But your wife is yours.

Interviewer: Doesn't Ashok have a duty or obligation to steal the drug?

Babaji: He may not get the medicine by stealing. He may sell himself. He may sell himself to someone for say 500 rupees for six months or one year. (Shweder & Much, 1987, p. 236)

Table 14.1 Two conceptions of interviewing

Conception of interviewing	Research instrument	Social practice
Conception of interview data	Reports, interview data as resource	Accounts, interview data as topics
Standard analytic focus	Lived experience—the “what”	Situated interaction—the “how”
Typical interviewer style	Receptive	Assertive
Main challenge	Validity of interviewee reports	Relevance of interviewee accounts
Paradigmatic background	Phenomenology, grounded theory, etc.	Discourse analysis, conversation analysis, etc.

a simplified dichotomy that should really be thought of as a continuum. The dichotomy has already played an implicit role earlier because it implies a distinction between interview talk as primarily descriptive (phenomenological) reports (concentrating on the “what” of communication) and interview talk as primarily (discursive) accounts (chiefly concerned with the “how” of talk). Phenomenological approaches to interviewing in a broad sense (exemplified in my exposition of semistructured life world interviewing) try to get as close as possible to precise descriptions of what people have experienced, whereas other analytical approaches (found, e.g., in certain schools of discourse analysis and conversation analysis) focus on how people express themselves and give accounts occasioned by the situation in which they find themselves. The two approaches are contrasted in Table 14.1, with “what” approaches on the left-hand side and “how” approaches on the right-hand side.

My inspiration for slicing the cake of qualitative interviewing in this manner comes from Talmy (2010) and Rapley (2001), who build on a distinction from Clive Seale between interview-data-as-resource and interview-data-as-topic.

Interviews as Research Instrument

Researchers working from the former perspective (corresponding to the left-hand side of Table 14.1) believe that interview data can reflect the interviewees’ reality outside the interview and consequently seek to minimize the interviewer’s effects on coloring interviewees’ reports of their everyday reality. The interview becomes a research instrument in the hands of interviewers, who are supposed to act receptively and interfere as little as possible with the interviewee reporting. The validity of the interviewees’ reports becomes a prime issue when one

approaches interviewing as a research instrument. And because interviews normally concern things experienced in the past, this significantly involves considerations about human memory and about how to enhance the trustworthiness of human recollections.

In one of the few publications to discuss the role of memory in interviewing, Thomsen and Brinkmann (2009) recommend that interviewers take the following points into account if they want to help interviewees’ improve the reporting and description of specific memories:

- Allow time for recall and assure the interviewee that this is normal.
- Provide concrete cues; for example, “the last time you were talking to a physician/nurse” rather than “a communication experience.”
- Use typical content categories of specific memories to derive cues (i.e., ongoing activity, location, persons, other people’s affect and own affect).
- Ask for recent specific memories.
- Use relevant extended time line and landmark events as contextual cues; such as “when you were working at x” to aid the recall of older memories.
- Ask the interviewee for a free and detailed narrative of the specific memory (adapted from Thomsen & Brinkmann, 2009).

Following such guidelines results in interviewee descriptions that are valid (they are about what the researcher intends them to be about) and close to the “lived experience” of something, or what was earlier referred to as “life world phenomena.” Although phenomenology is one typical paradigm to frame interviews analytically as research instruments, many other paradigms do so as well, for example grounded theory, developed by Glaser and Strauss (1967) with the intent of developing theoretical

understandings of phenomena grounded in empirical materials through meticulous coding of data.

A typical goal of qualitative analysis within a broad phenomenological perspective is to arrive at an understanding of the essential structures of conscious experience. Analysts can here apply an inductive form of analysis known as *meaning condensation* (Kvale & Brinkmann, 2008, p. 205). This refers to an abridgement of the meanings articulated by the interviewees into briefer formulations. Longer utterances are condensed into shorter statements in which the main sense of what is said is rephrased in a few words. This technique rests on the idea in phenomenology that there is a certain essential structure to the way we experience things in the life world, and this constitutes an experience *as* an experience of a given something (shame, anxiety, love, learning something new, etc.).

A specific approach to phenomenological analysis has been developed in a psychological context by Amedeo Giorgi (e.g., Giorgi & Giorgi, 2003). Giorgi breaks the analytic process down into four steps: (1) obtain a concrete description of a phenomenon (through an interview) as lived through by someone; read the description carefully and become familiar with it to get a sense of the whole, (2) establish meaning units in the description, (3) transform each meaning unit into expressions that communicate the psychological sense of the data, and (4) based on the transformed meaning units, articulate the general structure of the experience of the phenomenon (p. 170).

A large number of books exist on how to do a concrete analysis (e.g., Silverman, 2001), so I will refer the reader to these and also to relevant chapters of this handbook.

Interviewing as a Social Practice

In contrast to those approaches that see interviewing as a research instrument designed to capture the “what” of what is reported as accurately as possible, others working from more constructionist, localist, and situated perspectives have much greater analytic focus on the “how” of interviewing. They view interviewing as a social practice, as a site for a specific kind of situated interaction, which means that interview data primarily reflect “a reality constructed by the interviewee and interviewer” (Rapley, 2001, p. 304). The idea of obtaining valid reports that accurately reflect a reality outside the conversational situation is thus questioned, and the main challenge becomes instead how to explain the relevance of interview talk. That is, if what is said

in an interview is a product of this social practice itself, why is it relevant to conduct interviewing? Postmodern interviews, emphasizing performative and transformative aspects of interviewing, attempt to meet this challenge by arguing that if interviews do not concern a reality outside themselves, they can instead be used to perform or facilitate social change.

People subscribing to the right-hand side of Table 14.1 believe that interview talk should be conceived of as accounts. Unlike reports, which refer to experiences from the interviewee’s past that can be articulated when prompted, accounts are answers that are “normatively oriented to and designed for the questions that occasion them” (Talmy, 2010, p. 136). If interviewee talk is best understood as accounts, it must be seen as a kind of social action that has effects and does something in the situation of which it is a part. This perspective on interviewing is shared by some discourse and conversation analysts who limit themselves to analyzing interview talk as situated interaction.

Readers may wonder if these approaches are mutually exclusive. My own pragmatic answer is that they are not, but that none of the approaches should be brought to an extreme: it is true that huge problems are associated with viewing the interview as a site for pure, “unpolluted” reports of the past (we know too much about the constructive role of human memory and of how the social practice of interviewing mediates what is said to take this seriously). But it is also true that there are problems associated with denying that we can use our communicative powers to refer more or less accurately to past experiences. Those who follow the right-hand side of Table 14.1 to the extreme and deny that data can be resources for understanding experiences of the past still believe that their own communicative practice, materialized in their texts, are about matters outside this specific text. So, taken to extremes, both approaches become absurd, and I believe that it is now time for the two (sometimes opposed) camps to learn from one another and realize that they need not exclude one another. In my view, some of the most interesting interview studies are those in which analyses of the “what” and the “how” fertilize each other in productive ways. I end this chapter with a brief illustration of this, taken (rather shamelessly!) from a paper co-authored by myself (Musaeus & Brinkmann, 2011) that shows how an analytic look at interviews can employ perspectives from both sides at the same time. The two forms thus need not exclude each other, and some

interviews can favorably be analyzed using a combination of the two broad analytic approaches.

First a little contextualization to render the example meaningful: my colleague, Peter Musaeus, conducted in their home a relatively unstructured group qualitative interview with four members of a family that was receiving family therapy. We were interested in understanding the effects of the therapeutic process on the everyday life of the family. In the excerpt in Box 14.3, we meet Maren and Søren, a married couple, and Maren's daughter Kirstina, who was thirteen years old at the time (and we also see the interviewer's voice).⁴ In the following extract, Maren (the mother) has just made a joke about the movie *The Planet of the Apes* (a science-fiction movie telling the story of how apes are in control of the earth and keep humans as pets or slaves), and they have talked about the scene where the apes jokingly remark that females are cute, just as long as you get rid of them before puberty.

Toward the end of this sequence Søren, the father of the family, denies—as he does throughout the interview—that Maren is hitting her daughter, and he uses what the family calls a “stop sign” (line 17), which they were taught to employ in their therapy sessions. The verbal sign “STOP” (said in a loud voice) is supposed to bring the conflict cycle to a halt before it accelerates. In the interview, however,

the stop sign (like other similar signs from therapy that have been appropriated by the family members) sometimes function counter-productively to raise the conflict level because it is almost shouted by family members. The sudden question in line 20 is actually much more effective in defusing the conflict by diverting the participants' attention from the problem.

I have here just provided a glimpse of our analysis, which tries to bring forth the role of semiotic mediation—the use of signs (like the stop sign and other therapeutic tools)—in regulating social interaction in a troubled family. The point is, however, that the interview both contains family members' descriptions of their problems and challenges, thus giving us their reports of what they experience; but we also see the persons' shared past being formative of the present in the interview situation itself, resulting in quite significant accounts occasioned by the social episode itself. In short, the two analytic perspectives on interviewing (both as a resource providing reports and also as a topic in its own right, i.e., a social practice providing accounts) are mutually reinforcing in this case and have given us what we (as authors of the paper) believe is a valid analysis. Rather than just hearing people describing their problems, the interviewer is in fact witnessing the family members' problems as they play out in

Box 14.3 A family interview

1. Maren: And the comment that followed was: “Get rid of it before. . . ha, ha = “
2. Interviewer: Before it becomes a teenager?
3. Maren: Because it simply is so hard.
4. Interviewer: Yes, right, but it =
5. Kirstina: Should you also simply get rid of me?
6. Interviewer: Ha, ha.
7. Maren: No, are you crazy, I love you more than anything. But it's really hard
8. for all of us sometimes, I think.
9. Kirstina: Are you also in puberty when you hit me?
10. Maren: No, I am in the menopause, that is different.
11. Interviewer: Ha, ha.
12. Søren: You don't hit, do you? You say “when you hit”? Your mother doesn't
13. hit you.
14. Kirstina: She has hit me today and yesterday.
15. Maren: I probably did hit her but well =
16. Kirstina: Yes, but still, you may say that it isn't hitting, when you miss.
17. Søren: STOP Kirstina, it isn't true. Your mother hasn't hit you and you don't
18. hit.
19. Kirstina: No, no let's just say that.
20. Maren: Does anyone want a cream roll? (adapted from Musaeus & Brinkmann, 2011, p. 53)

their interaction, in front of him so to speak, thus offering him a chance to validate his analysis. The “what” and the “how” here intersect very closely.

Conclusion

In this chapter, I have given a broad introduction to qualitative interviewing. I have tried to demonstrate that the human world is a conversational reality in which interviewing takes a privileged position as a research method, at least in relation to a number of significant research questions that human and social scientists want to ask. Qualitative interviewing can be both a useful and valid approach, resulting in analyses with a certain objectivity in the sense that I introduced earlier. Throughout the chapter, I have kept a focus on interviewing as a social practice that has a cultural history, and I have warned against unreflective naturalization of this kind of human interaction (i.e., viewing it as a particularly natural and unproblematic way of staging human relationships).

Furthermore, I introduced a number of distinctions that are relevant when mapping the field of qualitative interviewing (e.g., between different levels of structure, numbers of participants, media of interviewing, and also interviewer styles). I also provided a detailed presentation of semistructured life world interviewing as the standard form of qualitative interviewing today.

I finally gave particular attention to two broad analytic approaches to interviewing: on the one side, experience-focused interviewing that seek to elicit accurate reports of what interviewees have experienced (in broad terms, the phenomenological positions), and, on the other side, language- and interaction-focused interviewing (discourse-oriented positions) that focus on the nature of interview interaction in its own right. In my eyes, none of these is superior per se, but each enables researchers to pose different kinds of questions to their materials. Too often, however, interviewers forget to make clear what kinds of questions they are interested in and also forget to consider whether their practice of interviewing and their analytic focus enable them to answer their research questions satisfactorily.

Future Directions

In the future, the field of qualitative interviewing is likely to continue its expansion. It is now among the most popular research tools in the human and social sciences, and nothing indicates that this trend will stop. However, a number of issues confront

qualitative interviewing as particularly pressing in my opinion:

- Using conversations for research purposes is close to an everyday practice of oral communication. We talk to people to get to know them, which—in a trivial sense—is also the goal of qualitative research. Will the focus on interviewing as a “method” (that can be articulated and perhaps spelled out procedurally) be counterproductive when the goal is human communication and getting to know people? Are we witnessing a fetishization of methods in qualitative research that is blocking the road to knowledge? And are there other ways of thinking about interviews and other “qualitative methods” than in the idiom of “methods”?

- Qualitative interviewers can now find publication channels for their work, but has the practice of interviewing become so unproblematic that people are forgetting to justify and theorize their means of knowledge production in concrete contexts? In my view, more work should be done to theorize interviewing as a social practice (the “how”), as essential to what goes on in interview interactions.

- When reporting qualitative analyses, researchers too often decontextualize interviewee statements and utterances. What person A has said is juxtaposed with the statements of person B, without any contextual clues. If an interview is a form of situated interaction, then readers of interview reports need to be provided with temporal and situational context in order to be able to interpret the talk (What question was this statement an answer to? What happened before and what came after?).

- Some qualitative researchers remain convinced that they are “on the good side” in relation to ethical questions. They “give voice” to individuals, listen to their “subjective accounts,” and are thus against the quantitative and “objectifying” approaches of other, more traditional researchers. However, qualitative interviewers should, in my view, be aware that very delicate ethical questions are an inherent part of interviewing. They should avoid the “qualitative ethicism” that sometimes characterizes qualitative inquiry, viz. that “we are good because we are qualitative.” Especially in an “interview society,” there is a need to think about the ethical problems of interviewing others (often about intimate and personal matters), when people are often seduced by the warmth and interest of interviewers to say “too much.”

Notes

1. The first journalistic interviews appeared in the middle of the nineteenth century (Silvester, 1993), and social science interviews emerged in the course of the twentieth century (see the history of interviewing recounted later in this chapter).
2. Obviously, these traditions are not identical, nor are their main concepts, but I believe that they here converge on the idea of a concretely lived and experienced social reality prior to scientific abstractions of it, which Husserl originally referred to as the life world and which remains central to most (if not all) paradigms in qualitative research.
3. Confronting interviews are sometimes misunderstood to imply a certain aggressive or disrespectful attitude, which, of course, is a misunderstanding. An interviewer can be actively and confrontingly curious and inquiring in a very respectful way, especially if she positions herself as not-knowing (ad modum Socrates in some of the dialogues) in order to avoid framing the interview as an oral examination.
4. Kirstina has an older sister, who no longer lives at home, and Søren is not the biological father of the girls. He has two children from a previous marriage. One of them has attempted suicide, which, however, is not the reason for the family's referral to therapy. The reason, instead, is Maren's violent behavior toward her daughter Kirstina.

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Oral History Interviewing: Issues and Possibilities

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Abstract

Oral history interviewing is a viable qualitative research orientation for many qualitative researchers in various disciplines. Oral history is the collection of stories, statements, and reminiscences of a person or persons who have firsthand knowledge of any number of experiences. It offers qualitative researchers a way to capture the lived experiences of participants. The techniques of oral history are those of the qualitative researcher, including interviews, document analysis, photographs, and video. The current digital era offers many opportunities to address issues and possibilities for the oral historian as qualitative researcher. Three major issues that emerge are those of social justice, arts-based approaches to oral history, and transdisciplinarity. Possibilities are endless in terms of using digital techniques for data presentation, data analysis, and dissemination. The power of oral history is the power of storytelling. By using current technology and working in a transdisciplinary context, oral history may now be more readily accessible and available to a wider population thus moving toward social justice.

Key Words: Oral history, interviewing, digital techniques, archival data, photography, transdisciplinarity, social justice

People are hungry for stories. It's part of our very being.
Storytelling is a form of history, of immortality too. It goes
from one generation to another.

—*Studs Terkel*

Many think of oral history as one recorded interview stored in a library or an archive and left on a shelf. In this article, I propose an active view of oral history that appreciates the techniques of the past and moves into the digital era with a critical eye toward analysis and interpretation of the oral history interview or set of interviews. In other words, we should consider moving beyond the oral history interview while incorporating it into any given narrative. In addition to interviews, and besides using documents in any given report, this article will treat the use of current technologies to augment the storytelling of any oral history project with an eye toward social justice. Current writers have awakened us to using a transdisciplinary

approach to qualitative research in general, and that will be a major issue for consideration here. I use choreography as a metaphor to describe and explain the current state of oral history with an eye to the future and punctuate the value of arts based approaches to oral history. The strength of oral history is that it offers a firsthand view of the lived experience of any number of participants in any moment of history. Oral history is a powerful technique for qualitative researchers. It is powerful because it tells a story of one or more person's lives. Furthermore, it renders a historical record for future generations. From these unique cases we can learn more about what it means to be part of the human condition.

Oral history is a solid way to capture lived experience. In recent memory we have a huge database of examples of completed oral histories following the disasters of September 11, 2001, and Hurricane Katrina—all of which are available online. These are also in print and on the web are free and open access. This is another admirable quality of oral history. Oral history is for the most part openly accessible in digital form, written text, and visual text. You do not have to pay to read an oral history, you may visit any library and view completed oral histories, and most recently, you may view on the internet many classic and new examples of oral history. Oral history archives are available online from virtually every corner of the earth. In this section, I discuss the issues and possibilities facing oral history in this digital era through the metaphor of choreography beginning with the basics of the oral history interview through the analysis, interpretation, and usefulness of oral history. Furthermore, in this digital era, free and moderately priced software is available to make interviewing and transcribing user friendly. Finally, future directions and possibilities in oral history will be discussed, specifically oral history as a social justice project, the value of transdisciplinarity approaches, and the value of arts based approaches to oral history.

The Choreography of Oral History

There are many resources on the web and in print defining oral history and describing basic techniques of interviewing. Here I use the term oral history as the collection of stories and reminiscences of those persons who have firsthand knowledge of any number of experiences (Janesick, 2010). I use this particular definition because it casts a wide net, is inclusive, and is moving toward oral history as a social justice project. The heart and soul of oral history is to find the testimony of someone with a story to tell. In this article, the term testimony is used in its generic meaning, giving testimony, oral or written, as a firsthand authentication of any event. Oral history and testimony provide us with an avenue of thick description, analysis, and interpretation of people's lives through probing the past in order to understand the present. The postmodern and interpretive appreciation of the study of people and their stories, those stories from persons generally on the outside or periphery of society, offer a unique opportunity to view oral history as a social justice project (Janesick, 2007). For example, women, minorities, and any person or group categorized as "the other" may find a benefit from actually

recording their stories not just for themselves but for future generations. As a social justice record is kept, the stories cannot be lost. While oral history as a genre is most often associated with the field of history, since the last century it has been readily used by the social sciences and most recently in the field of education. Many oral histories are written to describe firsthand witness accounts of traumatic events such as Hurricane Katrina survivors' oral histories or the first responders to the 9/11 World Trade Center attacks. Other types of oral history include the long interviews with soldiers returning from war fronts to document the trauma of war. The US military has been documenting all these stories for over a century, and they are catalogued in military libraries to use just one example. Initially the oral histories collected by the military were from generals. Then as time went on, a gradual movement toward documenting the experiences of the everyday soldier became a goal. Likewise, throughout history oral histories have been documented by virtually every group and every possible category of individual who has a story to tell. Another example of oral histories in a complete rendering of an era or experience is the oral histories of Holocaust survivors. Most often noticed is the project completed by Steven Spielberg, who filmed survivors of the Holocaust over a lengthy time period, and these are available for viewing through his project. If we view oral history as a continuum of stories, we find elite participants' stories on one end and ordinary participants on the other. In the middle of the continuum there is a median of combinations of stories from elite participants to everyday citizens stories. To use an example from the dance world, before his death, Merce Cunningham's dances were recorded, as were lengthy interviews with him about his art. Thus a new database will provide future dancers and choreographers with a rich and textured archive about choreography, artistic expression, improvisation, and performance. This is what we are trying to do today in the field of oral history. We are documenting the lived experience of individuals who experience life in any of its stages.

Oral History as Technique

Like the choreographer, all oral historians and qualitative researchers have to come to grips with the central techniques needed to tell a story. For oral historians, the well-tested techniques of interviewing and document analysis are first and foremost. Furthermore, in this the postmodern era, the visual image through photography and videotaping may

take prominent roles in terms of technique. Since interviewing is the heart and soul of oral history, the discussion begins here. There are literally thousands of articles in print and hundreds of books on interviewing.

Obviously, interviewing has taken hold in the social sciences, the arts, the sciences, society at large, business, and of course journalism. For the purposes of this article, we will look at interviewing in multiple ways. The first way is metaphorically by conceptualizing an interview much like a choreographer conceptualizes a dance. Both are working toward a performance activity—one a completed dance and the other a completed interview. Both are connected to some individual or group of individuals communicating through a regular feedback loop. Both work with social context, social boundaries, what to include and exclude, and what to eventually present in the form of a narrative or story.

Another way to look at interviewing is in terms of a creative habit. Like the dancer or choreographer who see dance and its technique as a creative habit, the oral historian as interviewer may view the interview as a creative habit. (See Janesick, 2011). Many choreographers have written about the creative habit (Hawkins, 1992; Tharp, 2003; De Mille, 1992). In my own field of education, it was John Dewey who wrote extensively on this topic featuring the idea of habits of mind (Dewey, 1934). I mention this to point out the transdisciplinary nature of the ideas of habits of mind and body. Transdisciplinarity has been described extensively and is influencing our understanding of research. (See Leavy, 2009, 2011, 2012). Transdisciplinary approaches are problem based, are methodologically sensitive, and are responsive to voices outside and inside the margins of society. They represent a holistic approach to research methods. For oral history, that means stretching to collaborate with at least one other discipline with high levels of integration. It means thinking in a new way about oral history and its borders. Thus it is an evolution toward developing new theoretical, conceptual, and methodological frameworks. For the oral historian, this is a custom fit. We already have at least two defined disciplines, oral history and qualitative research methodology, to begin with. Usually and most often another discipline—such as the performing or visual arts, sociology, or anthropology—may provide a third part of the triangle. If we used arts-based approaches such as film, photography, painting, dance, sculpture, theater, or graphic arts in our work, we add another textural layer. For

the purposes of this article, I will focus on interviewing as a creative habit dependent upon a collection of good habits of mind as well as practical habits. I have written about qualitative techniques as creative habits previously (Janesick, 2011). These habits include the creative habit, the writing habit, the interview habit, the observation habit, and the analysis and interpretation habit. I extend these ideas throughout this essay. Furthermore, the work of Elliot Eisner (1991, 2002) has been profoundly influential. His career, which is devoted to clarifying the importance of arts-based approaches to education, cannot be overlooked.

Interviewing as a Creative Habit

If we think about the creative act of interviewing, it may be a useful tool for oral historians and other qualitative researchers. Creativity is essentially about discovery, and interviewing allows us a great deal of room to discover the meaning of a person's life or portion of a life as well as allowing for an understanding of ourselves as researchers. I use creativity here in the sense that Csikszentmihaly (1996) views creativity, which is as a process by which a symbolic domain in the culture is changed. The creative act of interviewing is such a process for the symbolic meaning of the interview, its analysis and interpretation, and its final narrative form change the landscape of the historical record. Each researcher, dancer, choreographer, or social scientist is called upon to develop habits of mind and body that change the culture. Some practical habits for the interviewer may include preparing materials for the interview such as testing the digital voice recorder, bringing an extra thumb drive for the recorder, and bringing a battery charger if the recorder is chargeable. In other words, all the technical components need to be in order to facilitate the creative habit of interviewing. In addition, the habit of being at the site of the interview ahead of time to test equipment and see that the setting is in order is always a good practical habit to develop. Another habit of mind is to compose as many thoughtful questions as possible. It is far better to be overprepared rather than to get caught in an interview without questions. Usually five or six questions of the type described in the following section are reasonable and may yield well over an hour of interview data on tape. A simple question like, "Tell me about your day as an airline pilot" once yielded nearly two hours of interview data, leaving all the other questions for another interview time. You will learn to develop a sense of awareness and timing about your

participants in the study and rearrange accordingly. All these habits help to make way for the creative act of interviewing.

Probably the most rewarding component of any qualitative research project, especially oral history, is interviewing because it is a creative act and often requires the use of imagination much like the choreographer imagining what the dance will look like. In addition to those just noted, another useful habit to develop before the actual interview includes the reading of recent texts and articles on interviewing. For example, see Rubin & Rubin (2012) and Kvale and Brinkman (2008). Oral history texts and feminist research methods texts also have described interviewing in great detail (Yow, 1994; Reinhartz, 1992; and Hesse-Biber and Leavy, 2008). A good deal of what can be learned about interviewing ultimately may come from trial and error within long-term oral history projects by practicing the interview act. I have defined interviewing earlier as a meeting of two persons to exchange information and ideas through questions and responses, resulting in communication and joint construction of meaning about a particular topic (Janesick, 2004). With that in mind as we are always researchers in the process of conducting a study, we rely on different kinds of questions for eliciting various responses.

The How: Active Interviewing

Many agree that the mainstay of oral history is interviewing. Interviewing is well described in all fields, but for this article I agree with those who view interviewing as a type of guided conversation (Rubin & Rubin, 2012). Furthermore, I see interviewing as a creative act. Just as the choreographer must know the technique and components of a dance, the interviewer needs to prepare questions. All of us as oral historians or qualitative researchers practice our craft and get better presumably as we go. In addition, we are prepared with the latest digital equipment and have done research prior to the interview about the social context in which the interviewee is immersed. Depending on the stage of our own development as researchers, we may construct various types of interview questions.

Types of Interview Questions to Consider

BASIC DESCRIPTIVE OR

HELP-ME-UNDERSTAND QUESTIONS

Can you talk to me about the recent decision you spoke of earlier that gave you such stress concerning reporting child abuse? Tell me what happened following this decision. Help me understand what you

meant by the statement, “They are always with me.” Basically, you the interviewer probe further into the meaning of the experience of the participant.

STRUCTURAL/PARADIGMATIC QUESTIONS

Of all the things you have told me about being a social worker, what keeps you going every day? Can you walk me through a typical day? What are some of your proudest achievements? Are there days that were more difficult, and can you describe such a day?

FOLLOW-UP/CLARIFYING QUESTIONS

You mentioned that “time for meditation is important” to you. Can you tell me how you use this time? Or another example might be, “Tell me more about what you mean about your description of yourself as a ‘technology nut.’”

EXPERIENCE/EXAMPLE QUESTIONS

You mentioned that you are seeing students succeed in ways you never imagined. Can you give me an example of this success? Can you give me an example of your most difficult day during your interviews for this position? You said, “High-stakes testing is killing our school.” Can you say a bit more about this?

COMPARISON/CONTRAST QUESTIONS

You said there was a big difference between a great leader and an ordinary one. What are some of these differences? Can you describe a few for me? You mentioned that there is no simple board meeting, and at the same time you can almost predict what will be the point of contention at the meeting. Can you say more about this?

CLOSING THE INTERVIEW

Closing an interview is often difficult for both interviewer and interviewee

Another good rule of thumb for this situation is to ask questions that indicate the end of the interview and also that enable the participant to keep thinking about the information already given and quite possibly look forward to another interview. Here are two solid questions for closing an interview: *Is there anything you wish to add to our conversation today? Is there anything I have forgotten to ask and which you feel is important?* Notice that there is always room for the participant to deal elegantly with the end of the interview in the moment with such a closing set of questions. In fact many oral historians and other researchers report that participants will later

say they are still thinking about these closing questions and want to tell the researcher something that was forgotten at the time of the interview. If this occurs, there is a serious opportunity for rich data to complement the existing interview or set of interviews through a follow-up interview.

While the interview is the mainstay of oral history, many oral historians go further to augment and support the interview data. This can be done with collecting other types of data. For example, the use of demographics to develop and describe the social context is always helpful. In addition, photographs, videos, newspaper clippings of the day, and any other written documents relevant to the main themes are also useful. Documents are a mainstay and can be analyzed just as interviews are analyzed through the constant comparative method, looking for themes, and coming to some interpretation of the interviews and documents. In fact Emergent Document Analysis (EDA) has been described by Altheide et al. (2008) as a way to study and deconstruct power. In this sense, EDA moves toward a social justice orientation.

Additional helpful ideas as you design your interview project

1. Remember the categories of culture that affect how you frame a question, deliver the question, take field notes as the tape is recording, and ultimately how you make sense of the data.
 - a) Cognitive culture—how the interviewer and interviewee perceive their own context and culture
 - b) Collective culture—how both see themselves as part of a collective culture including gender, race, class, religion, and ethnicity
 - c) Descriptive culture—all those written works and works of art and science that have had an effect on both the interviewee and the person who takes the role of interviewer
2. Assumptions to be aware of while interviewing someone:
 - a) Assumption of similarities—just because you may professionally act in a role as an educator and you are interviewing another educator, do not assume similarity of thoughts, beliefs, values, etc.
 - b) Language difference—the importance of one's own first language and the misinterpretation of meaning in other language usage is critical

c) Nonverbal misinterpretation—obviously we may all read nonverbal language incorrectly, which is why you interview someone more than once, for example, and why you keep coming back to find the answer to your questions

d) Stereotypes—before interviewing, check yourself for any stereotypes and be clear about their description in your role as a researcher

e) Tendency to evaluate—while most educators continue to evaluate every spoken or written word even outside the classroom, try to avoid an evaluation of the content of given remarks

f) Stress of interviewing—if you are stressed out, the person being interviewed may pick up on those cues. Go in prepared, use all your active listening skills, relax, and enjoy the interview

Thus we see the what and how of interviewing. But, you may ask, why do we do oral history?

The Why of Doing Oral History

For the purpose of this article, I will work predominantly from a postmodern perspective to emphasize the evolution of oral history. In this perspective, oral history takes on more texture and possibly more credibility. Thus *postmodern oral history* is characterized by:

1. An interpretive approach that may include the participant in the project as a co-researcher or at least an active participant in terms of member checking the material to be included in the final report.
2. Both interviewer and interviewee use ordinary language in the final report to make the story understandable to the widest possible audience.
3. Technology is used to enhance the power of the story being told, and researchers may make multiple uses of technology and the written word to complete the story telling. Digital cameras, digital video cameras, cell phones, and other devices are regularly used as part of the narrative itself. Possible posting on YouTube or other internet site for more rapid access to a larger audience is an option. The use of blogs, wikis, social networks, and potential use of computer-assisted software for data analysis is ever present.
4. Ethical issues are discussed and brought to the forefront of the project and throughout the project.
5. Oral history is an approach to qualitative research work that continually persists and prevails

and is available in public spaces such as libraries and web sites. It is one of the most transparent and most public approaches regardless of the discipline base, such as history, sociology, education, gerontology, and medicine.

6. Oral history validates the subjectivities of participants and is proud of it. We acknowledge subjectivity and celebrate it in order to reach new understanding of someone's lived experience. This in turn helps us to make more sense of the human condition.

7. Voices and stories of those members of society typically disenfranchised and marginalized are included for study and documentation. In that regard, oral history may be seen as a social justice project.

8. Oral history is viewed as a democratic project acknowledging that any person's story may be documented using accessible means to the data.

In addition, to use the metaphor of choreography to help in understanding oral history, it is helpful to understand something about the work of choreography. Not to oversimplify, but often the choreographer asks the following questions as a general beginning to any dance/art work:

- *Who (or what) is doing*
- *What to whom (or what) and*
- *Where, in what context and*
- *Why, what were the difficulties?*

Thus oral history stands as a noteworthy approach to understanding the lived experience of any number of individuals. It is a user-friendly list of questions to guide and oral history project.

Furthermore, like the choreographer whose aim is to communicate a story of some kind to an audience, the oral historian has to communicate a story. This means that writing becomes critical for the person who is becoming an oral historian. Like the choreographer and the dancer who trains the body to perform, the prospective oral historian also is in training, particularly as a writer. In fact, writing is an athletic activity in the same way that dance and choreography are athletic activities. To write oral history, as in dance, you are engaging your mind, memory, and your body parts such as the hands, muscles, nervous system, spine, joints, eyes, ears, and brain.

Many may ask questions about why they should do oral history at all. Many wonder about the qualities that may assist the oral historian and the characteristics of oral history. In reflecting this, the

following points may serve as a guideline to think about as you become an oral historian in the field and as you begin interviewing someone.

- Oral history is holistic. Even if you are telling the story of a vignette of someone's life, that vignette gives the entire picture of the vignette. Oral history takes into account the social context, the emotional context, and the big picture.

- Oral history by virtue of telling a story looks at relationships. It is a people-centered occupation.

- Oral history usually depends on face-to-face, immediate interactions, particularly in the interview and then later with member checking. Thus oral historians should possess good communications skills.

- Oral history like all qualitative work demands equal time for analysis as the time spent in the field. Interviews do not interpret themselves. Part of the job of oral historians should be to analyze and interpret the data.

- Oral history acknowledges ethical issues that may arise in the interview. Also, oral historians recognize that ethics come into play when deciding what stays in the report and what is left out. Issues of confidentiality, protecting the rights of the participant, and other such questions are always a potential reality.

- Oral history relies on the researcher as the research instrument.

- Oral history seeks to tell a story as it is, without reference to prediction, proof, control, or generalizability. We are researching subjectivity and proud of it.

- Oral history incorporates a description of the role of the oral historian/researcher.

- Oral history incorporates informed consent and release forms or any formal documentation needed to protect persons involved in the oral history.

- Oral historians check back with participants as a *member check* to share transcripts and converse about the meaning of data.

- Oral historians read widely and do all that is possible for understanding the social context of the person being interviewed. Collecting artifacts or written documents often is part of any oral history project. Having an outlook of transdisciplinarity is helpful in oral history projects. This demands awareness of more than one discipline and a deep use of the disciplines involved as a basis for the final narrative report.

- Oral historians use all sorts of data. Even though oral history is a qualitative research

technique, demographic information, documents, and other pertinent information may be used. Arts-based representations are useful and powerful tools for oral history projects. Photos, videos, and posting stories on social media outlets and YouTube help disseminate a great deal of oral history. Archives store multiple types of data.

- Oral historians write every day and practice writing on a regular basis.
- Oral historians have deep appreciation for history and the historical context, and appreciate other disciplines and what they may offer in terms of understanding oral history.
- Oral historians may use the technology of the day such as the internet to learn from YouTube, blogs, written and posted diaries and journals, letters and any other documentation to tell a story. Digital oral history examples are widely available on the internet and beyond.
- Oral historians may use photography and film to capture someone's lived experience and to augment the narrative. As a result, oral historians need to use up-to-date digital equipment and software that allows for incoming data appropriate to the level of sophistication of the software.
- Oral historians may decide to tell the narrator's story using poetry, drama, or other art forms found in documents or and in the transcripts or craft their own poetry or use other art forms as well in the story telling.
- Oral historians by virtue of doing oral history research are gaining knowledge and insight into the human condition by understanding some aspect of someone else's lived experience. They also learn from the research they undertake.

This information of course is not new, but as individuals discover this for themselves, they can set about the task of becoming an oral historian. Many who shy away from oral history need not be intimidated.

Writing Up Oral History as a Narrative

In order to do oral history, one needs to be an above-average writer. Think of the great storytellers in print. Recall your favorite writer as you read this. Most likely this writer is adept at storytelling through a written narrative. In order to do oral history, a good strategy to employ in terms of writing is to keep a researcher reflective journal. By writing a journal of reflections, you clarify your position and situate yourself in the oral history. Writing the narrative story depends on the interview transcripts, any documents being analyzed, and any other

supporting data sets such as photographs, demographic data, artifacts, videos, and the researcher's reflective journal as well as any observations on the scene. These may help to fill out the context of the story. Likewise, the researcher reflective journal is a valuable tool. I have written in more detail earlier (Janesick, 1999, 2004) on the importance of the researcher reflective journal. Let us turn once again to that topic here to clarify some points on writing and the researcher's reflective journal.

The Researcher Reflective Journal

Journal writing as a reflective research activity has been called reflective journaling and also called reflexive by many sociologists and researchers in training. It has been most used by qualitative researchers in the social sciences, education, medicine, health, business mental health, gerontology, criminal justice, and other fields since these professionals are seeking to describe a given social setting or a person's life history in its entirety. The researcher reflective journal has proven to be an effective tool for understanding the processes of research more fully, as well as the experiences, mindsets, biases, and emotional states of the researcher. Thus it may serve to augment any oral history reporting. This inclusion of the description of the *role of the researcher* and any reflections on the processes of the oral history project can be a valuable data set to include any final reporting.

Many qualitative researchers advocate the use of a reflective journal at various points in the research project timeline. To begin with, a journal is a remarkable tool for any researcher to use to reflect upon the methods of a given work in progress, including how and when certain techniques are used in the study. Likewise, it is a good idea to track the thinking processes of the researcher and participants in a study. In fact, writing a reflective journal on the role of the researcher in any given qualitative project is an effective means to describe and explain research thought processes. Often qualitative researchers are criticized for not explaining exactly how they conducted a study. Researcher reflective journal writing is one device that assists in developing a record of how a study was designed, why certain techniques were selected, and subsequent ethical issues that evolved in the study. A researcher may track in a journal the daily workings of the study. For example, did the participant change an interview appointment? How did this subsequently affect the flow of the study? Did a serious ethical issue emerge from the conduct of the study? If so,

how was this described, explained, and resolved? These and other such questions are a few examples of the types of prompts for the writer. In addition, this emphasizes the importance of keeping a journal on the role of the researcher and for the research process throughout the entire project, in this case an oral history project.

The inclusion of the reflective journal as part of the data-collection procedure indicates to some extent the credibility and trustworthiness of this technique. Does it not also act as a source of credibility and descriptive substance for the overall project? As a research technique, keeping a journal is user friendly and often instills a sense of confidence in beginning researchers and a sense of accomplishment in experienced researchers. Many researchers verify that the use of a reflective journal makes the challenge of interviewing, observing, and taking field notes much more fluid. Researchers who use the reflective journal often become more reflective actors and better writers. Writing in a journal every day instills a habit of mind that can only help in the writing of the final research report. In education, for example, many researchers ask participants to keep a reflective journal as well and end up relating to each other as co-researchers in a given project. In this type of work, journal writing becomes an act of empowerment and illumination for both researcher and participants.

In beginning the researcher reflective journal, it is always useful regardless of the project to supply all the basic descriptive data in each entry. Information such as the date, time, place, participants, and any other descriptive information should be registered in order to provide accuracy in reporting later in the study. Especially in long-term projects, the specific evidence that locates members and activities of the project can become most useful in the final analysis and interpretation of the research findings. Journal writing has an elegant, long, and documented history itself that is useful to recall.

Journal writing began from a need to tell a story. Famous journal writers throughout history have provided us with eminent examples and various categories of journals (see Progoff 1992). For example, Progoff suggests using a dialogue journal where you and I as writers imagine a dialogue going on with the self and society. In this format, one actually writes a dialogue and answers the thoughtful questions posed. No matter what orientation taken by the journal writer, it is generally agreed that reflexive journal writing is utilized for providing crispness of description and meaning, organizing one's

thoughts and feelings, and for eventually achieving understanding. Thus the oral history researcher has a valuable tool in reflective journal writing. Basically the journal writer is interacting with one's self in a sense.

Thus the art of journal writing and subsequent interpretations of journal writing produce meaning and understanding that are shaped by genre, the narrative form used, and personal cultural and paradigmatic conventions of the writer who is the researcher, participant, and or co-researcher. As Progoff (1992) notes, journal writing is ultimately a way of getting feedback from ourselves. In so doing, this enables us to experience, in a full and open-ended way, the movement of our lives as a whole and the meaning of the oral history project. Journal writing allows one to reflect, to dig deeper into the heart of the words, beliefs, and behaviors, we describe in our journals. The act of writing down one's thoughts will allow for stepping into one's inner mind and reaching further for clarity and interpretations of the behaviors, beliefs, and words we write. The journal becomes a tool for training the research instrument, the person. Since qualitative social science relies heavily on the researcher as research instrument, journal writing can only assist researchers in reaching their goals in any given project especially in oral history projects. I see journal writing as a critical tool in becoming a solid narrative writer and a good oral historian.

Major Issues Facing Oral History Researchers: Digital Technologies, Transdisciplinarity, and Social Justice

I wish to focus on three key issues facing oral historians today: First, how can we use the many digital technologies, software, and equipment more readily and in a socially just manner? Secondly, how can transdisciplinarity enrich our narratives? Third, how might we use arts-based approaches to oral history that work in a transdisciplinary way, incorporate digital arts based approaches, and arrive closer to a social justice approach to oral history? These issues may present us with a few problems. As many will attest, when doing oral history interviews, some information is shared that is basically private, the participant would like to keep it private, and video and audio materials need to be protected just as any research report in hard text would be. In other words, ethics is an overriding umbrella for oral history researchers in terms our work. At the same time, with the proliferation of social media, YouTube, and the readily available technology to

use these media, the current generation of researchers seems dedicated if not glued to computers and other hand held devices that basically open up to the world what previously might have been private.

Oral History in the Digital Era: A Way to Use Arts-Based Approaches

Technology is a welcome addition to the oral historian's tool kit. Using technology is like choreographing a dance. You start with the basics as discussed earlier and figure out a way to tell someone's story in photographs, video, and other media. In addition some researchers use social networks such as Facebook or Twitter, blogs, wikis, YouTube, and TeacherTube to collect data and represent the data through technology. Here are some potential assistive devices for the oral historian and any qualitative researcher. By the time this volume goes to press, it is highly probable that other sites will emerge. What is happening is a move to go beyond the interview transcript(s) to make full use of the transcript(s). See the following resources and sites on the internet. for strategies that will enable the oral historian to do exactly that.

Basic Technology Tools and Trends for Inquiry

The annual *Horizon Report* describes the continuing work of the New Media Consortium's Horizon Project, a qualitative research project established in 2002 that identifies and describes emerging technologies likely to have a large impact on teaching, learning, or creative inquiry on college and university campuses within the next five years (see <http://wp.nmc.org/horizon2010> and <http://net.educause.edu/ir/library/pdf/HR2012.pdf>)

WHAT IS VOICETHREAD?

A VoiceThread (www.voicethread.com) is a collaborative, multimedia slide show that is stored and accessed online that holds images, documents, and videos and allows people to navigate pages and leave comments in five ways—using voice (with a microphone or telephone), text, audio file, or video (via a webcam). A VoiceThread can be shared with other professors, researchers, students, and the wider community for them to record comments, as well. VoiceThreads can also be downloaded as a movie file, but there is a cost for this function. VoiceThread supports PDF; Microsoft Word, Excel, and PowerPoint (including Office 2007 formats); images; and videos. VoiceThread also imports photos from Flickr, Facebook, and other websites. Each

of these allows for artistic expression including incorporation of photography and video.

VoiceThread is a flexible tool that can be used for a wide variety of uses, including:

- Orally publishing written work with matching artwork displayed on the slide
- Uploading interviews for analysis
- Describing qualitative methods and techniques in a research class
- Displaying videos for comment and feedback
- Art portfolios describing processes used at each step or just as a simple art gallery
- Gathering perspectives on an idea or concept from participants indicating a more active role for participants
- Creating an archive of interview responses.

By going to the wikis that house VoiceThread information, you can save yourself a great deal of time (see <http://readingqueens.wikispaces.com/Voicethread+Directions>).

Other resources

PowerPoint presentation on VoiceThread: <http://www.authorstream.com/Presentation/kcercone-299598-directions-using-voicethread-education-ppt-powerpoint/> <http://readingqueens.wikispaces.com/Voicethread+Directions> <http://www.frenchteachers.org/technology/VoiceThread.pdf>

Animoto

Animoto automatically produces beautifully orchestrated, completely unique video pieces from your photos, video clips and music. See: <http://animoto.com/>

With this resource, you will learn to make a video from your photographs:

Tutorial: <http://www.youtube.com/watch?v=tivxjNRFLaY>

Sample: <http://www.youtube.com/watch?v=uMiws3Qq5pY>

By using Animoto, the oral historian starts to practice ways to merge artistic expression and the written text.

Using Wordle

By using Wordle, it is possible to create a visually stunning use of word arrangements for part of the oral history narrative. Please go to: <http://www.wordle.net/>

Wordle is a tool for generating "word clouds" from text that you provide. The clouds give greater

prominence to words that appear more frequently in the source text. You can tweak your clouds with different fonts, layouts, and color schemes. The images you create with Wordle are yours to use however you like. You can print them out or save them to the Wordle gallery to share with others. See this example:

<http://www.slideshare.net/murcha/using-wordle-in-the-classroom-presentation>

Creating a Blog with WordPress to tell a story

WordPress is an open-source blog publishing application. It features integrated link management; a search engine-friendly, clean permalink structure; and the ability to assign nested, multiple categories to articles. Also multiple author capability is built into the system. There is support for tagging posts and articles. Some have made posters from WordPress to display data at conferences and other sites. In addition, WordPress is a tool that actually advertises as having beautiful graphics and photos for use. It is worth exploring (see <http://www.wordpress.com>)

Audacity

Audacity is free shareware for recording and mixing audio tracks from different sources. You can download and install it from <http://audacity.sourceforge.net>. Choose an earlier, more stable version (not the beta version). With Audacity, you can capture the actual voices of your participants.

Using Animoto to convert photos into free videos

Animoto is a free video-rendering tool that can be used to create videos from photographs. It advertises that anyone can create stunning videos, a claim I believe is accurate (see <http://animoto.com>)

WHAT IS ANIMOTO?

Animoto is a web application that allows you to create video clips using graphics in .jpg or .gif format. Thirty-second video clips are free, or you can pay an annual fee (\$30.00) and make video clips of any length. You upload your images, add some text (optional), and select your music, and Animoto does the rest. This is a useful tool for the oral historian's tool kit.

USING PHOTOGRAPHY IN ORAL HISTORY PROJECTS

Photography is a powerful research tool for oral historians and other researchers (Harper, 2003, Rose, 2007). Many fields recognize the value of and

use of photography to augment the final narrative of any given qualitative research project. In fact to use just one example here, since many libraries are adding to their digital collections, doctoral students today can expect that they will be doing completely digital dissertations. Thus current doctoral students have the opportunity to use photography in the final product of the dissertation. Hard copies are going the way of the dinosaur for many individuals. In many fields, including oral history, researchers are using PhotoVoice as a key technique.

PhotoVoice

Photo voice is a technique used in some projects to allow participants to photograph, describe, and explain their social context, particularly groups most often on the margins of society. This project began as a way for underprivileged students and parents to capture through photography neglect, abuse, and other aspects of the social context that give witness to those on the outskirts of society. For a more involved description and examples of PhotoVoice, see <http://www.photovoice.org/>.

If you do an internet search for PhotoVoice, you will find numerous articles on this activity. PhotoVoice is most often described as a process. Here people can identify, photograph, and explain their community through a specific photographic technique. PhotoVoice has various goals, including: 1. to enable an individual to keep a record and reflect a community's strengths and concerns, such as the photographs taken after Hurricane Katrina; 2. to promote critical dialogue about community issues within a given community; and 3. Eventually to reach policymakers through the power of the photograph. A growing body of PhotoVoice examples can be found on YouTube at <http://www.youtube.com/watch?v=shrFa2c305g>.

Since this is a visual medium, it is helpful to view these many examples for a model of what is possible for oral historians. Familiarity with many of these sites can be helpful in crafting the final narrative much like the choreographer improves her craft by photographing and making videos of segments of a dance or an entire dance performance.

Future Directions and Final Reflections

To make sense of oral history, choreographing the story we tell as historians and researchers includes art, experience, and inquiry. I return historically to the third chapter of *Art as Experience*, a

groundbreaking text by John Dewey (1859–1952) that suggests the following:

Experience occurs continuously because the interaction of live creature and environing condition is involved in the very process of living. [p. 35]

And while Dewey speaks about this in theory as a philosopher, choreographer Erick Hawkins (1909–1994) writes in the here and now of an actual dance in progress. Hawkins wants us to see the body as the perfect instrument of the lived experience:

Several times so called critics have judged the dancers of my company as being “too graceful.” How can you be too graceful? How can you obey the laws of movement too much? . . . The answer is a kind of feeling introspected in the body and leads one into doing the correct effort for any movement. The kinesiological rule is to just do the movement . . . The tenderness in the mind takes care of the movement in action. [1992, pp. 133–134]

Similarly, Hawkins wrote,

One of the reasons we are not accustomed as a culture to graceful movement is because we do not treasure it. The saying among the Greeks of the Athenian supremacy was that the body was to be treasured and great sensitivity was used in the observation of movement. They treasured the body by having many statues of deity . . . maybe they understood that the body is a clear place. [1992, p. 134]

We can learn from these writers as we look ahead to our qualitative oral history research projects. We can see the lessons here

1. We learn about the critical importance of experience, curiosity, imagination, and the resulting artifact, the oral history narrative, is layered and connected.
2. We learn about the power and value of the subjective experience in interpretation of the oral history interview and documents, and these may be artistically rendered.
3. We learn that the landscape of feeling and emotions cannot and should not be avoided when expressing art or artifact. In fact, these are embraced in oral history.
4. For researchers to “have the experience” of telling someone’s story, the researcher, must acknowledge the experience component of empathy, understanding, and the story itself. The oral historian must be prepared with the best possible tools and techniques of our craft.

5. We celebrate narrative storytelling in whatever form it may take but appreciate the visual options through digital media and arts-based approaches to storytelling.

6. Because we are bombarded by images through multiple forms of media, it makes sense to use multimedia to effect a powerful story by use of photography, video, and other arts based approaches to assist in using our research to move toward a more socially just world.

7. We can feel comfortable in returning to a true appreciation of storytelling, a space from which oral history derived.

One of the reasons I do qualitative research and specifically oral history is that it is multifaceted and may include more than one art form, such as writing and photography. In fact, I see oral history as an art form itself. It is through the arts that a larger audience is most likely reached than any other curricular or cultural arena. The arts can meet the need of nearly every person, no matter who that person is and no matter where that person is in the world, and so there are social justice implications. The current digital revolution is filled with art, dance, music, poetry, collage, and other art forms stored in the vast digital archives of Google and YouTube. Oral history helps us understand the power of experience, art as experience, and artifacts resulting from the experience—all of which transcend the day-to-day moments of life. In fact, storytelling is its own art form. As we tell stories about the lived experiences of our narrator, art illuminates that experience. To me, using poetry, photography, and video whenever possible helps to widen the repertoire of techniques for anyone who wishes to become an oral historian and document and interpret a story. So it seems to me that, as we practice oral history, we ourselves keep a digital record, a reflective record, and move ahead to carve out our place in the inquiry process as we are building a record of lived experience.

Three Questions for the Field

To conclude this piece, here are three questions for oral historians to think about in terms of shaping the future directions in the field.

1. How might our work be used to advance a social justice project?
2. How might we think about arts-based approaches to improve our practice?
3. How might we work in a transdisciplinary way to augment the oral history interview?

HOW MIGHT OUR WORK BE USED TO ADVANCE A SOCIAL JUSTICE PROJECT?

One of the strengths of oral history is that a diverse and multicultural knowledge base is built through the use of oral history interviewing. To use just one example, truth commissions across the globe, there is a steady stream of documenting injustice. Thus this ironically can lead us to more of a movement toward social justice. By way of explanation, think about the critical testimony of the victims of apartheid in South Africa. Here, the South African Truth and Reconciliation Commission (TRC) was the major vehicle for capturing what occurred throughout this difficult period. Ordinary citizens came forward and faced their abusers. They gave long interviews, all of which are recorded, and the perpetrators of the various crimes asked for forgiveness. The person being interviewed had to forgive the individual. Torturers, murderers, and transgressors admitted their crimes, and they were interviewed as well. There was an understanding that, once the testimony was given by both parties, forgiveness was given, the case was closed and both/all parties moved on. Desmond Tutu was the originator and overseer of the TRC. He wrote of the experience (Tutu, 1999). What these testimonials as oral history gave us was a powerful understanding of the cultural, political, emotional, and psychological aspects of apartheid as never before. Furthermore, we see the agonizing tales of brutality and its aftermath. It was an example for the world of what is possible and how to move toward social justice at least in these overt, clear cases.

As oral historians, we see how, when, where, and why people were able to recount their stories. Tutu (1999) reminds us of what he calls his four types of truths. He first describes factual or forensic truth, or the actual evidence of what occurred. Second, he mentions the personal or narrative truth of the interviewee. This was the story told by the witness giving testimony. Thus we have the actual human story and the description of the lived experience, which is the goal of oral history. Third, Tutu describes the social or cultural truth, that is the context of what occurred historically and up to the present. Here, this may also include the forensic and personal truth. Finally, he describes the restorative or healing truth, which is describes as what is needed to heal the wounds of the three previously outlined truths. So testimony as oral history becomes a way to move toward social justice. Obviously this is a unique case and one the entire world was updated on as the hearings unfolded. Yet in the everyday lives of

those we live and work with, there are many injustices that may be documented through oral history methods.

HOW MIGHT WE THINK ABOUT ARTS-BASED APPROACHES TO IMPROVE OUR PRACTICE?

While writers have already outlined many arts-based approaches to qualitative research, it makes sense to use arts based approaches to enhance the oral history narrative. For example, many possibilities for arts-based approaches include using readily available digital technology for photography and videos as described earlier in this piece. In addition, using performing arts such as playmaking and reader's theater to augment data presentation offers us many opportunities to integrate arts-based approaches to oral history. I want to focus on another type of arts-based approach: found data poetry and other forms of poetry. Found data poetry is poetry found in the data itself, such as interviews and documents from the site of the study. This means using words found in the data and also making poetry from the meaning of the text. Poetry offers us a new way to look at our data and a new way to express it. See the following short example from a transcript of an oral history project I am currently completing on oral histories of female leaders. In this excerpt, a female school assistant superintendent is from a north central state explaining one of the challenges in a particular case regarding attempted censorship of a middle school reading text. The text covered a story about a young ostracized obese student who contemplated suicide but actually realized he was good at music, so he threw his energy into music. It is actually a story of redemption, not about suicide, which students appreciated. However some fundamentalist parents tried to mount an email campaign from around the United States and Canada to object to this optional text on the summer reading list. Following this excerpt, which was used as an example of how leaders deal with the public, you will see poetry constructed from the interview.

Sample excerpt from a transcript of an interview with a female assistant superintendent of schools in a north central state

Q: Think about yesterday and today, not necessarily as typical days, but what does your day look like? Tell me about the things you deal with.

A: Well, it's been atypical days so that's... And that's something I've been thinking about. There isn't... There are typical days and they're boring.

The typical days are the days when you're sitting and

working on paperwork for the state and working on budgets and trying to analyze test scores to make them meaningful to the teachers and to the...and whatever. So those are the typical boring days. This is our second week of school so there's no typical beginning of the school year. Now I'm spending more time supporting teachers, right now new staff. Right now I'm doing...pulling on my Special Ed background. I have a little guy who is in one of our self-contained classrooms but he's struggling with the transition coming back to school and mornings aren't good for him and he's got a new teacher. And the principal in that school is on maternity leave. And the principal who is filling in was a little panicked. And so we met and talked about strategies for this little guy that, no, you know in first grade he's not ready for therapeutic day school. He's not hurt anybody. Everything's fine. It will be okay. We have a controversy going on right now related to curriculum materials that have been selected for students' optional use, optional reading. So we've been laughing and...on one hand...and cringing on the other because we're responding to one parent's concern.

We have only heard from one parent who has a concern about a book that was on the summer reading list. Kids take home a list of six or seven books that are optional. The kids give a synopsis of the book at school. They talk about them. And if you don't like any of those books you can read any other book in the whole wide world to choose from. And this one is as much young adult literature is -has controversial themes because it gives us the opportunity to support kids as they worry about these things.

Q: Can you tell the name of the book?

A: It's *Fat Kid Rules The World* by Kale Going. And the themes really are friendship, not giving up, perseverance. A student in there contemplates suicide. He's had a very tough time. His mom's died from cancer. His dad's an alcoholic. He's in an abusive home situation. And he is befriended by a homeless teen who is a gifted guitarist who asks this kid to join his band and play the drums. And it basically is about acceptance and you know it's a great story of Redemption. It's a wonderful story. And the parent that objects is objecting based on the proliferation of the "f" word. And it is in there and it...kids are in Brooklyn. And interestingly enough, but it's not really spoken out loud. It's in this kid's thoughts. That she's objecting to the normal sexual fantasies of teenagers. He's describing a person and saying no not this one, not the one with the large breasts you know the other one...physical features.

So you know things like that. This parent has you know not accepted that the fact that her child was not required to read the book and...She did not ask for the book to be banned from the library. I think she just asked for it to come off the summer reading list. However that has snowballed to some right-wing web sites...Concerned Women for America, the Illinois Family Network. I don't know which all...Save Libraries.org. And we have been getting interesting e-mails from basically all over the country and Canada.

Q: What would be an interesting e-mail?

A: Oh some that are saying...One was, you know, "If I knew where Osama Bin Laden was, I would turn him in but first I would tell him where your school was so that he could bomb it. Hopefully when there was no children...on the weekends when no children were present."

You know, "You're responsible for the moral degradation of children and the increase in rapes and murders and school shootings because children have read...because we have forced children to read this book."

Q: Have you been threatened or has anybody?

A: I have not personally been threatened. The junior high Principal has been threatened. You know "When someone comes and murders your family, it'll be because of how you taught them." Rather interesting. No one from the immediate community...No other parents in the community... There's an article in today's paper, we had a prepared statement to share with people who called anticipating...And we did end up sending a note home today you know saying that you know we didn't believe that the threat was really credible but that we did have, you know, that there was a police presence.

...It was on the book list for incoming 8th graders, so that would be 13 and 14 year-olds. They talk about these things. And some statistic that I had recently come across said three to five... Three out of five teenagers contemplate suicide at one point. So, um... yeah, it's kind of important to maybe say, yeah, there's a place to talk about this. It was... It's probably towards the young end of the age spectrum that the book might be appropriate for. And we did have a parent come and talk in support of the book. Her student had read it during 7th grade. He's a very capable student. And as a parent she also read the book and thought it was a perfect avenue to discuss some of these difficult situations.

In this example, we have many options for creating poetry to describe the content of the issues at

hand and the emotional meaning. Here is an example of poetry written by Jill Flansburg (2011), who read the transcript and created this poem:

Parents misconstrue
Parent Misconstrue
The teachers, the kids the book.
Narrow mindedness.
Poise under pressure
Never a typical day
But I really care...
Parents find fault
Intolerant of teachers
And the kids miss out.

You can see the power of the poem. It makes us think in new patterns and see something in the data that inspires poetic form. Poetry becomes a way to see possibility and hope in our work. Poetry allows us to say things that may not have been said or that makes us notice what exactly has been said. Poetry becomes a method of discovery and a powerful technique in one's toolkit for oral history. It is evocative and personal. Many poetry sites on the internet and many poetry blogs may even be used as a way to analyze, interpret, and disseminate data in digital formats. Poetry as data presentation is one meaningful strategy for the oral historian. This goes beyond the actual interview.

HOW MIGHT WE WORK IN A TRANSDISCIPLINARY WAY TO AUGMENT THE ORAL HISTORY INTERVIEW?

I like to think about ways to make oral history more accessible. Thinking about digital technologies, poetry, and social justice as contextual, it makes sense to think about what other disciplines might teach us. For years, researchers have been writing, thinking, and talking about triangulating data. We also have seen many writers discuss cross- and/or interdisciplinary approaches to research. For example, social sciences and health sciences may have researchers who team up to study obesity. Medical and educational researchers may team up to study AIDS education programs. But the question then becomes how deeply this collaboration might occur between and among disciplines? A new development has occurred recently in terms of thinking about transdisciplinarity.

Historically Jean Piaget most likely was the first to introduce transdisciplinarity around 1970. In the 1980s in Europe, interest continued in this area, and in 1987 the International Center for Transdisciplinary Research adopted the Charter of

Transdisciplinarity at its first world congress. See the Charter of Transdisciplinarity at <http://basarab.nicolescu.perso.sfr.fr/ciret/english/charten.htm>.

Basically this charter calls for conducting research with a holistic approach by crossing disciplines and going deeply into the union of the disciplines while designing research that is problem based. Currently many European research centers such as those in Switzerland, Germany, and France appreciate and use this approach. In many ways, it is like choreography for it shares many characteristics, and in choreography the artist uses at least two disciplines deeply imbedded in each other: dance and music. For example, the choreographer mentioned earlier, Merce Cunningham, worked with composer John Cage (1912–1992) on a lifetime of collaboration in dance and music performance. They were able to create deep meaning from this collaboration in performance, in critique, and in developing new projects.

For oral historians, transdisciplinarity can work effectively as oral historians may use two or three disciplines imbedded in each other. For example, in my current research I use education, history, and sociology to examine the oral histories of female leaders. In order to go beyond the transcripts of interviews, transdisciplinarity offers us much to work with in terms of research design, analysis, and interpretations (See Hirsch et al. 2008, Leavy, 2011). In addition, this approach is well suited to qualitative approaches in general history, and particularly in oral history. I see this as a steady progression toward a more integrated, unified, socially just, artistic, and rational approach to our work. In closing, let me ask that we think about going beyond the interview through the use of arts-based approaches to presenting data, that we consider through the use of the digital tools available how we contribute to social justice through our work, and that we appreciate the value of transdisciplinarity in our work.

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Focus Group Research: Retrospect and Prospect

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Abstract

This chapter is both historical and conceptual, first highlighting the origins, tensions, and continuities/discontinuities of focus group research, then arguing for how such research embodies three primary, related functions: inquiry, pedagogy, and political. The quasi-unique potentials or affordances of focus group work are explored, including mitigating the researcher's authority; disclosing the constitutive power of discourse; approximating the natural; filling in knowledge gaps and saturating understanding; drawing out complexity, nuance, and contradiction; disclosing eclipsed connections; and creating opportunities for political activism. Contemporary threats to focus group work are described, and new research frontiers are proposed, especially in relation to new information technologies. The chapter integrates historical, conceptual, and practical perspectives to fully explain the potentials of focus group research, with the goal of advancing a set of understandings about focus group work that attends to its relatively unique potentials for conducting qualitative inquiry across a wide range of topics and disciplinary contexts.

Key Words: dialogic interviewing, collective conversations, communitarian ethics, researcher positioning, focus group functions, focus groups, praxis-oriented research, innovative research practices, virtual worlds

Focus Group Research: A Conceptual-Historical Introduction

The use of focus group research extends back as far as early media effects studies at the Bureau of Applied Social Research at Columbia University in the 1940s, particularly the work of Robert Merton. Of particular interest in the post-World War II era was the study of mass-mediated propaganda. Several new methods emerged from the Bureau, including the focus group or “focused interview.” The focused interview had the virtue of expediency. As such, this interviewing strategy was rooted in positivist or post-positivist epistemologies, which assume that the Truth is “out there.” In many respects, the empirical material that emerged could be analyzed with the same tools used to analyze one-on-one interviews. Although taking place in

a focus group, the “unit of analysis” was still the individual.

In addition to highlighting the role of epistemology in research practice, this privileging of the individual had disciplinary implications as well. Specifically, by locating the truth in the individual, focus group research favored psychological approaches and explanations over sociological ones. Problems and explanations, here, tend not to be viewed in terms of class structure or gender inequalities or race and racism but in terms of the motivations, attitudes, and beliefs of individuals. In important ways, this one-on-one interview paradigm results in theory and research being underutilized.

With an eye toward broadening our understanding of the nature and functions of focus

groups in research, we question some of the assumptions of early focus group work, offer a conceptual and practical introduction, highlight the fissures in the tradition of research that has used focus groups, and postulate some of their yet untapped potentials. Indeed, focus groups can encompass a wide range of practices and overlapping purposes—pedagogical, political, traditionally empirical. In this chapter, we explore focus group research systematically, not as an extension or elaboration of interview work alone but as its own specific, structured mode of conducting research.

This is a practical, strategic decision. The techniques and tools one uses to collect one-on-one interview data cannot easily be imported into focus group settings in ways that mine the unique and rich potentials for knowledge generation, pedagogy, and political work that focus groups can afford. But the differences between individual interviews and focus group conversations extend beyond technique to important theoretical and conceptual distinctions, such as functions and definitions of self that are both rooted in and generated through the process. One-on-one interviews are predicated on an Enlightenment notion of the “self.” The Enlightenment self is a transcendent consciousness that functions unencumbered by social and material conditions and that is the source of all knowledge and the agent of all action. In contrast, from a more sociological or social constructionist perspective, the self is seen as produced in and through historical, social, and material practices. The self is not reduced to an a priori mind, a social formation, or a sign. Rather, the self is a particular configuration of discursive and material practices that is constantly constructing, deconstructing, and reconstructing itself and is always already social.

Madriz’s (1997) study of women’s fear of crime exemplifies how this way of thinking about the relations between the self and the social can play out in focus group work. Albeit in slightly different terms, she clearly treated the “group” as the unit of analysis and saw the self and the social as constituting each other. This allowed her to understand fear as a collective phenomenon—something to be understood and addressed as a social, not psychological, issue. A “sociology of fear” supplants the all-too-often evoked “psychology of fear.” The “reality” of fear is thus challenged as a normative concept. Once challenged, fear can be responded to differently and perhaps more productively, as research and its application can now be directed toward solutions.

Focus groups are especially fertile sites for empirical investigations of these new theoretical formulations of self. In particular, they give us opportunities to see whether and how “self,” “other,” and “context” seem indeed to be co-emergent phenomena, getting to the very heart of the social processes social theorists argue constitute reality. Our approach to focus groups embodies this emergent and ecumenical character. As such, it has particular consequences for how we think through and utilize data in focus group research.

Throughout this chapter, we try to develop a workable set of theoretical and practical distinctions that mark focus group work as quasi-unique and sociological. As noted, focus groups can be group interviews or collective conversations. Most are situated somewhere along that continuum. Key here is the degree to which groups are “managed” by the researcher or allowed to develop in more self-organizing ways. When they are allowed to be more free flowing, focus groups can mitigate or inhibit the authority of the researcher, allowing participants to “take over” or “own” the interview space. Although not a “natural” occurrence, focus groups allow researchers to create better approximations of natural interactions than do individual interviews or even observations. Finally, focus groups can allow for what we call “memory synergy” and “political synergy” among participants. These knowledge-generating affordances are, in many ways, unique to focus group work; they are extraordinarily important for understanding certain kinds of social phenomena.

We turn now to the earliest work on focus groups, teasing out some key continuities and discontinuities that still pepper the landscape of qualitative inquiry today.

The Idea of “Focus”

The researcher determines the “focus” in focus group research. In their most controlled forms, individual interviews imply a dyadic, even clinical, relationship between interviewer and interviewee. In their less controlled forms, the interviewer negotiates with the interviewee. Interview protocols are relatively open-ended, the interviewee is encouraged to introduce topics, and the conversation is allowed to move in unstructured directions.

In a seminal article published in 1946 in the *American Journal of Sociology*, Robert Merton and Patricia Kendall discussed the “focused interview” as an important new methodological innovation, allowing interviewers to gather specific information

from participants around delimited topics. Merton and Kendall envisioned the focused interview as a space tightly defined by the researcher. “Equipped in advance with a content analysis, the interviewer can readily distinguish the objective facts of the case from the subjective definitions of the situation” (p. 541) and thus tightly control the content and flow of group talk accordingly.

The authors went on to provide several practical examples of the focused interview. The examples draw largely from (then contemporary) World War II propaganda research, with a goal of eliciting responses free from the directive influence of the interviewer. That is, the interviewer set the stage and delimited the context. But the goal was to excavate specific, targeted responses from the interviewees. The interview was posited as an “informal listening-post or ‘clinic’ or ‘laboratory’”—not a “debating society or authoritarian arena in which the interviewer defines the situation” (Merton & Kendall, 1946, p. 547). One sees a concern here with “data contamination” or a “Heisenberg effect.” According to Merton, the focused interview should avoid these problems as much as possible—another indication of the positivist epistemological impulses that motivated this project.

In terms of method, the goal was to integrate quantitative and qualitative work. Using focused interviews, researchers could develop hypotheses that could later be systematically tested (Merton & Kendall, 1946, p. 557). Or, alternatively, the focused interview could be used to interpret material gathered from experimental studies (p. 557). Merton and Kendall were indeed pioneers, and the world of research methods would not catch up to them for more than five decades, with the emergence of “mixed methods” research (e.g., Cresswell, 2008; Teddlie & Tashakkori, 2008).

Indeed, their seminal 1946 article was groundbreaking, later becoming the book, *The Focused Interview* (Merton, 1990), a founding text for focus group research. Rereading the article and book today is instructive; these focused interviews were not concerned with group processes or collective understandings. The key was the “focus” of the interview. The number and kinds of participants involved were secondary. *The Focused Interview* does address the question of “the group”—but only briefly (in chapter 7). Even there, Merton is tentative, pointing to potential affordances of conducting the “focused interview” in a group. An obvious noted advantage is the diversity of responses. But, he adds a caveat: “Little enough is firmly known

about the systemic differences between the types of data provided by interviews with individuals and with groups. It is not at all certain that the private interview is uniformly preferable to the interview with groups. It may even develop, on further study of this problem, that the group interview is preferable to the private interview for certain types of problems” (pp. 135–136).

This last point is worth emphasizing. The pioneers of focus group work were not particularly concerned with the advantages of studying “the group.” Rather, they were concerned with the role of the interviewer and the “focus” of the interview—the ability to define a situation objectively and gather the responses of many individuals to that situation.

Yet a closer examination reveals an abiding positivist orientation that would remain the legacy of much of this work. According to Merton and Kendall (1946), focus groups could generate “hypotheses” that could later be “submitted to systematic tests.” Alternatively, focus groups could help “interpret previously ascertained experimental findings” (p. 557). In both cases, developing the “focus” in the “focused interview” allowed for commensurability and dialogue with experimental work. Merton provided researchers a functional, epistemological blueprint for getting at deep and irrefutable truths.

The Idea of the “Group”

The original meaning and use of the term “group” in “focus group” was highly specific. These groups had no identity outside of the research context. In fact, the term “focus group” only emerged during the 1960s in the field of marketing, probably emerging from the everyday practice of professional marketers (Lee, 2010). Its history is scarcely documented, embedded in the archives (if at all) of corporate sponsors. Lee also gestured toward the supposed cost-efficiency of focus group work. This would long be an attractive element of focus group work—but one with significant drawbacks as well.

As Lee made clear, focus groups were heavily utilized by marketers and other corporate types with commercial interests. The applied nature of their work appealed to researchers and practitioners in the fields of “social marketing” and health promotion. Here, as well, focus group work blurred the line between research and application. For example, if the goal of early cigarette studies was to understand attitudes toward smoking to perfect their advertising and sales, then we see the group treated as a “target market”—gauged for their attitudes, targeted for seemingly positive, social ends. Focus

group research would continue to flourish in these fields. Both areas are, of course, largely “applied”—that is, they define their goals and outcomes a priori. The use of focus groups in these fields is not dialogic or co-constituted by facilitators and participants. Instead, facilitators control topics and flows of talk.

From the early 1980s, focus group work continued to flourish but the interest in “the group” proliferated, especially in “audience” or “reception” research studies. The primary goal of these studies was to understand the complexities involved in how people understood and interpreted media texts. To accomplish this goal, researchers focused on group dynamics themselves because they believed that both the meanings constructed within groups of viewers and the viewers themselves were largely socially constructed.

In a groundbreaking study along these lines, David Morley (1980) attempted to chart the various responses of viewing groups from different social and economic classes to the popular television show *Nationwide*. Working from within a social constructionist framework (e.g., Berger & Luckmann, 1966), his use of focus groups was strategic: “The choice to work with groups rather than individuals . . . was made on the grounds that much individually based interview research is flawed by a focus on individuals as social atoms divorced from their social context” (p. 97). In contrast, for Morley and other scholars interested in audience reception practices, focus groups were invaluable because they afforded insights into how meanings are constructed collectively and in situ.

Morley and others would reconceptualize the construct of the “group” in focus group. While drawing on the tools of early propaganda and other research, Morley and others also broadened the scope and use of these tools, with the audience treated not as a “target market” but as a collective having its own particular autonomies and dynamic potentials. The goal of audience reception studies was to create situations that were as close as possible to how people actually interacted with fellow viewers of particular media. Underpinning this orientation is a social constructivist epistemology—one that views the reception process—both of individuals and of groups—as dynamic and emergent.

Another trajectory of work would also challenge narrow notions of “applied” social research—the literacy “study circles” of Brazilian pedagogue Paulo Freire. Conducted in poor, rural areas of Brazil in the 1970s, Freire organized collective discussions meant to elicit words fundamentally important to

the lives of illiterate adults. He called these “generative words.” These words were then used as starting points for literacy learning, deployed in the service of social and political activism. Importantly, this use of “focus groups” differs considerably from uses we have discussed so far. The topics for discussion come from the stakeholders themselves. The facilitators are nondirective. The truth is not considered to be “out there” to be discovered but a phenomenon that emerges through dialogue or collective conversation. This empirical material demands a different set of analytic tools that cannot be imported from one-on-one interviews. We return to this issue later.

So, the nature and functions of focus groups have proliferated in several directions at once. In some versions, the “focus” has been dictated tightly by the interviewer. In others, the focus has been constructed in dialogue among the participants. In some versions, the “group” has been irrelevant. In others, it has been constitutive. In some versions, the audience has been constructed as a “market segment.” In addition, focus groups have been used across considerably different domains of inquiry: corporate marketing, anthropology, sociology, media studies, health sciences, education, and many more. Focus group research has also moved across various epistemologies, from positivism and post-positivism to social constructivism and post-structuralism. Finally, it has been used by a variety of stakeholders, from the military to the corporate world to medicine to the academy. Given this complex and variegated inquiry landscape, the researcher is forced to fundamentally reimagine his or her role in focus group work.

Focus Groups and/in Applied Fields

There are many books on focus group research. These include *Doing Focus Groups* (Barbour, 2008), *Focus Groups in Social Research* (Bloor, Frankland, Thomas, & Robson, 2001), *Focus Groups: A Practical Guide for Applied Research* (Krueger & Casey, 2008), and others. These texts are largely “how-to” guides. Although useful, they typically lack historical and theoretical grounding. One goal of this chapter is to situate focus group inquiry within broader historical and theoretical lines. To accomplish this goal, we treat the use of focus groups as a methodological strategy with its own historical and theoretical specificity.

Focus groups have gained considerable purchase in applied fields. In most healthcare fields, their use has increased dramatically in recent decades. The idea that focus group research allows health practitioners

to gather critically productive information is echoed in journals such as *Journal of School Nursing*, *Nurse Researcher*, and *Qualitative Health Research*. In fact, *Nurse Researcher* devoted a special issue to focus groups in 2007. The issue's editorial introduction contextualized articles in the issue, calling focus groups "useful" but noting their limitations, especially their small sample size and the attendant dangers of generalization without adequate warrant (Parahoo, 2007). Echoing Merton and his colleagues, Parahoo also noted that focus groups may be used "to develop tools such as questionnaires or interview schedules, to clarify and explore the findings from other methods" (p. 5). A clear subtext of his breezy introduction is that focus groups are subordinate to more systematic modes of data collection. Nearly all the other articles in this issue also advocate this cautious approach to focus group work, noting its limited uses for gauging attitudes and beliefs.

Focus groups have also been widely used in counseling and counseling education (e.g., Kress & Shoffner, 2007). According to Nabors (2001), focus groups are an "effective method for examining stakeholders' perception of mental health programs for children and their families" (p. 243), assessing client needs, and developing programmatic interventions.

In addition, focus groups are used to research sensitive topics and vulnerable populations in counseling-type settings. Supplementing survey work, for example, Hopson and Steiker (2008) used focus groups to explore drug abuse prevention programs in alternative schools. Similarly, Nelson-Gardell (2001) used focus groups to explore survivors of childhood sexual abuse. And Briller et al. (2007/2008) used focus groups in a study of bereavement. Like most such studies, this one was largely concerned with implementing focus groups effectively and sensitively.

Marketing has primarily drawn on focus group research to determine the needs and desires of consumers and clients. More academic uses of focus group research in marketing have often involved comparing focus groups to other methods. York, Brannon, Roberts, Shanklin, and Howells (2009), for example, compared survey research to focus group research in studies designed to assess employee beliefs and practices around food safety. Other academic marketing researchers have sought to understand technical or pragmatic aspects of focus group work. Tuckel and Wood (2009), for example, investigated the ways respondents are (or are not) "cooperative" in focus group processes.

Focus groups have also been widely used in education research. Peters (2009), for example, demonstrated how focus groups were used to revise the master's level public administration curricula at Western Michigan University. Similarly, Hall, Williams, and Daniel (2010) used focus groups to assess the effectiveness of an after-school program for disadvantaged youth. Importantly, focus groups are central to the pedagogical interests of other applied fields—counselor education, nursing education, medical school education, business education, and the like.

This brief introduction shows that theory and research around focus groups continues to revisit many of the conceptual and practical issues raised by the earliest researchers to use focus groups. Despite their widespread use in many fields for many decades, focus group research remains undertheorized and underutilized. One goal of this chapter is to redress this situation. To do this, we focus on three interrelated key functions—pedagogical, political, and empirical. These dynamic relations, again, tend to occur more often with focus groups than with many other data collection strategies.

Multiple, Interrelated Functions of Focus Group Work

Focus groups enact at least three primary functions most of the time: inquiry (research), teaching and learning (pedagogy), and social activism (politics). Within any given project, these different functions of focus group work emerge and interact, with one function being dominant in each group.

Different insights about focus group work tend to flow from each of these three functions. The *pedagogical* function foregrounds the dialogic nature of focus group interactions, as well as the possibility for transformative encounters. The *political* function highlights the sources of collective support that occur around social and political issues. The *empirical* (or inquiry) function alerts us to the deep epistemological issues and concerns around "the research act," including the complex negotiations between "self" and "other" in inquiry. Here, we provide accounts of research endeavors in which one or another of these functions has been dominant.

The Pedagogical Function

The work of Paulo Freire in Brazil illustrates the pedagogical function especially well. Among other things, we like Freire's work because he worked *with* people and not *on* them, thus modeling an important praxis disposition for contemporary

educators and qualitative researchers (e.g., Barbour & Kitzinger, 1999). Additionally, although pedagogy was clearly the dominant function in Freire's work, inquiry always nourished pedagogy.

Freire's work was intensely practical, as well as deeply philosophical. His most famous book, *Pedagogy of the Oppressed* (1993), can be read as equal parts social theory, philosophy, and pedagogical method. Throughout *Pedagogy of the Oppressed*, Freire argued that the goal of education is to begin to name the world, part of which is to recognize that we are all "subjects" of our own lives and narratives, not "objects" in the stories of others. As particularly powerful ideological state apparatuses, schools play a big role in this process.

In this regard, Freire argued that most education is based on the "banking model." This implies an Enlightenment worldview, in which subject and object are a priori independent of each other and where subjects are objectified and thus dehumanized. In other words, "the teacher knows everything and the students know nothing" (Freire, 1993, p. 54). As problematic as it is, the banking model provides the epistemological foundation for most contemporary educational institutions and practices.

To supplant a "banking model" of education, Freire offered a model based on the elicitation of words that are fundamentally important in the lives of the subjects. He called these "generative words." The primary goal of these activities was help people use words to exercise power over the material and ideological conditions of their own lives. Thus, Freire's pedagogical programs were designed to raise people's critical consciousness (*conscientization*) and encourage them to engage in "praxis" or critical reflection linked to political action in the real world. He clearly underscored the fact that praxis is never easy and always involves power struggles, including violent ones.

The fact that Freire insisted that the unending process of emancipation must be a collective effort is far from trivial, especially because of its implications for focus group work. Freire believed that dialogue, fellowship, and solidarity are essential to human liberation and transformation, and only dialogue is capable of producing critical consciousness and praxis. Thus, all educational programs must be dialogic.

Within Freirean pedagogics, the development and use of "generative" words and phrases and the cultivation of conscientization are enacted in the context of locally situated "study circles" (or focus

groups). The goal for the educator within these study circles is to engage with people in their lived realities, producing and transforming them.

To illustrate this kind of problem-posing education rooted in people's lived realities and contradictions, Freire created what would now be called collaborative action research (CAR) or participatory action research (PAR) programs, including one designed around alcoholism. Because alcoholism was a serious problem in the city where Freire lived and worked, he and his research team showed assembled groups a photograph of a drunken man walking past three other men and asked them to talk about what was going on in the photograph. Alcoholism was "read" as a response to oppression and exploitation. This example of problem-posing pedagogy is quite different from (and we would argue much more effective than) more didactic approaches that would more likely involve "sermonizing" to people about their failings. Problem-posing education is proactive and designed to allow people themselves to identify and generate solutions to the problems they face.

In sum, focus group work has always been central to the kinds of radical pedagogics advocated and fought for by intellectual workers like Paulo Freire and his many followers (e.g., Henry Giroux, Joe Kincheloe, Jonathan Kozol, and Peter McLaren). The impulses that motivate focus groups in pedagogical domains have important implications for reimagining and using focus groups as resources for constructing "effective histories" (Foucault, 1984) within qualitative research endeavors.

The Political Function

Here, we offer descriptions and interpretations of focus groups in the service of radical political work designed within social justice agendas. In particular, we focus on how the consciousness raising groups (CRGs) of second- and third-wave feminism have been deployed to mobilize empowerment agendas and enact social change.

Whereas Freire's primary goal was to use literacy (albeit broadly defined) to mobilize oppressed groups to work against their oppression through praxis, the primary goal of the CRGs of second- and third-wave feminism was to build theory from the lived experiences of women that could work toward their own emancipation. In our discussion of CRGs, we draw heavily on Esther Madriz's retrospective analyses of second-wave feminist work and her own third-wave feminist work. In both, Madriz focused on political (and politicized)

uses of focus groups within qualitative inquiry. Importantly, as forms of collective testimony, focus group participation has often been empowering for women, especially women of color (Madriz, 2000, p. 843), for several reasons. Focus groups decenter the authority of the researcher, allowing women safe spaces in which to talk about their own lives and struggles. These groups also allow women to connect with each other collectively, share their own experiences, and “reclaim their humanity” in a nurturing context (p. 843). Very often, Madriz noted, women themselves take these groups over, reconceptualizing them in fundamental ways and with simple yet far-reaching political and practical consequences. Perhaps the most striking realization that emerges from examining some of the original texts of second-wave feminism are the explicitly self-conscious ways in which women used focus groups to deploy theory and thereby enact political change. Second-wave feminists persisted in building theory from the “standpoint” of women’s lived experiences, and their efforts eventually became a powerful social force in the struggle for equal rights.

In many respects, the CRGs of second-wave feminism helped set the agenda for the next generation of feminist activism. As Hester Eisenstein (1984) noted, these groups helped bring personal issues in women’s lives to the forefront of political discourse. Abortion, incest, sexual molestation, and domestic and physical abuse, for example, emerged from these groups as pressing social issues around which public policy and legislation had to be enacted. By finding out which issues were most pressing in women’s lives, CRGs were able to articulate what had previously been considered individual, psychological, and private matters to the agendas of local collectives and eventually to social and political agendas at regional and national levels.

Working within the movement(s) of third-wave feminism, Madriz herself used focus groups in powerful ways, some of which are evidenced in her 1997 book, *Nothing Bad Happens to Good Girls: Fear of Crime in Women’s Lives*. In this book, Madriz discussed all the ways in which the fear of crime works to produce an insidious form of social control on women’s lives.

With respect to research methods, Madriz called attention to the fact that it is hard to get people—women in particular—to talk about sensitive topics in uninhibited and honest ways in the context of oral or written surveys completed alone or in relation to a single social scientist interviewer. This general problem is further complicated by differences

in power relations between researchers and research participants. To work against the various alienating forces that seem inherent in survey research, Madriz used focus groups, noting that these groups provided a context in which women could support each other in discussing their experiences of and fears and concerns about crime. Focus groups afford women safer and more supportive contexts within which to explore their lived experiences with other women who will understand what they are saying intellectually, emotionally, and viscerally. This idea of safe and supportive spaces ushers in the importance of constituting groups in ways that mitigate alienation, create solidarity, and enhance community building.

In relation to this point, CRGs of second-wave feminism suffered from essentializing tendencies that ended up glossing different and even contradictory experiences of many women under the singular sign of “white middle-class women.” Acknowledging the need for variability in this regard, third-wave feminist researchers refracted and multiplied the “standpoints” from which testimonies and voices might flow. Although many held onto the post-positivist ideal of “building theory” from lived experience, researchers like Madriz pushed for theory that accounted more fully for the local, complex, and nuanced nature of lived experience. In the end, a primary goal of focus group activity within third-wave feminist research is not to offer prescriptive conclusions but to highlight the productive potentials (both oppressive and emancipatory) of particular social contexts (with their historically produced and durable power relations) within which such prescriptions typically unfold. In this regard, the work of Madriz is a synecdoche for third-wave feminist work more broadly conceived—particularly work conducted by women of color, such as Dorinne Kondo, Smadar Lavie, Ruth Behar, Aiwa Ong, and Lila Abu-Lughod.

Another key emphasis of focus groups within feminist traditions has been the discovery or production of *voice*. Because focus groups often result in the sharing of similar stories of everyday experience, struggle, rage, and the like, they often end up validating individual voices that had previously been negatively constructed within and through mainstream discourses. Because they foreground and exploit the power of testimony and voice, focus groups can create a critical mass of visible solidarity—a necessary first step toward social and political change.

Focus groups within feminist traditions have also mitigated the Western tendency to separate

thinking and feeling, thus opening up possibilities for reimagining knowledge as distributed, relational, embodied, and sensuous. Viewing knowledge in this light brings into view the relations between power and knowledge and thus insists that qualitative research is always already political—implicated in social critique and social change.

Finally, the break from second-wave to third-wave feminism called into question the monolithic sign of “woman” that characterized much of second-wave thinking and also highlighted the importance of creating focus groups that are relatively “homogeneous” because such groups are more likely to achieve the kind of solidarity and collective identity requisite for producing “effective histories” (Foucault, 1984).

Politicized forms of focus group work are perhaps best evidenced today in various PAR projects. In the United States, over the last several years, Michelle Fine has helped form various “research collectives” with youth at the CUNY Graduate Center. Fine, Roberts, and Torre (2004) brought together multiethnic groups of suburban and urban high schools for *Echoes of Brown*, a study of the legacies of *Brown v. Board of Education*. Originally a study of the so-called achievement gap, the framework soon shifted, due largely to the focus group-like sessions that drew the participants together. After discussion, the framework changed from one of “achievement gap”—a construct the youth felt put too much of the onus on themselves—to “opportunity gap.”

These research collectives opened up spaces for youth to challenge themselves and others in ongoing dialogue—a key affordance of focus group work. “As we moved through our work, youth were able to better understand material, or to move away from experiences that were too uncomfortable, or to make connections across seemingly different positions” (Torre & Fine, 2006, p. 276). Ultimately, these youth were able to carry out both empirical projects around “push out rates” and disciplinary practices in schools, as well as to produce powerful individual and collective testimonies about their own perspectives on and experiences of schooling, fifty years after *Brown*. See Cammarota and Fine (2008) for additional examples of this PAR principle at work.

We have highlighted here the political function of focus groups—the ways these groups allow participants to coalesce around key issues, co-producing knowledge and strategies for transcending their social, economic, and political circumstances. The interactions among participants in

consciousness-raising and other feminist groups, for example, are deeply pedagogical because knowledge is co-created in situated and dialogic ways. And, of course, the PAR work we have mentioned continues to extend the productive linkages among pedagogy, politics, and inquiry.

Michelle Fine’s notion of “strong objectivity” is helpful in this regard. According to Fine, we must work toward new forms of objectivity informed by the insights and advances of critical scholarship—particularly scholarship about the “situatedness” of all knowledge.

Such an approach helps researchers become more aware of potential “blind spots” that they may import, albeit often unwittingly, to their work. Such work can be most usefully done collectively with others. Such collectives seem to share the best impulses of focus group work because participants forge new kinds of understandings and try to avoid premature closure. These impulses allow us to interrogate how focus groups can foster approaches to political work that challenges the notion of “collectivity” itself.

The Inquiry Function

At least since the work of Merton and his colleagues, the inquiry function has been foregrounded by most scholars. Working within the Office of Radio Research at Columbia University, Merton and colleagues recruited groups of people to respond to radio programs designed to boost “morale” for the war effort (e.g., Merton, 1987, p. 552). Originally, the pair asked participants to push buttons to indicate their satisfaction or dissatisfaction with the content of the radio programs and used focus groups as forums for getting participants to explain why they responded as they did. Importantly, Merton’s use of focus groups strategies for data collection always remained secondary to (and less legitimate than) the various quantitative strategies also used.

In philosophy of science terms, the early use of focus groups as resources for conducting research was highly conservative in nature. This is not at all surprising when we consider that the work of Merton and others was funded by the military and that the goal of most of this work was to use knowledge about people’s beliefs and decision-making processes to develop increasingly effective forms of propaganda—inquiry in the service of politics.

Although both their goals and techniques merit harsh criticism (especially from progressive and radical camps), two key ideas from Merton’s work have become central to the legacy of using focus groups within qualitative research: (a) capturing people’s

responses in real time and space in the context of face-to-face interactions and (b) strategically generating interview prompts in situ based on important themes that are generated in these face-to-face interactions.

The kind of focus group research conducted by scholars such as Merton continued as a powerful force within corporate-sponsored market research, but all but disappeared within the field of sociology in the middle-part of the twentieth century. When it did reemerge in the 1980s, it was no longer wed to (or used in the service of) predominantly quantitative-oriented research.

Criticisms notwithstanding, audience analysis research has become interpretive and increasingly dialogic and emancipatory. Its primary goal is to understand the complexities of how people understand and interpret media texts. For example, Janice Radway used focus groups to great effect in her pioneering research on the reading practices of romance novel enthusiasts that resulted in her 1984 book, *Reading the Romance*. The research took place in and around a local bookstore, and Radway's participants included the storeowner and a group of forty-two women who frequented the store and were regular romance novel readers. Radway developed a mixed-method research design that included text analysis and focus group interviews, "formalizing" some of their ongoing social activities. She read all of the books that her participants read. She talked with many of them informally whenever they were at the bookstore together. And she conducted formal focus groups.

Among other things, Radway came to understand how important belonging to a reading group was for mitigating the stigma often associated with the practice of reading romance novels: "Because I knew beforehand that many women are afraid to admit their preference for romantic novels for fear of being scorned as illiterate or immoral, I suspected that the strength of numbers might make my informants less reluctant about discussing their obsession" (p. 252). She also learned that how she positioned herself within the reading groups was crucial. She noted, for example, that when she was gently encouraging and when she backgrounded her own involvement, "the conversation flowed more naturally as the participants disagreed among themselves, contradicted one another, and delightfully discovered that they still agreed about many things" (p. 48).

All of the various strategies that Radway deployed helped to generate kinds of data that are difficult, if not impossible, to generate through individual

interviews and even observations. Radway concluded her book with a call to praxis, noting that "it is absolutely essential that we who are committed to social change learn not to overlook this... legitimate form of protest... and to learn how best to encourage it and bring it to fruition" (p. 222)

If Radway's work began to outline the political, ethical, and praxis potentials of focus groups within qualitative inquiry, Patti Lather's work attempted to push the "limit conditions" of such work even further. In their book, *Troubling the Angels*, for example, Lather and Smithies (1997) explored the lives, experiences, and narratives of twenty-five women living with HIV/AIDS, through five years of focus group interviews conducted within different "support groups" in five major cities in Ohio. Lather and Smithies met and talked with their women participants at birthday parties and holiday get-togethers, hospital rooms and funerals, baby showers and picnics. In both "strategic" and "found" ways, more organized occasions for "collecting data" constantly blurred into the "practices of everyday life" (deCerteau, 1984). Yet Lather and Smithies were careful to work against the tendency to sentimentalize or romanticize their roles or their work. Their participants, for example, wanted to produce a collection of autobiographies or autoethnographies of "lived experience." Lather and Smithies were more interested in theorizing their participants' experiences and foregrounding the political (especially micropolitical) dimensions and effects of these experiences. According to Lather, these competing goals were constantly negotiated in focus groups. This pedagogic and political activity resulted in a book that embodies a productive tension between the two competing goals.

Although much of Lather and Smithies' book is devoted to troubling the waters of ethnographic representation, the experience of conducting fieldwork primarily through focus groups also troubled the waters of research practice. In this regard, Lather and Smithies integrated sociological, political, historical, therapeutic, and pedagogical practices and discourses into their work. One of the most interesting sections of the book for our purposes is one in which Lather and Smithies cultivate what they call a "methodology of getting lost": "Here we all get lost: the women, the researchers, the readers, the angels, in order to open up present frames of knowing to the possibilities of thinking differently" (Lather & Smithies, 1997, p. 52).

Although these reflections refer to the book itself rather than to the process of conducting the

research that led to it, they apply equally well to working with research participants in the field. For example, Lather and Smithies refused to position themselves as grand theorists and to interpret or explain the women's lives to them. Additionally, Lather and Smithies acknowledged their impositions and admitted that a different kind of book might have pleased their participants more. But such a book would have taken Lather and Smithies outside their own predilections and perhaps competencies as researchers.

More than most, Lather and Smithies's work offers us ways to think about research that transcends and transforms the potentials of using focus groups for revisioning epistemology, interrogating the relative purchase of both lived experience and theory, reimagining ethics within research practice, and enacting field work in ways that are more attuned to its spiritual, even sacred, dimensions.

Summary of Focus Group Functions

Focus group work has a long and chaotic history in various domains of research. Here, we have tried to offer another kind of approach—one of overlapping genealogies of functions. Each of these functions allows us to mine the unique potentials of focus group activity. Hopefully, our accounts of these three functional trajectories of focus group work showed how each primary function is unique and foregrounds certain potentials of focus group work and that all functions are almost always co-present and co-constitutive in most research projects. The pedagogical function of focus groups highlights the deeply dialogic and transformative nature of such work while showing us that such work has no guarantees. The political function shows us focus groups as deep sources of collective support around important social issues while alerting us to the dangers of naïve notions of collectivity. The inquiry function highlights the ways inquiry can open deep philosophical questions about “the research act” itself, including the relationship between “self” and “other.” Each of these functions provides unique insights about how we (and our research participants) can benefit from the legacy of focus group research. Each also offers a partially unique set of ideas, strategies, and practices that can be brought to bear on research today across a variety of domains.

Focus Groups from a Performative Perspective

Kenneth and Mary Gergen (2012) posed three important insights on performative social science.

First, they noted that social science research is concerned with communicating ideas to others. Second, they emphasized the performative nature of language. Echoing J. L. Austin (1962), they noted that language is never simply about communicating information. It is also about expressing, sustaining, and extending relations. Third, they claimed that performance itself “expands our scope and sensitivities as social scientists” (Gergen & Gergen, 2012, pp. 11–12). We look to extend this discussion here. Performance has recently emerged as a basic ontological but inherently contested concept. Such a notion of performance gives us nowhere to hide in our responsibilities for the work we do, forcing us to see the routine as potentially ambiguous, foreign, and contentious.

The performative turn has opened up powerful spaces for thinking about emergent methodologies that “explore new ways of thinking about and framing knowledge construction,” while remaining conscious of the links among epistemologies, methodologies, and the techniques used to carry out empirical work (Hesse-Biber & Leavy, 2006, pp. xi–xii). From this perspective, inquiry (especially qualitative inquiry) is no longer a discrete set of methods we deploy functionally to solve problems defined a priori. Instead, we must question the reification of particular methods that has marked the emergence of qualitative inquiry as a transdisciplinary field (Kamberelis & Dimitriadis, 2005).

Because the performative turn has decentered the research act, reimagining qualitative inquiry largely involves seeing it more as a matter of asking new questions that are not definitively answerable. In this regard, our orientation to focus group work encourages a new angle of vision on the politics of evidence. Mindful of the best impulses of the sociology of knowledge and the attendant co-implication of knowledge and power, a new politics of evidence, we believe, must attend to the specificity and autonomy of evidence in new ways. Recognizing that evidence never “speaks for itself,” a key task today is finding ways to challenge our thoughts and practices.

Because focus groups have some quasi-unique affordances compared to other data collection strategies, they are especially useful in disarticulating and rearticulating what we mean by evidence and the politics of evidence. Focus group work includes some very important and powerful affordances. Focus groups can (and often do) mitigate or inhibit the authority of the researcher, allowing participants to “take over” or “own” the interview

space. The leveling of power relations usually also allows researchers to explore group dynamics and the constitutive power of discourse in people's lives. Another affordance of focus group research is to draw out complexities, nuances, and contradictions with respect to whatever is being studied. The intensely social nature of focus groups tends to promote a kind of "memory synergy" among participants and bring forth the collective memory of particular social groups or formations (e.g., African Americans, wounded war veterans, former cult members). Finally, focus groups can (and often do) become democratic spaces for solidarity building and political effectivity. Because of these quasi-unique affordances, the use of focus group often allows researchers to generate richer and more complex and nuanced information, especially in relation to certain topics or domains of inquiry. With savvy, responsive facilitation, focus groups can draw out several information-gathering affordances in especially powerful ways.

Next, we unpack each of these affordances in greater detail and provide examples of some ways they have played out in actual practice.

Mitigating the Researcher's Authority

Focus groups can mitigate or inhibit the authority of the researcher, allowing participants to "take over" or "own" the interview space, which can result in richer, deeper understandings of whatever is being studied. A key challenge here is working against premature closure, to avoid succumbing to the temptations of weak evidence; with skillful, responsive, empathic facilitation, focus groups can go a long way toward democratizing interactional spaces, allowing participants a sense of safety and ownership of the activity and thus generating deep, rich, complex understandings of the issues under study.

We thus see "mitigating authority" and "generating deeper understandings" as twinned phenomena. Several studies have demonstrated the power of mitigating the researcher's authority in focus groups. We highlight one such example here—Marc Lamont Hill's (2009) *Beats, Rhymes, and Classroom Life*. We chose this study because it offers an example of group discussions that draw on the affordances of focus groups without being marked as such. In this study, Hill developed a "Hip Hop Lit" class in a relatively poor, urban Philadelphia high school, along with a teacher at the school, Mr. Columbo. Hill did not call the discussion sessions that happened in the class "focus groups." However, they clearly bear a strong family resemblance to

what we have described as focus groups throughout this chapter. The discussion groups conducted by Hill and Colombo represent an excellent example of studies that help us push both conceptual and pragmatic understandings of the potentials of focus group work.

Across several of the chapters in *Beats, Rhymes, and Classroom Life*, Hill documented the ways he discussed complex personal and social issues with his students through the lens of hip hop. In an early discussion of hip hop and "the real" with his students, Hill came face to face with some of his own presuppositions about hip hop. "My choices were reflective of a broader tendency within my HHBE [hip hop-based education] contexts documented in the research literature. Typically, HHBE educators choose texts that they deem politically, intellectually, or culturally sophisticated or relevant. While appropriate, such moves often lead to the development of curricula that respond to the interests, experiences, and generational orientation of the teacher rather than the student" (p. 39). Hill's insights about the need to back away from his own interests and preferences was largely afforded by the "performative" space he and Mr. Columbo constituted with these youth—a space in which notion of hip hop itself was open to contestation. Importantly, Hill (Marc) made himself vulnerable during these discussions as well.

During a discussion of fatherhood, for example, Hill revealed that he was soon going to be the father of a daughter: "I have a baby on the way right now that I didn't expect. Her mom is six months pregnant, and I'm really stressin' about it. I ain't worried about money or nothin' like that. It's just . . . I wasn't expecting this, and she and I not together and she [the mother] gotta be in my life forever" (p. 86). The students "co-signed" (to use Hill's term) his experiences, and a discussion started about the role of parenting and gender roles. Some of the students had had similar experiences and shared them: "I feel you. My baby moms be trippin'" (p. 86). Hill made himself "vulnerable" here and opened a safe space for his participants.

Interestingly, part of becoming vulnerable and mitigating his authority required that Hill negotiate a particular kind of role in the classroom—a role different from that of Mr. Columbo, who was a formal teacher in the school. Because of his official role as teacher, Mr. Columbo did not "self-disclose." The group noticed this and discussed why Mr. Columbo was so reticent in discussions, providing both racial and status explanations for his behavior. One young

man commented, “He just, y’ah mean, he can’t relate ‘cause he from, you know, a different culture so he don’t want to say nothin’.” A young woman responded, “Other people can’t relate too but they try... And he a *teacher*” (p. 89). Although Hill and Columbo worked out their roles in the classroom in different ways, each was aware of how mitigating and negotiating their authority made a difference in the work they did. In the democratized, interactional space of their classroom, students were increasingly willing to share personal issues, which, among other things, yielded data that were very rich indeed. Key here was the focus group affordance of negotiating one’s role in relation to authority and power relations.

Excavating the Lifeblood of Social Activity

The leveling of power relations between researchers and research participants usually also allows researchers to witness something close to “natural” group dynamics. In this, the power of discourse in people’s lives is revealed. At least since the Chicago School of Sociology emerged in the early part of the twentieth century, the constitutive power of talk has been widely acknowledged and exploited within the interpretive research community. The Chicago School included many of the twentieth century’s most brilliant sociologists and social theorists, including Robert Park, Ernest Burgess, William Foote Whyte, and Frederic Thrasher. Many Chicago School scholars were interested in understanding how “natural areas” such as the Jewish ghetto (Wirth, 1928), hobo jungles (Anderson, 1923), hangouts and gathering places (Whyte, 1943), and areas that housed gangs (Thrasher, 1927) came to be. All grounded their work in the everyday lives of the people they studied, and all found that patterns of talk and social interaction were central to what one might call the lifeblood of the social formations or communities in which their participants lived and worked.

Mitchell Duneier’s (1994) *Slim’s Table: Race, Respectability, and Masculinity* is a powerful example of the ways in which talk brings communities into being and is responsible for how they remain the same or change over time. Rooted in the ethos and worldview of the Chicago School scholars, Duneier studied the lives and stories told by a group of African-American (and some white) men who hung out together in a neighborhood eating establishment called Valois on the south side of Chicago in the late 1980s and early 1990s. Duneier, too, ate

and hung out at Valois, listening to conversations and stories, entering some conversations, and eventually conducting individual and group interviews with some of the cafeteria’s “regulars.” Although some of the “regulars” showcased in Duneier’s book were white and/or middle class, most were older black working-class men living in or near the neighborhood where Valois was located. Rooted in the African-American cultural tradition of “care” for others in conditions of violence and oppression, the bonds between and among these men were wrought from their commitments to compassion, loyalty, and personal integrity. Their moral compasses are unwavering; as they share all of their dreams, hopes, frustrations, and losses with each other, they reveal themselves to be men of extraordinary substance and character at a time when such attributes have become both scarce and undervalued. Were this not the case, they would not remain in the circle of “regulars.” Their talk and social interaction would not be sanctioned because it would contaminate the lifeblood of the group.

With respect to our purposes in this chapter, Duneier’s work is exemplary for a variety of reasons. It clearly demonstrates the blurry boundaries of what has traditionally been considered focus group work, thus allowing us to imagine the boundaries of focus groups in increasingly wider ways. Perhaps more than any other study with which we are familiar, Duneier’s work also demonstrates how the lifeblood of any group or community is constituted (both habitually and ritually) through everyday talk and social interaction. Finally, in the spirit of William Foote White’s insights about the lineaments of fieldwork, Duneier’s discussions of how he entered into conversations at Valois underscore the fundamental importance of building and sustaining relationships in the research process. Only when one is successful at doing this does one begin to yield data that truly reflect the rich and variegated fabric of the social life one is studying.

Approximating the Natural

Although not entirely “natural,” focus group activity can afford a closer approximation to natural interaction than do many other data collection strategies and activities. As noted earlier, our approach to focus group work is rooted in the performative turn in qualitative inquiry. From this perspective, the line between focus groups and everyday interaction becomes blurry. We highlight here another example of research that utilized focus group strategies in complex and expansive

ways—the *Echoes of Brown Project* that Michelle Fine headed up with several students and colleagues. The *Echoes of Brown Project* had its roots in the Educational Opportunity Gap Project (EOGP), which involved scores of youth in documenting and understanding the so-called achievement gap from *their* point of view. The project ran from 2001 to 2003 and involved a series of “research camps” where more than one-hundred youth from urban and suburban schools in New York and New Jersey met with researchers from the CUNY graduate center to “study youth perspectives on racial and class based (in)justice in schools and the nation” (Torre, Fine, et al., 2008, p. 28). The participants became familiar with a host of techniques, including developing and administering surveys, interviewing, and conducting focus groups. They also developed and conducted creative theater workshops to learn more about *Brown v. Board of Education* legislation and to create a performance “that brought together political history, personal experience, research, and knowledge gathered from generations living in the immediate and the long shadow of Brown” (p. 33). Also, there are segments that chronicle the development of several written texts and dance pieces. Although these workshops were ostensibly led by resident artists and scholars, much of the “pedagogy” that happened within them ended up being dialogic and democratic, with the students teaching as much as learning.

In the EOGP project, politically powerful spoken word poetry and dance emerged from focus group activities. Invoking insights from the performative turn in social science research, Fine and her colleagues shared their acute awareness that data never simply speak for themselves but must always be made relevant to audiences. Invoking the pioneering work of Mary Louise Pratt (1991), Fine referred to her focus group formations as “contact zones,” “social spaces where disparate cultures meet, clash, grapple with each other, often in highly asymmetrical relations of power” (Pratt, 1991, quoted in Torre, Fine, et al., 2008, p. 24). The projects Fine and her colleagues initiated did indeed bring together youth from different classes, races, and ethnicities. As one participant noted:

Participating in something like Echoes and the Arts and Social Justice Institute was the first time where I had to work as closely and as intensely as I did with people who were so different from me. The project brought youth from very different racial, economic, academic, and social backgrounds into

one space to be creative and to most importantly just be themselves. The comfort and safety that was established in the very beginning was instrumental in allowing for the work to get done and the performance to be shaped and constructed. (Torre et al., 2008, p. 24)

Thinking about these various speech events as “contact zones” allows us to see broad continuities in both form and function across them. And the insights that emerge from this process allow us to reconceptualize “the natural” as a diverse set of spaces and practices shot through with potential problems and possibilities.

Saturating Understanding

Focus groups are particularly useful for filling in gaps in understanding derived primarily from other methods. Although we have been critical of deploying focus groups as subordinate to other forms of data collection strategies (particularly in relation to quantitative data), they do pair nicely with other modes of data collection in certain research contexts. In particular, focus groups can be used strategically to saturate understandings of key issues disclosed in partial or understated ways during the research process. For example, Getnet Tizazu Fetene of Addis Ababa University in Ethiopia used focus groups to understand how young college students understood HIV/AIDS (see Fetene, 2009). As Fetene found, the source of the problem of HIV/AIDS in Ethiopia has been constructed primarily as a kind of “knowledge gap” (see Fetene & Dimitriadis, 2010, for a more in-depth discussion of this issue). For example, a government report framed the problem as one of understanding preventative measurements—the more, the better. “Measuring comprehensive knowledge of the respondents by taking those who knew all three preventive methods and with no misconceptions was found to be low (less than 20 percent), which is in agreement with UNAIDS reports. Comprehensive knowledge seems to increase along with increase in educational level” (Ministry of Health, 2004, p. 38).

Fetene wanted to understand the phenomena in more depth. In addition to administering a survey, he deployed several qualitative data collection strategies, including ethnographic observation and document analysis. With the assistance of a co-researcher, he conducted focus groups with ten young men and ten young women. Several new and provocative findings emerged from these groups related to contemporary understandings of HIV/AIDS.

Many of Fetene's participants reported that they were weary of being inundated with information about HIV/AIDS and that such information no longer had any educational or preventative effectivity:

Kaleb: HIV/AIDS is a subject that I have known since I was a kid. Let alone talking about it with my friends seriously, I don't even want it to be mentioned. I think this is the overall feeling here. Suppose one announces to talk about HIV even at a time when there are no exams, in a situation where students have nothing serious to do, nobody would bother to attend the talk. On the contrary, we would say, "What the hell is he talking about? If he wishes, let me give him a lecture on it! *Sira fetual!* ["Doesn't he have anything to do?"] Students feel they know every thing about AIDS.

Minyichel: He [Kaleb] is right. The subject of AIDS has become boring. If one says let us talk about AIDS, every one would say, "Fuck you!"

As Fetene demonstrated, the young people in his study were facing something like a perfect storm. They were saturated with information about HIV/AIDS, long-standing mores around sex and sexuality are changing, yet puritanical and patriarchal ideologies about sex prevail. All these social facts help to explain why knowledge about condoms and condom use does not always translate into practice.

In form at least, Fetene's focus groups were probably the most prototypic kinds we have discussed in this chapter. Like the less prototypic kinds we have discussed, however, they were extremely effective at surfacing kinds of information that would have less likely surfaced using other data collection strategies. For example, his participants did not respond well to discussing sensitive topics related to sex and HIV/AIDS in one-on-one interviews. Indeed, when the authority of the researcher was diluted and the discussions were "owned" by groups of peers, participants were both comfortable and forthcoming about sharing the abundance of information they had related to major social problems.

We return to this study later in discussing other affordances of focus group work.

Drawing Out Complexity, Nuance, and Contradiction

Another affordance of focus group research is to draw out complexities, nuances, and contradictions

with respect to whatever is being studied. The intensely social nature of focus group work often elicits subtexts and cognitive and emotional "break-downs," which, among other things, index sensitive issues and problems that research participants may have alluded to but not addressed directly.

Focus groups were used effectively to draw out complexity, nuance, and contradiction in Michelle Fine and Lois Weis's work with poor and working-class young adults in Buffalo, New York and Jersey City, New Jersey. This work resulted in *The Unknown City* (1998) and other publications (e.g., Weis, 2004). Fine and Weis conducted interviews and focus groups with African-American, Caucasian, and Puerto Rican men and women around issues of domestic abuse, the police, and schooling experiences. *The Unknown City* presents a kaleidoscopic view of how the general loss of public safety nets was understood differently by different groups. For example, Caucasian men tended not to implicate larger structural forces in explaining their social and economic circumstances. In contrast, African-American men provided sophisticated structural explanations for their circumstances.

Additionally, Caucasian women had different perspectives than African-American women on many social issues. Both African-American and Caucasian women discussed domestic violence. Discussions of domestic violence were more frequent among Caucasian women, yet they were reluctant to name white men as perpetrators, typically locating cause elsewhere (Weis, 2004, p. 41). In contrast, African-American women held black men responsible for domestic violence. Weis noted, "It is striking that White women are reluctant to name domestic violence as a problem in the community, although obviously it is, whereas Black women speak openly and directly about violence in their homes" (Weis, 2004, p. 41).

The role of focus groups was crucial for unpacking these differences and the reasons behind them. Caucasian women tended to understand and explain their experiences in highly personal, psychological terms—as secrets not to be shared with others (or at least not with many others). They did not like to discuss them in focus groups. For the Caucasian women, there was a clear disjuncture between the individual interviews and the focus groups. In the former, abuse was shared as an individual, private problem. In the latter, abuse was avoided as a whole.

In contrast, in both individual interviews and focus groups, African-American women tended to talk more publically, and their stories included more

structural, sociological explanations. “Unlike White women, African American women spoke in focus group as well as in individual interviews, where they shared experiences of pain and suffering as well as strength and hope. They [told] and [retold] stories of abuse to one another, with sympathetic nods all around” (p. 45)

More generally, Weis found that domestic violence is more likely to spill out into the public domain in African-American communities than in white ones. Domestic abuse is experienced publically and addressed publically. Coming to understand both the experiences of domestic violence among African-American and White women happened largely as a function of the focus group component of Fine and Weis’s work. Their focus groups became key public sites where complexities, nuances, and contradictions related to ostensibly similar experiences could be brought to light and understood within and across racial lines. They were also crucial for understanding the ways that men of different races understood and explained the reasons for their social and economic circumstances. Thus, the focus groups in this study became staging grounds for understanding how social phenomena such as poverty and domestic violence are experienced, understood, and explained in different ways for different groups. These are, again, unique affordances of focus groups.

Disclosing Shadowy Connections

We use the somewhat unusual term *disclosing* intentionally here. It is a term coined by Martin Heidegger (e.g., 1962, 1971, 1993) to talk about how we engage in genuine modes of being and saying to “bring forth” the essence or organization of experiences and their contexts. Such disclosing activity has become particularly relevant in the complexly connected, globalized environments in which most of us now conduct research. Responding to this social fact, George Marcus’s (1998) insights about postmodern, multisited ethnographies are especially relevant here. In these ethnographies, relevant comparative dimensions develop as a function of the fractured, discontinuous plane of movement and discovery among sites. As the researcher maps an object of study, she often finds herself needing to posit logics of relationship, translation, and association among several real or virtual sites. She must attend to comparisons that emerge from putting questions to an emergent domain of inquiry or object of study whose lineaments are not known beforehand. She recognizes that most objects of

study are mobile and multiply situated. She draws lines of connection in the process of research that have previously been thought to be (or have been conceptually kept) “worlds apart.” There is, then, an inherent metaphoric character to the research process. The global is collapsed into and made an integral part of parallel, related local situations and flows (of culture, ethnicities, economic capital, etc.—see Appadurai, 1990). Interpretive accounts are thus created in a landscape for which there is not yet a map—an “accepted” theoretical or descriptive model. Researchers and their participants contribute to shaping the objects they study in the absence of reliable models of macroprocesses for contextualizing referents of research such as “the world system,” “capitalism,” “the state,” “the nation,” and the like.

In many ways, focus group work often involves and integrates Heidegger’s disclosing activity and Marcus’s postmodern ethnographic strategies in powerful albeit often tacit ways. Emily Martin’s (1995) work, which is reported in the book, *Flexible Bodies: Tracking Immunity in American Culture from the Days of Polio to the Age of AIDS*, is an exceptional example of how research can involve such an integration. She began this work interested in how the body’s immune system was constructed within multiple intersecting discourses: the media, the scientific community, traditional and nontraditional medical practitioners, the public imagination, and the like and also recognized its mimicry of social and political structures.

This led Martin to design a study that would allow her to understand this connection (and other related connections) more fully. Martin and her colleagues engaged in many kinds of data collection. To ensure that they crisscrossed diverse cultural terrains, they entered the social worlds of their participants—living in many different kinds of neighborhoods in Baltimore, Maryland, “joining neighborhood associations, attending community meetings, block parties, and festivals, and volunteering to work on neighborhood projects” (p. 9). Additionally, these researchers conducted open-ended conversational interviews with individuals, pairs, and small groups of people. Finally, in what we might call postmodern focus group practice, they developed qualitative data analysis software that—at least metaphorically—allowed the content of each conversational interview and focus group to interact with the content of all others. In other words, all transcripts—whether from interviews with individuals, pairs, or small groups were treated analytically as

a “collectively produced text, a kind of encyclopedia of what a diverse population thinks is sayable, imaginable, or thinkable about health, illness, the body, and society. . . . The open-ended nature of the conversations allowed issues and ways of thinking that we could not anticipate, to emerge and be heard. . . . Both [researcher and research participants] explored issues of mutual concern and interest interpretively. . . . The range of community settings in which we worked ensured that they [the participants] would make up a broad cross-section of the society” (p. 10). Their context-sensitive and very inventive research design and data collection and analysis strategies allowed these researchers to see, from different angles, aspects of how health or illness was constructed in many different realms of life and work.

From her many and varied findings, Martin was able to develop an argument for the emergence of a post-Darwinian public imagination in the United States. Participants from across diverse social contexts, cultures, and occupational settings consistently constructed health and illness in terms of a kind of social-cultural-racial survival of the fittest. For example, many people made “comparative estimates about the quality of different people’s immune systems” (p. 240) as a function of race or social class. In the end, Martin argued that “what is at stake in our understanding of ‘health’ are the broadest issues of the survival and death of the social order itself” (p. 240).

Disclosing connections thusly constitutes a powerful way to suture sites of cultural production previously unconnected and to create empirically grounded new accounts of intersecting social landscapes and their effects. Additionally, Martin’s individual and collective conversations, along with her prescient reflections upon these conversations, surfaced synergistic linkages among inquiry, politics, and pedagogy. As we have argued throughout, these linkages are always latent within focus group work, and they often end up disclosing complexities, nuances, and contradictions embodied in “lived experience.”

Prompting Solidarity and Political Activism

In an age when spaces for democratic interactions and communally enacted social justice agendas are becoming increasingly eclipsed and atomized, focus groups can become transformative democratic spaces for solidarity building and political effectivity. Perhaps the clearest example of focus groups and their role in politically charged work is through PAR and CAR. A particularly good example of this kind

of research is represented in Eve Tuck’s (2008) *Urban Youth and School Pushout: Gateways, Get-Aways, and the GED*. According to Tuck, “[r]ather than a set of methods, PAR is best described as an ethic, as a set of beliefs about knowledge, where it comes from, and how knowledge is validated and strengthened” (2012, p. 4). Participatory action research typically involves participants in the entire research process—from the definition of the problem through the research itself to the dissemination of results. Perhaps not surprisingly, focus groups have been used in many PAR projects. Again, Tuck’s study is a good example here.

In this study, Tuck examined the GED degree and what it means to the young people who flock to it year after year as an alternative to a traditional high school degree. Among other things, she highlighted the inexhaustible search for “dignity” that pushes many young people out of high schools and into the Byzantine world of this testing apparatus. To frame her work conceptually, Tuck drew together two contemporary bodies of theory and research with obvious political valences—the postcolonial work of indigenous scholars and the post-structural work of Deleuze and Guattari (e.g., 1987). Using the postcolonial construct of “repatriation” allowed Tuck to see how and why her participants were so bent on carving out some sense of “dignity.” Using Deleuze and Guattari’s insights about the fundamental importance of “desire” in all human activity, Tuck was able to see and explain how “desire” was the key motivational mechanism operating in the lives of the youth with whom she worked.

Of particular interest to us here is how Tuck used focus groups in inventive ways. A fundamental part of Tuck’s work was to involve her participants in “mapping” projects. Deleuze and Guattari (1987) make an important distinction between *tracing* and *mapping*. A tracing is a copy and operates according to “genetic” principles of reproduction based on an a priori deep structure and a faith in the discovery and representation of that structure. Tracings are based on phenomenological experience that is assumed to be essential, stable, and universal. Defined thus, the findings from most research projects are tracings. Deleuze and Guattari use psychoanalysis as an example of a historically powerful regime of truth within which tracings are always at work—forever recreating Oedipus. In contrast to tracings, maps are open systems—contingent, unpredictable, and productive—attending both to the actual and the possible.

As such, maps exceed both individual and collective experiences of what seems “naturally” real. Deleuze and Guattari suggest that after constructing maps, one may then place more apparently stable tracings back onto them, interrogating breaks and fissures where one finds them. Ultimately, a map produces an organization of reality rather than reproducing some prior description or theorization of it.

In one mapping activity, Tuck worked with her participants to create “problem tree” maps. This activity was designed to explore and document the ways young people understood complex phenomena, like the GED, in their lives. They represented their emerging knowledge in terms of roots and branches. Their “problem trees” were ways for young people to think through three levels of analysis in looking at a problem—the leaves or the “everyday symptoms of the problem,” the trunk or the “common beliefs and assumptions that support the leaves,” and the roots or the “ideologies that structure the whole tree” (2008, p. 49).

Importantly, these connections and the map that produced them were collectively generated in focus groups, and Tuck attributed much of the success of her work to the intellectual and political synergy they afford, even demand. Tuck’s PAR work, then, is a telling example of how PAR and CAR—which pivot on political synergy becoming political activism—often depend on focus group activity for their success.

Summary of Focus Groups’ Quasi-Unique Affordances

In this section, we focused on the key affordances of focus group work. These include mitigating the researcher’s authority and generating deeper understandings; disclosing the constitutive power of discourse and the lifeblood of social activity; approximating the natural; filling in knowledge gaps and saturating understanding; drawing out complexity, nuance, and contradiction; disclosing eclipsed or invisible connections; and creating opportunities for solidarity building and political action. Taken together, these affordances highlight many of the reasons why the specificities and autonomies of focus group work allow researchers to excavate information from participants that they would never be able to excavate using other data collection strategies.

We would like to offer some thoughts on these affordances and the relations among them. First, the affordances we discussed can and do overlap with

each other. For example, mitigating the researcher’s authority often helps to generate deeper understandings that can and do draw out complexity, nuance, and contradiction. Second, these affordances underscore the expansive and ecumenical definition of focus groups that we have tried to develop throughout this chapter. We have emphasized neither the form of focus groups nor the procedures for conducting them; instead, we have emphasized their potentials—what they afford in terms of unearthing rich datasets that most other data collection strategies do not afford (or afford as well). This shift in emphasis has allowed us to think through studies that have not been marked specifically as “focus group” research as traditionally defined. Third, emphasizing affordances has allowed us to highlight the inherent multifunctionality of focus groups—the ways they allow for outcomes that are simultaneously pedagogical, political, and research-oriented, as well as how lines connecting these three functions are blurred at best in actual practice. For example, mitigating the authority of the focus group facilitator or drawing out nuance, complexity, and contradiction are all very much a part of research, pedagogy, and political activism. Finally, using the word “affordance” itself is a matter of hedging our claims. None of these “affordances” are “inherent” to a particular research strategy or approach. Conceivably, for example, some of these affordances could play out using one-on-one interviews or even surveys. Still, based on our review of the literature and our own research experience, we are confident that the affordances we have discussed emerge much more often and in more powerful ways in focus groups compared to most (perhaps all) other modes of data collection.

Contemporary Dilemmas and Horizons of Focus Group Research

We have attempted in this chapter to develop a more expansive understanding of the nature, functions, and affordances of focus groups and focus group work than currently exists in the extensive literature on this general topic. Although this chapter has been primarily conceptual, we reject the notion that conceptual work can easily be distinguished from the gritty practicalities of research in the field. Because the two are always of a piece, we have interleaved practical advice for researchers and real-world examples throughout. Importantly, this material has not been added in an ad hoc fashion. Rather, we have tried to demonstrate how and to explain why the conceptual and the practical are

always inextricably intertwined. We extend this impulse to pair the conceptual and practical in this final section, as we look at contemporary threats to—and future possibilities for—focus group work.

Key here are the analytic categories particular to this work. As we noted throughout, focus group work performs the three primary functions of inquiry, pedagogy, and political action. Focus group work also has some quasi-unique affordances, which we unpacked in considerable detail. Because of the quasi-unique affordances, particular rich, complex, and nuanced kinds of data often emerge from focus groups that seldom emerge from other data collection strategies, including observations and one-on-one interviews. Additionally, particular understandings of self are drawn out in focus groups work that are different from those drawn out in individual interviews—selves-in-dialogue, social selves, selves-in-community.

In what follows, we deploy these ideas to animate discussions about pressing contemporary issues in qualitative inquiry (generally) and focus group work (specifically). These include contemporary threats to focus group work and strategic responses to these threats, research ethics and the public–private split, and problems and possibilities for focus group work in Web 2.0.

Contemporary Threats to Focus Group Work

Institutional review boards (IRBs) are university and college committees that oversee the protection of all “subjects” participating in research by university personnel. These committees must ensure that all such research is compliant with the principles and policies of the federal government (more on this later). Although primarily targeted toward research funded by federal grants, most universities and colleges require all faculty members and students to comply with their guidelines and principles. Recently, some argue that universities have begun to use these policies to protect against and derail potential lawsuits. (e.g., Christians, 2011, p. 67).

Interestingly, many professional organizations had already developed codes of ethics similar to those of IRBs during the twentieth century. The central role of the US federal government in research regulation only became pronounced in 1978, with the publication of the *Belmont Report*, which outlined ethical principles and guidelines for the protection of human subjects of research. This report came in response to past abuses of federally funded research.

The *Belmont Report* focused on biomedical and behavioral research. In time, though, its principles were brought to bear on all natural and social sciences, and its influence persists. To this day, IRBs are often made up of medical and behavioral scientists who, for the most part, conduct experimental research and know little about the assumptions, practices, and purposes of qualitative research.

One central concern IRBs often have with focus group research is insuring “anonymity” or protecting the rights of participants to be anonymous in formal or informal public presentations of research. Using pseudonyms and eliminating all identity markers in datasets are the typical ways the identities of research subjects are protected. The very public nature of focus groups problematizes the issue of anonymity. By their very nature, focus groups generate public data, and the facts that focus groups are social and socially intense is a primary reason for their unique power in getting at the *hows* and *whys* of whatever is being studied. This means that the most appropriate unit of analysis for focus group work is the collective. This conceptual/operational shift raises some practical concerns about safeguarding the anonymity of individuals within groups. Some examples will help here.

Author G. D. has served as major advisor and committee member for doctoral students who used focus groups as a primary tool for collecting empirical material. We already discussed the work of one of his students, Getnet Tizazu Fetene, who studied attitudes toward HIV/AIDS among college-aged youth in Ethiopia. This project was flagged by the university IRB as a potentially “high-risk” endeavor, and the question of anonymity remained central among the IRB’s concerns. Although Fetene’s and other students’ proposals all required strict confidentiality among participants, there was no real way to guarantee this. As a result, some members of the IRB suggested doing large numbers of one-on-one interviews instead of conducting focus groups.

What was really at issue became clearer when G. D. talked with the IRB. Unfamiliar with focus group work, some members echoed vestigial elements from the history of focus group research; the earliest proponents of focus group work stressed that “focus” could allow for a larger number of people to be interviewed simultaneously. That is, focus groups were an efficient way to conduct research. Because the IRB members who reviewed Fetene’s proposal did not understand the logics, functions, and affordances of focus group work, they flagged their use as a problem with the research design.

On behalf of Fetene, G. D. met with the IRB and provided historical and conceptual information about focus groups relevant to their concerns—including the fact that participants often feel more comfortable talking about sensitive topics in peer groups. He also explained that some kinds of information are more likely to be shared in focus group discussions that would almost never be shared in one-on-one interviews. In doing so, he used language and concepts that were familiar to typical IRB members. The IRB found his arguments compelling and approved the proposals.

The validity of G. D.'s arguments was clearly borne out in Fetene's study. Participants supported and responded to each other in very productive ways in their focus group conversations. Finally, Fetene's study brought into high relief the fact that the *collective* is the most appropriate unit of analysis for focus group work, as well as the fact that anonymity is a problematic construct.

Indeed, participants in Fetene's study formed a contingent, collective identity—challenging the notion that they did not “know” about HIV/AIDS. They constantly built on and extended each other's knowledge and experiences and lodged a far-reaching group critique of HIV/AIDS education programs in Ethiopia. They took ownership of discussions and used them for their own purposes. It seems highly unlikely that similar data would have emerged had Fetene conducted many one-on-one interviews.

To put it differently, G. D. had to argue for the practical and theoretical specificity of focus group work and make this explicit to IRB members. What is most important for our purposes here is the fact that getting approval for this research required educating powerful administrators about the benefits of a data collection strategy about which they had almost no knowledge. They knew nothing about the history of using focus groups in research, and they had little familiarity with the method's unique functions and affordances.

Our discussion of this experience with this IRB is worth underscoring. As is well documented, IRBs can be problematic gatekeepers for qualitative researchers because, by and large, they still operate from within a positivist epistemological orientation and evaluate all research against the standard of the medical model. Moreover, ethically grounded in the *Belmont Report*, they are fundamentally concerned with assessing the tensions and tradeoffs of means–end/risk–reward ratios. In the current political climate—perhaps more than ever

before—researchers need to educate IRBs about how potential benefits of unfamiliar data collection strategies outweigh their potential risks.

Research Ethics in the Twenty-First Century

An even more vexing conceptual issue is indexed by G. D.'s experience with the IRB at his institution. IRBs function to protect individual “subjects.” However, one argument we have made throughout this chapter is that the most appropriate unit of analysis for much qualitative research (and especially focus group research) is the group. In many respects, this shift provides a challenge to how guidelines for the ethical conduct of research are constructed. So, it would be useful to step back a bit here and talk about the philosophical and institutional foundations upon which IRBs rest.

As Cliff Christians (2011) has argued, the history of ethical deliberations about research in the West has been grounded in the Enlightenment tradition. Within this tradition, ethical guidelines are generated outside of particular communities and operationalized as a series of disconnected rules. The individual remains the unit of analysis here, marginalizing the role and importance of community. Similarly, the means–ends ratios are created to balance potential risks and benefits to individuals (and, ultimately, the public). These are logics that Christians and others see as increasingly unable to address contemporary ethical challenges. Even more troubling is the fact that IRBs currently function more to protect institutions from lawsuits than to protect individuals from physical or psychological harm (e.g., Christians, 2011; Denzin & Lincoln, 2005, 2011).

Christians (2011) also highlighted the particular limitations of current IRB policies and practices for thinking about the privacy of the individual. “Codes and ethics,” he noted, “insist on safeguards to protect people's identities and those of the research locations” (p. 66). Such safeguards are built on the assumption of the autonomous self, a problematic legacy of Enlightenment thinking, in which the “self became essential to the construction of a unique personhood” (p. 66). This conception of the self precludes other more social, communal, and democratic conceptions of the self and has proven difficult to sustain today. “Despite the signature status of privacy protection, watertight confidentiality has proven to be impossible. . . . Encoding privacy protection is meaningless when there is no distinction between the public and private that has consensus any longer” (p. 66).

In the face of this dilemma, Christians has proposed another form of ethics—one that “presumes the community is ontologically and axiologically prior to people. . . . We are born into a sociological universe where values, moral commitments, and existential meanings are negotiated dialogically” (p. 70). This form of ethics is ground zero for focus groups research as described in this chapter. And the most appropriate unit of analysis for such an ethics is the group. Thus, this epistemological orientation opens up possibilities for emergent group norms becoming the basis for new forms of ethics.

Christians’s ruminations about the ethics of research echoes a recent, powerful trajectory of thought in cultural historical activity theory built on the work of Mikhail Bakhtin. Bakhtin has long been appropriated by social science scholars, primarily in considering the dialogic nature of discourse. Indeed, his theories of the “utterance,” “heteroglossia,” “social languages,” and “speech genres” have been widely discussed and applied. His dialogic theories of the self received much less attention, but are particularly relevant here.

According to Bakhtin (1990, 1993), dialogue is fundamentally a matter of answerability or being ethically responsive within social relationships, and individual selves develop in response to and affiliations with collectives through relationships of care, empathy, and ethical responsibility. How individual selves appropriate socially shared texts and practices—the dimension of Bakhtin’s work most commonly discussed—always and only happens within meaningful engagements with specific others. Any philosophical anthropology, Bakhtin argued, must pivot on the real histories of real individuals engaged in relations of “answerability,” wherein each individual “owns” her responses to others and “intones” them with both her own meanings and those compelled by the other in dialogue. This claim was predicated on his insistence that the other is essential to the formation of the self—the individual’s “absolute need for the other, for the other’s seeing, remembering, gathering, and unifying self-activity” (1990, pp. 35–36). And for Bakhtin, this need has affective, valuational, and cognitive dimensions. How the self develops in and through its relationships with other selves always involves care, compassion, mutual responsibility, and love.

Crucial for our purposes here is that Bakhtin insisted that such a practical philosophical anthropology could never be adequately constructed in relation to any form of Enlightenment ethics. Bakhtin saw chains of caring and ethical answerability as

fundamental to social life and social justice. Without attention to its emotional-volitional dimensions, human interaction becomes susceptible to rationalist objectification. It loses what makes it qualitatively different from the more objective, determinate kinds of relations that constitute the natural world. Only within relationships of answerability can individuals (with others) embrace or resist the historical or cultural realities in which they find themselves. Reason is most rational when one is morally and ethically answerable to oneself and others. “The actually performed act in its undivided wholeness is more than rational—it is *answerable*. Rationality is but a moment of answerability. . . like the glimmer of a lamp before the sun” (1993, p. 29).

Living a meaningful, ethically responsible life, then, means living responsively with others, which is largely a matter of paying attention and being willing to be moved to action by the particulars of others’ actions, feelings, thoughts, and evaluative responses. “Life can be consciously comprehended only in concrete answerability. . . . A life that has fallen away from answerability cannot have a philosophy; it is, in its very principle, fortuitous and incapable of being rooted” (Bakhtin, 1993, p. 56). From the point of view of Bakhtin’s dialogic philosophical anthropology—and the theory and research it has spawned in the human sciences—conceptions of ethics that motivate the policies and practices of IRBs seem woefully inadequate indeed. It is high time to develop new conceptions—ones that take seriously the ways in which the self is always already social.

Like the work of Christians and recent invocations of Bakhtin’s philosophical anthropology within social scientific theories of the self, focus groups and focus group research have motivated the need for rethinking the constructs of “public” and “private,” as well as the consequences of operating with particular versions of such constructs. This point is worth underscoring. Politics has traditionally assumed a split between private and public spheres. The public sphere has been assumed the realm of “official politics,” where one leaves one’s private interests, assumptions, and biases behind. The “public voice” in politics is logical and designed for collective persuasion (e.g., Levine, 2008). Drawing on Amy Gutmann and Dennis Thompson’s (2004) work, Levine argued that “[w]hen in the public sphere, one must advance arguments that any rational person can accept. The public figure is an ethical and rational legislator, addressing an assembly of peers on matters of public concern” (p. 121).

Focus groups—as we have (re)defined them in this chapter—are extraordinarily fertile sites for rethinking this public–private split. Paradoxically, they are public spheres of potential collective action, but they do not ask us to leave behind the personal. In fact, focus groups are spaces where the personal can (and often does) become political. Like the various social movements discussed earlier, focus groups challenge normative notions of ethics grounded in Enlightenment notions of the self and the relations between the self and the social. As such, focus groups can help us reimagine aspects of the *Belmont Report* principles using the group (rather than the individual) as an organizing trope.

We would like to make one final point about the question of anonymity in research. Institutional review boards have traditionally been concerned about protecting the identities of individuals. We find it anomalous—and telling—that the participants in all of our focus group studies have often wanted their identities made public. In short, for both individuals and groups, anonymity is often low on their list of concerns—trumped by the desire for and prestige of public recognition. We might speculate why this is the case, invoking constructs like the conversationalization of public discourse (e.g., Fairclough, 1995) or new media forms such as talk shows, Facebook, Twitter, and so on. These speculations notwithstanding, a reconfigured approach to research ethics would put the problems and concerns we have raised here more squarely on the table.

When Focus Groups Go Virtual

The modulation of the public and the private is at the heart of emerging Web 2.0 technologies. As such, we can reflect on some of the issues being raised about internet research through the conceptual tools of focus groups as discussed throughout this chapter. As Marilyn Lichtman (2011) noted in *Qualitative Research in Education: A User's Guide*, “as the Internet becomes more widely available and as high speed connections link many people to the web and potentially to each other, conducting focus groups online offers a new alternative to the traditional type of focus group setting. ... I believe there is great potential for online focus groups. It is too early to say what methodological issues may arise” (p. 159).

The nature and effects of differences between asynchronous and synchronous discussions is an issue that has been addressed by many scholars. Among other things, these scholars have noted that

asynchronous discussions are akin to listservs, blogs, or e-mail discussion threads and that synchronous discussions are more like instant messaging (IM) or other chat group formats, in which conversations unfold in real time. In this regard, Bruggen and Williams (2009) emphasized that “the boom in online marketing research” is one of the “fastest growing” research segments in the field. The authors highlighted the advantages of online work, including “shorter project lead times, shorter field times, greater access to busy professionals, and international reach” (p. 363–364). However, in the same journal issue, Tuckel and Wood (2009) sounded a cautionary alarm, suggesting that “the visual anonymity provided by computer-mediated communication (CMC) may lead to deindividuation” and the cultivation of “anti-normative behavior.” They went on to say that CMC group members may “feel freer to find fault with others’ ideas, leading to more disagreement and criticism.” At the same time, “the visual anonymity provided by CMC can lead to lowered self-awareness (as others cannot see you) and heightened private self-awareness (as one can reflect on one’s own thoughts and how to type them), leading to increased self-disclosure” (p. 134).

Fox, Morris, and Rumsey (2007) discussed the implications of online focus group work for health research. In particular, they explored the use of online focus groups for drawing together participants with visible skin ailments. Here, too, questions of anonymity were paramount. They were particularly interested in the potential consequences of bringing together people who might be self-conscious about their appearance. Still, the authors voiced familiar enthusiasm for the practicality of online focus groups, “including reduced time and cost in terms of venues and traveling. It is also beneficial in eliminating transcription time and error” (p. 545).

In another article, Stewart and Williams (2007) compared online focus groups to “3D graphical environments,” such as *Second Life*. They noted that “[f]eatures of Internet interactions such as perceived anonymity, reduced social cues, and the realization of time-space distancing may lead individuals to reveal more about themselves within online environments than would be done in offline equivalents” (p. 399). They also suggested that users may perceive “computer mediated interactions as somewhat ephemeral: unguarded “conversations on a train” in an uncensored unpoliced environment” (p. 399). The question of the importance of capturing and analyzing multiple social cues was

extended in their comparisons between online focus groups and three-dimensional (3D) virtual reality environments.

The rush to online focus groups has raised several interesting and important questions. For example, the idea that virtual reality environments might “nullify” the concerns of earlier work typifies the utopic thinking that often accompanies discussions of computer-mediated social spaces. One is reminded here of the fantasies that accompanied distance education—that it would be just as good as “the real thing.” Yet, as many have come to realize, the communicative functions of the subtleties of bodily cues and other nonverbal elements of face-to-face interaction are not so easily recreated in virtual reality environments. Whether and how much the limitations of online focus groups are “nullified” within virtual reality environments thus remains an open question.

Utopian thinking is evident even in the language of most articles about the interactional and communicative affordances of new information technologies. Face-to-face interactions are even subtly recast as happening “offline”—as if the default were otherwise. Utopian impulses notwithstanding, all of these articles also insist that to understand the logics and affordances of online focus group work in richer, more sophisticated ways will require much more conceptual and empirical work.

Indeed, one great outcome of the advent of social media and other kinds of CMC is that they force us to engage with persistent questions on fresh terrains, each of which might be usefully thought of as a particular “modality” of interaction and communication with its own unique enablements and constraints. Many practical questions emerge from having multiple new modalities of interaction and communication available to us. Can technologically mediated forms of communication draw out the complexities and nuances encountered in face-to-face communication? How might online interactions allow for groups to “take over” in ways that mitigate the role of the researcher even more than we have discussed in this book? What might *subtexts* and *breakdowns* look like within these new modalities? Whether and how might they motivate the radical modes of self-interrogation that can (and often do) happen in traditional focus group conversations? What else might they spawn? Answers to these and other questions partially depend on how we conceptualize the “group” in technologically mediated contexts. Although we agree with Lichtman (2011) about the need for further research

on the nature, affordances, and functions of online focus group work, we also think that much conceptual work needs to be done with respect to units of analysis that motivate such research, differences in the specific constitutions of different interactional/communicative modalities, and our understandings of private, public, and the relations between the two.

Musings and Conjectures

The future of focus group research is wide open. New uses and affordances will certainly emerge as new conceptual breakthroughs are made with respect to qualitative inquiry, as researchers explore and exploit their affordances, and as new tools and environments for conducting research proliferate. As focus groups are increasingly “opened up” in new ways, their mediation across time and place will create a host of new possibilities for research. As Stewart and Williams (2007) argued, “the advent of Internet and networked communications has resulted in the proliferation of new social spaces, devoid of physicality. Adapted and adaptive social science research methods more generally allow for the collection and analysis of data from these diverse populations. These reengineered methodologies and methods can take advantage of these social worlds” (p. 413). Indeed, there is a great deal of conceptual and methodological work we need to do to develop tools that will allow us to explore and understand these worlds and their potentials fully.

Conducting focus groups work in virtual worlds also raises new questions and re-reflects old ones. Take the question of anonymity, for instance. The disembodied nature of social media allows participants to take on new roles and identities. This could help participants engage more openly and honestly when discussing potentially embarrassing or sensitive topics. It could also allow participants to speak more freely and honestly than they would in face-to-face settings. Yet, the reverse could be true as well. The disembodied nature of social media could allow participants to act in ways unconstrained by social conventions. Increased anonymity could also allow people to deceive others about their identities. How might talk and social interaction unfold, for example, in a study of racial differences in a social media context, if participants’ racial backgrounds were only known through self-report? And how would we make sense of data gathered in such a study?

Anonymity is only one of the many issues we need to (re)consider as new information technologies

proliferate. The issue of “community” is another. The disembodied nature of social media allows participants to create new forms of community—affinity groups, clusters, and spokescouncils, for example. Participants from across the globe can now communicate with each other instantly, in real time. Yet, whether and how the interactional dynamics (and effects) possible in virtual environments will be the same or different from those in proximal interactions remains almost completely unknown. How important, for example, is embodied, face-to-face presence in the creation of community? Can community itself be mediated? Will the glue that binds community in virtual landscapes be weaker or stronger than the glue that binds community in more traditional social landscapes?

As Sherry Turkle and others have made clear, mediated communication often has a striated or formatted feel to it. “The simple clarities of our globalized computer worlds depend on their virtuality. The real world is messy and painted in shades of grey. In that world we need to be comfortable with ambivalence and contradiction” (Turkle, 2004, p. 112). Do social media environments reduce complexity and contradiction, thus affording only more superficial connections among people? If so, what might be the consequences for research of conducting focus groups in these environments?

These questions all have ethical implications as well, some of which we have already broached (e.g., protecting anonymity and detecting deception). Other implications with respect to issues such as privacy, trust, transparency, control of content, and public welfare are relevant here as well and will need to be addressed.

Considering ethical questions with the collective (rather than the individual) at the center of things provides another perspective on these questions. And when the idea of the collective (or community) itself is troubled and expended within social media landscapes, more perspectives are likely to emerge. If, as we have suggested, focus group research has already challenged the Enlightenment self and the public–private split, then focus group research in virtual worlds is likely to challenge them even more.

These issues (and many similar ones) are indeed pressing. One can read the so-called Arab Spring and the Occupy Wall Street movements through them. On the one hand, these and other worldwide protests and revolutions were enabled by social media like Facebook. New forms of community were imagined and created. Private concerns became public. Regimes fell, and new concerns

around inequality came to the fore. On the other hand, lasting political movements and interventions depend on deep and abiding social connections and ties. The fates of these new forms of community are indeed unknown. Will they last? If so, what will account for their stability? Will they transmogrify? If so, how? And what will account for their shape shifting? These are very much open questions, the answers to which have important consequences for focus group research in the future. Suffice it to say that with the world changing as fast as it is, imagining how the forms, functions, and affordances of focus groups and focus group work might change in the wake of this changing world is dizzying indeed. Would that we had a crystal ball.

Conclusions

We would now like to offer some final thoughts. We discussed the history of focus group work. Typically, the goal of focus group work in these contexts was to gauge the effects of prescribed and delimited messages, products, and practices. Recall that “focus” was the methodological breakthrough in the earliest focus group work—a move that allowed for a particular kind of “scaling up” that often ignored the role of the “group.” Whereas most other books and treatises on focus groups have attempted to think through the nature of focus groups or to outline procedures for conducting them, we have attempted a more systematic historical and conceptual interrogation of the nature, functions, and affordances of focus group work in relation to contemporary debates about inquiry and method.

Although we did not intend this article to be a “how-to” guide—a point we are sure is evident by now—we do think it is practical in many ways. Because, all too often, we have seen questions of method reduced to questions of technique alone, we have tried to provide an antidote to this tendency. We have done this by arguing that an informed, principled use of research strategies and methods requires in-depth understanding of the co-evolution of theoretical, historical, and practical dimensions of any technique or strategy one might use—focus groups, for example. Additionally, interrogating the conceptual foundations and histories of method allows one to interrogate our epistemologies.

We hope this chapter has provided some tools for doing this kind of intellectual work. We also hope it can be used for practical purposes as well, but practical purposes enacted in sophisticated rather than simple ways. In this spirit, we end by

posing some questions and offering some musings in relation to them.

How does one begin to frame a research project? Novice researchers are often cautioned against letting “method” drive research topics and questions. In many ways, this is good advice, but it is not airtight. For some projects—ones that pivot on naturalistic, dynamic social interaction and where the collective is clearly the unit of analysis—a researcher may know that focus groups are essential to the project even before research questions are finalized.

How will you puzzle through the ways research questions and research methods might play out in your own work? Where or to whom might you appeal for help in this process?

How does one choose a site for conducting focus group work? As we implied throughout, location matters, especially with respect to issues of safety, comfort, and community building. Focus groups can, of course, be held anywhere. One needs to decide whether the setting and the study are of a piece, as was the case in Janice Radway’s study of romance novel enthusiasts. Even if setting and study are not of a piece, most studies cannot simply be conducted anywhere, with the expectation of generating the same findings. Holding focus groups in spaces that are familiar, safe, and comfortable to participants is fundamentally important.

What space or spaces do you think might work best for your focus group study? Why? How might these spaces help you make the most of the functions and affordances of focus group work?

Who will your participants be? We have advocated for exploiting preexisting networks because they tend to encourage collegiality and solidarity building. In most of the studies we discussed, including our own, groups were homogeneous. However, Michelle Fine and her colleagues in the Echoes Project chose to assemble groups composed of young people who did not normally interact with each other—people from across the ethnic, economic, and racial spectrum. Their hunches paid off in the sense that very dynamic, contested conversations occurred in their focus groups. Additionally, these groups produced very rich data, especially with respect to how participants understood the perspectives of others very different from themselves, interacted with those others, dealt with apparent contradictions, negotiated differences when they arose, and resolved conflicts.

Who will you recruit to be in your focus groups? Why? What advantages and disadvantages do you

see in having more homogeneous versus more heterogeneous groups? Why?

How will you recruit your participants? Again, there are many ways to answer this question. In Janice Radway’s study, she enlisted the help of Dot, the bookstore owner whom the women she wanted to recruit knew, admired, and trusted. The participants in Mitchell Duenier’s *Slim’s Table* (1992) were, in a way, “found art”—friendship circles at the restaurant where he conducted his study. As we argued, sometimes decisions about samples can be made strategically and in advance. Sometimes what seem to be principled, sensible recruitment plans do not pan out, and you need to develop new ones.

What kinds of recruitment strategies do you think might be most effective for your project? Why? What problems or issues do you think you might encounter in recruiting participants? How might you work through these problems?

What about facilitation strategies? Facilitators operate on a continuum from more active and directive to more participatory to more passive and nondirective. We think that a more “hands-off” approach to facilitation results in drawing out the unique and powerful functions and affordances of focus groups more fully. Some researchers, especially novice ones, find more nonautocratic approaches uncomfortable and difficult to maintain. Our suggestion for these researchers would be to ease into a more nondirective approach to facilitation slowly and deliberately, paying close attention to the effects of different ways of facilitating participants’ activity.

We have also advocated keeping notes, following up on key themes and gaps, listening for *breakdowns* and *subtexts*, and asking for elaboration on issues that remain. One could, of course, choose to be more focused and directive from the outset, although this would likely constrain the range of participants’ responses; we also realize that a more directive approach may work better in some projects and contexts. Exactly how to facilitate focus groups is something only discovered in the thick of things and often in collaboration with fellow researchers and research participants.

So, on the continuum from structured interviews to collective conversations, where do you think you should position yourself for your project? Why? What changes do you anticipate making as your research unfolds? Why do you suppose these changes will enhance the research process?

When is it time to end a focus group study? This is a difficult question to answer and often depends on a host of factors, from funding to time

availability (both of researchers and research participants) to impasses in the process of discovery to data saturation to disruption by unexpected and even traumatic events, to name a few.

Under normal circumstances (i.e., no traumatic reasons for ending), the standard response is to end a study when one reaches data saturation. However, there are a variety of reasons why one might want to continue a study or introduce a new angle on the study beyond this point. As we have argued throughout, focus groups are complex human affairs. They can and do develop a life of their own. The best focus groups are perhaps best thought of as eventually dissolving into the flux and flow of everyday human affairs. In this sense, deciding when to end a focus group study is akin to deciding when to end a conversation—and to begin a new one.

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Abstract

This chapter describes and discusses the major research methods used to study museums. These include gallery analyses and interviews with museum visitors, professionals, and stakeholders, as well as ethnographic fieldwork at museums. Drawing from a range of case studies conducted by museum practitioners, anthropologists, historians, and other museum studies scholars at a variety of museums, the author explores how these qualitative methods can be adapted to the study of exhibits, programs, and museums as knowledge-generating institutions. Approaches to research design, data analyses, and writing up are also examined.

Key Words: Knowledge creation, museums, exhibits, gallery analysis, ethnographic fieldwork, interviews, content analysis, case studies

The museum is teaching—expressly, as part of an education program and an articulated agenda, but also subtly, almost unconsciously—a system of highly political values expressed not only in the style of presentation but in myriad facets of its operation.

—*Susan Vogel* (1991, p. 200)

Why Study Museums?

Museums are a venue in which the construction of knowledge and its dissemination to the public is made visible and accessible. The constructed nature of exhibits allows for a window into social attitudes toward the objects, peoples, events, and places on display, as well as how these understandings change over time. As such, museums offer opportunities to explore how tacit assumptions are made explicit and tangible through the process of exhibit design. Processes of knowledge creation are present in the choices curators and other museum personnel make about what to display and how to display it, as well as in the voices that are given authoritative status through signage, labels, reconstructions, and video footage (Gurian, 1991,

p. 185; Moser, 2010, pp. 26–27). Moreover, subtle cues such as lighting, sound effects, and music, as well as the juxtaposition of one exhibit to another and even the architectural style of the museum itself can shape visitors' attitudes toward what they view (Levy, 2008; Moser, 2010, pp. 24–26).

Museums also offer opportunities to examine how people learn in informal settings, as well as why they might choose to do so. Recent research conducted by museum scholars explores how people understand and relate to the information on display during their visits and the factors that influence whether and how informal, voluntary learning resonates in their lives after their visit ends (Falk & Dierking, 2000).

Museum studies has its roots in research on material culture. Contemporary journals devoted to museum studies publish articles that are driven by research in museum collections, as well as critical readings of exhibits. Anthropologists, historians, and art historians have long used museum collections as a resource with which to study the technologies of the past, the diffusion of technologies from one society to another, and changes in material culture over space and time. This approach is suited to answering questions about the people and societies that created the items museums collect and display. Scholars engaged in material culture research utilize methodologies that require an in-depth understanding of culture and history and how a society's environment shapes its technology in terms of both materials at hand and the techniques used to create objects. This chapter focuses on a different kind of museum research: critical studies of exhibits, programming, and museum cultures. This type of research is best suited to questions about the role of museums in society, their relationships with various constituencies, how museums create knowledge, and what visitors take away from exhibitions. Such research requires ethnographic methods including content analysis of galleries, exhibition catalogs and brochures, museum websites, and curricular materials; interviews with museum professionals, visitors, and stakeholders; and participant observation at museums and the activities they sponsor. These methods can be deployed by scholars to study museums, but also by museum practitioners to evaluate their own work and determine if their exhibits and programs are meeting their goals and objectives.

Research Design

One can employ a variety of research methods to study the effectiveness of museum exhibits and programming and the knowledge-creating capacity of museums. The methods we choose are shaped first and foremost by the questions we seek to answer, but also by considerations such as the amount of time at hand, the availability of funding for long-term research and travel, and the desired length of the final project. Whether we conduct a content analysis of visual displays and signage, interviews with museum professionals or visitors, or participant observation in galleries or of museum programming, we must start with a few basic questions. As Stephanie Moser, archaeologist and museum studies scholar, points out in her piece "The Devil Is in the Detail," we must keep in mind that within museums "critical components of displays complement

and reinforce one another to create a system of representations" that shape visitors' understandings of certain subjects (Moser, 2010, p. 23). Thus, it is necessary to learn about the educational and professional background of those involved in creating the exhibit and choosing the themes that are exhibited, as well as their goals in doing so (p. 24). What were the criteria they used for deciding what was or was not included in the display? It is important to note how and to what extent this type of information is acknowledged and incorporated into the exhibition itself. In some cases, the history of the collection itself, how it was acquired by the museum, and the conditions under which certain pieces were created can be equally important (p. 24; see, for example, Jacknis, 2008).

One must also consider who the museum perceives as its intended audience for an exhibit, what they are meant to learn as a result of their visit, and how such goals tie into the museum's mission. For example, we might consider children the primary audience at children's museums, but they are always accompanied by an adult, typically a parent. When I visited the Children's Museum of Indianapolis with my son, I felt like I was an important part of the audience, too, because the majority of exhibits had two sets of labels. The first, written for children, were placed no more than three feet off the floor and, in some areas designated for toddlers, signage was even lower and featured questions or rhymes written in simple sentences. The second set of labels, clearly targeted to adults, were displayed at somewhere around five feet and provided information about how children learn or offered suggestions on how to engage with children through the displays. As a result, I not only had fun playing with my son but came away from my visits feeling that I had learned something about children's learning. Not surprisingly, the Children's Museum of Indianapolis emphasizes learning in its mission statement: "Our Mission: To create extraordinarily learning experiences across the arts, sciences, and humanities that have the power to transform the lives of children and families," and they follow this with statements stressing that their goal is to "understand how children and families learn" and "to foster family learning and nurture interaction between children and their families" (<http://www.childrensmuseum.org/mission>). This particular museum emphasizes the social and familial aspects of learning.

It may seem obvious that parents are a large potential audience at museums designed for children, but few that I have visited actually address

parents as a target audience through labels directed at them. The mission statement of the Boston Children's Museum emphasizes the importance of learning through play, "Boston Children's Museum engages children and families in joyful discovery experiences that instill an appreciation of our world, develop foundational skills, and spark a lifelong love of learning" (<http://www.bostonchildrensmuseum.org/about/mission-vision-values>). Statements that follow talk about children and families, but the overall emphasis is on the child, "Boston Children's Museum's vision is to be a welcoming, imaginative, child-centered learning environment that supports diverse families in nurturing their children's creativity and curiosity" (<http://www.bostonchildrensmuseum.org/about/mission-vision-values>). This museum has a number of excellent exhibits that are engaging to visitors of a variety of ages, but during my visits with my son I did not see any labels that were directed specifically at parents. I had fun playing with my son at these exhibits and felt that he learned from our visits, but I did not feel as if the exhibits' designers were attempting to speak to me as a parent.

It would be interesting to compare levels of parental engagement at children's museums that emphasize learning as a family activity versus learning as play. Are there things that institutions do to foster or limit family interactions in exhibit spaces? Even in this short exercise, we see how a museum's mission might inform practice, potentially resulting in different experiences for both children and their families.

Questions of curatorial motivations, selection, interpretation, and intended audience are the foundations on which all other inquiry is built. The remainder of this chapter is an exploration of some major approaches to research design in the field of museum studies. I discuss approaches to exhibit analysis, as well as the use of interviews and participant observation in museum-based research. Some of these might be answered before even setting foot in the museum itself by consulting a museum's website for mission statements or other promotional materials to see how visitors' expectations are being shaped by advertisements and other museum authored information.

Exhibit Analysis

Visual displays; reconstructions; labels and voice; the use of lighting, sound, and interactive elements; as well as the architecture of the museum itself and the juxtaposition of one exhibit in relation

to others all convey tacit and overt messages about the items on display, the people who created them, and the cultures and epochs they represent. Design creates content (Moser, 2010, p. 23; Serrell, 2006, p. 33). As a result, detailed description and analysis of the visual, written, and technological exhibit elements is typically the starting point for data collection in museum research. I recommend that scholars begin by drawing a map of the exhibit, making note of indicators given to visitors of the direction of the exhibit (or lack thereof) and where objects and texts are displayed in relation to one another. The location of interactive elements of the display should be noted as well, with attention paid to issues of accessibility. For example, height may restrict their use to adult, ambulatory visitors, and the space around them may either limit use to one visitor at a time or encourage interaction and discussion. Mapping should be followed with a detailed outline of information conveyed through signage, labels, and charts, as well as by other sorts of visuals such as maps and photographs. The voice and font used in any sort of written aspect of the display can be powerful in shaping attitudes; as Elaine Gurian suggests, "labels may assume the role of teacher, coconspirator, colleague, preacher, gossip columnist" (1991, p. 185), in this way labels may encourage a specific type of audience interaction with the exhibit. Additionally, museum researchers should take note of the reading level of the language used, the definition of terms, descriptions of techniques used to create objects on display, and the location of all geographic locales on maps (p. 185). The font used in labels and signage can convey messages to viewers as well (Moser, 2010, p. 27). Similarly, James Clifford has suggested that we would do well to pay attention not simply to the content of photographs in displays, but to the ways in which they might indicate a historical approach, in contrast to an aesthetic one (1991, p. 222). He argues that the systematic use of black-and-white or sepia photographs gives the impression that what is on display is of the past, whereas color photography signals currency (Clifford, 1991).

Many other factors can contribute to mood as well. Among these, sound is important. Whether we are considering the deliberate inclusion of background music and sound effects or unintentional sounds, such as creaking floorboards or the sound of audio or visitors from neighboring exhibits, sounds influence visitors' experiences. Lighting can also be key in conveying a mood. Boutique lighting can be used to spotlight the uniqueness of objects

in display cases, potentially inspiring wonder at the objects on display (Moser, 2010, p. 26). However, it can be difficult to discern the extent to which lighting—particularly low lighting—has been dictated by guidelines and agreements regarding the number of lumens per day to which objects can be exposed versus aesthetic considerations. One thing to keep in mind is that unless such considerations are spelled out for them, few visitors will interpret lighting in terms of curatorial decisions about the care of the specimens on display. One of the most memorable areas at the Indianapolis Zoo is a reptile room that is almost completely dark save for the small rectangular glass cases lit from within where snakes are displayed among artistically arranged stones, water bowls, and perches that contrast to their coloration. The boutique lighting and the small cases created an impression that I was seeing rare, one of a kind objects like jewels, and I found myself reflecting on the snakes' beauty rather than what their lives might be like in their natural habitats. Regardless of the motivations behind such choices, lighting will shape how visitors experience and react to an exhibit.

The case studies that follow are all based on exhibit analysis and illustrate the importance of the factors I have discussed to this point in shaping visitors' interactions and understandings of a variety of exhibit displays. Some are classics of museum studies research that offer relevant insights even today; others are newer examples of museological inquiry. Although carried out in art museums, natural history, and regional and community museums, all the exhibits discussed here are focused on Native Americans or exhibits of non-Western art. Such exhibits specifically address other cultures or groups within our own society that have been marginalized and, in doing so, render explicit the knowledge-generating power of museums, offering glimpses of the implications of the messages they convey beyond the walls of the museum itself.

In "Other Cultures in Museum Perspective," Ivan Karp shows how choices such as how objects are placed in relation to one another can serve to exoticize or assimilate the subjects of the exhibit by making them seem alien or familiar (1991, p. 375). Taking the Museum of Modern Art (MoMA)'s "Primitivism in Twentieth Century Art: Affinity of the Tribal and the Modern" as a case in point, Karp examined how curator William Rubin paired Western modern art created by Picasso, Man Ray, and others with the African art that served as its model or with pieces Rubin perceived as aesthetically similar (p. 376). All the works on display

were exhibited with the minimalist labels typical of art museums. Karp suggests that this technique emphasizes the aesthetic value of the African pieces (by displaying them in the same way the museum would display Western art) and encourages visitors to appreciate it on those terms alone (pp. 376–377). However, he argues that the juxtaposition of the pieces, coupled with the lack of contextualizing information about the African pieces and the artists who created them, reduced the non-Western works to a resource to be mined by Western artists and that, in the process, differences in content, intentionality, sociocultural contexts of production, and history are lost in a comparison that ultimately assimilates the aesthetics of African artists into "a particular moment within his [William Rubin's] own tradition" (p. 377). Karp uses "'Primitivism' in Twentieth Century Art" as an entrée to discuss exhibition techniques that assimilate difference (as does the MoMA exhibit) versus those that exoticize difference by contrasting it to something familiar and accentuating the dissimilarities so as to make them seem bizarre. Simply by placing some groups of people and their creations in history, science, or art museums and others in natural history museums, we assimilate some groups and exoticize others.

Native Americans are among those who, until relatively recently, were rarely seen in exhibits outside of natural history museums (Karp, 1991, p. 377). Scholarship on museum representations of Native Americans offers a powerful example of the effectiveness of gallery analysis in drawing out tacit understandings of cultural others conveyed through museum exhibitions. Several authors have written pieces analyzing and comparing representations of Native Americans in the same institutions over time (Hill, 2000; Jacknis, 2008), whereas others have compared exhibits in major state-sponsored institutions to those in community-operated museums (Clifford, 1991; see also Levy, 2008, on the Saami). In these cases, gallery analyses were used as a starting point to discuss changing trends in popular representations of Native Americans and to explore the relationship between art and culture.

Drawing on gallery analyses supplemented by analyses of print and visual materials produced by the museums, Clifford compares four Northwest Coast museums: the University of British Columbia Museum of Anthropology, the Kwagwiltz Museum and Cultural Centre in Quadra Island's Cape Mudge Village, the Royal British Columbia Museum in Victoria, and the U'mista Cultural Centre in Alert Bay on Comorant Island. Although

the museums exhibit similar objects, some present the objects as fine art, whereas others treat them as artifacts demonstrating cultural change, adaptation, oppression, and struggle. Clifford uses descriptions of each museum's locale, architecture, and style of exhibition to examine fundamental characteristics of majority and tribal museums and to explore differences in their discourses on history, community, and ownership. He also explores the broader implications of exhibiting objects as art versus artifact.

For example, at the University of British Columbia Museum of Anthropology, Clifford notes that the printed guide's first sentence explains that the objects are exhibited as fine art, emphasizing their visual quality (1991, p. 219). Thus, the labels are similar to those at an art museum, noting cultural group, place and date of creation, and type of object (e.g., contemporary carving) and giving a brief description that includes the name of the artist if known. Some labels contextualize the pieces by including small drawings of the works in their original settings, such as how posts or entryways were attached to homes (p. 219). Similarly, Clifford describes how boutique lighting is used in the museum's "Masterpiece Gallery" to drive home the message that the pieces displayed there are works of art. Clifford describes how older artifacts are displayed in close proximity to contemporary work, conveying the message that "tribal works are part of an ongoing, dynamic tradition" rather than something salvaged from a vanishing culture (pp. 220–221). At the University of British Columbia Museum of Anthropology, there is no overarching historical narrative. Visitors can take a variety of paths through exhibit spaces, and the message of the permanent exhibit does not depend on taking one particular route. Furthermore, Clifford notes that the exhibit relies on drawings to provide contrast, rather than on historical or contemporary photographs. In this way, the museum emphasizes the aesthetic properties of each piece, communicating across cultures a sense of its quality and value (pp. 221).

In contrast, at the Royal British Columbia Museum, the focus is on adaptation, crisis, and conflict throughout the region rather than on the particular histories or traditions of a given tribe (Clifford, 1991, p. 218). Thus, the historical sequence in which the pieces are presented "suggests that the traditional objects on display were not necessarily made prior to white power but in relation and sometimes in defiance of it" (p. 218). To illustrate this point, he describes galleries devoted to cultural contact with missionaries that contrast

Haida Shaman figures carved in the 1880–1890s with a Tlingit sculpture of a Christian priest to signal the decline of the shaman's authority and the rise of new forces in the area (p. 216). Focusing on exhibits devoted to the smallpox epidemic of 1860s, missionary influence, and *potlatch* and its suppression, as well as a large reconstruction of a chief's house, Clifford illustrates that change is discussed throughout the museum, giving a sense of the dynamism and adaptability of indigenous culture (pp. 217–218).

Similarly, history is also central to the story told with objects at the U'mista Cultural Centre in Alert Bay. In this case, though, the story is not an overarching history of the region but specific to the Kwagiulth and the Great Potlatch ceremony of 1921 that led to the arrest of a number of chiefs and the confiscation of their *potlatch* regalia by Canadian authorities. Indeed, the collections of both the tribal museums studied by Clifford are comprised of repatriated *potlatch* gifts returned not to the family's who owned them, but to tribal museums (1991, p. 230). Clifford relates how the U'mista Cultural Centre conveys the story of the *potlatch*, its suppression, the arrest of their leaders, and their struggle to regain the lost regalia by displaying the objects around the perimeter of a great hall in the same order that they were presented at the Great Potlatch of 1921 (pp. 237–238). Cards bearing text tell little about each individual piece or its use or significance beyond the Great Potlatch, focusing instead on the history of the event (p. 238).

Visitors stand in the center of a large room in the same area where guests at a *potlatch* would sit, although, as Clifford notes, the atmosphere and awe created by seeing the works by firelight is lost here. Clifford writes, "The display's effect, on me at least, was of powerful storytelling, a practice implicating its audience... I was not permitted simply to admire or comprehend the regalia. They embarrassed, saddened, inspired and angered me—responses that emerged in the evocative space between objects and texts" (1991, p. 240). Clifford's piece is itself a compelling example of how museum studies in general and comparative gallery analysis in particular can serve as the basis for exploring a number of topics, among them the disconnects between dominant and oppositional discourses on the past and the classification and presentation of objects as art versus ethnographic artifact.

Similarly, Richard W. Hill Sr.'s "The Museum Indian Still Frozen in Time and Mind" compares Native American galleries at two Denver museums,

the Denver Art Museum and the Denver Museum of Natural History (DMNH). Hill traces the origins of contemporary displays of Native American culture to nineteenth-century anthropologist Lewis Henry Morgan's attempts to create a taxonomy of Native Americans focused on the technological development of tools and containers (Hill, 2000, p. 42). Anyone who has ever visited a museum of natural history and wandered through displays of arrowheads, basketry, and mannequins in traditional attire has experienced the long-reaching legacy of this approach to presenting Native American culture as ethnologic artifact. In the 1970s, the Denver Museum of Art sought to challenge that mode of exhibiting by presenting objects from their Native American collections not as artifacts in glass cases but as fine art to be appreciated for its intrinsic aesthetic value (p. 43). Similarly, the Denver Museum of Natural History sought to break the mold of presenting Native American culture by environmental region (e.g., Eastern Woodlands, Great Lakes, Central Plains, Northern Plains, Southeast, Plateau, Northwest Coast, Prairie, Sub-Arctic and Far North), which all too often obscured a great deal of cultural and linguistic diversity in an effort to demonstrate common traits of the distinct nations living in a given region (p. 43). Together, these institutions challenged a mode of exhibiting Native Americans through a taxonomy of culture rooted in the nineteenth century, one that focused on adaptations to the environment and utilitarian aspects of material culture.

In 2000, Hill returned to these groundbreaking museums to see how their exhibits had changed since the late 1970s (2000, p. 43). Beginning with a walk through of the DMNH's Indian Exhibitions, Hill notes that, on the surface, little had changed. A display of medicine masks of the Tuscarora had been repatriated under the Native American Graves Protection and Repatriation Act (NAGPRA) and an explanation was placed near their former location in the exhibit (p. 44). This allowed visitors to see that the museum was responsive to concerns of Native peoples and that their sacred practices were a part of a living, ongoing tradition (p. 44).

The regional approach to displaying Native American culture was predominant at the DMNH, as were dioramas and other reconstructions, which Hill describes in detail. One that was particularly noteworthy was the Miccosuki diorama depicting a Miccosuki man in the clothing of his ancestors, paddling a dugout canoe through the Everglades (Hill, 2000, p. 44). Hill notes that because

Miccosuki people in Florida still dress in the style of their ancestors, the diorama gives a somewhat misleading notion of timelessness (p. 44). To anchor visitors in the present, curators added wrappers from contemporary snack items to the grass by the river. Ultimately, Hill illustrates that, although beautiful and creative, the reconstructions at DMNH still represent Native Americans as timelessly tribal, ignoring important changes in their lives and their communities, as well as their responses to them (p. 44).

Like the DMNH, the Denver Art Museum's Gallery for American Indian Art is organized by geographic region. However, in documenting the gallery's design, Hill shows how curators have worked to display objects as fine art pieces with utilitarian functions that might reflect an individual's cultural, spiritual, or personal needs (Hill, 2000, p. 58). For example Plains Indian *parfleche* bags are displayed on gray tripods in order to give a sense of how they were intended to be seen. Hill suggests that this allows viewers to "imagine what they might look like if the wind blew the fringe" (p. 59). According to Hill, the exhibit included text offering historical and cultural information, as well as descriptions of how the objects were used. In this way, pieces are treated as art, but visitors are given tools with which to place them into cultural and historical context. Unfortunately, Hill does not share the content of any of this signage with his readers. Texts convey more than just the "facts" or which facts were privileged over others in the selection process, and style and voice can influence how visitors perceive subjects (Moser, 2010, pp. 26–27). Without the actual text, however, all of these data on how museum professionals interpret and present knowledge to the visitor are lost. Nonetheless, Hill's work is a useful example of gallery analysis. He demonstrates the lasting impact of nineteenth-century views of Native Americans and how these ideas continue to influence knowledge creation and dissemination in public venues. At the same time, Hill offers thoughtful and practical suggestions for how to move beyond stereotypes in a variety of museum contexts.

Ira Jacknis's examination of changing exhibits about Ishi and the Yahi at the University of California Museum in San Francisco focuses on changing displays of the same collection over time at the same institution. Ishi, as Jacknis reminds us, was the last surviving member of the Yahi tribe, and he came to the museum after a life lived in hiding from white Californians who wiped out his community;

he ultimately resided in the museum itself, appearing not infrequently as a living part of the exhibit (2008, p. 60). Alfred Kroeber, anthropologist and curator of the museum, viewed Ishi as the last Native American in California to have lived a traditional life free of the influence of cultural change, despite the fact that this was manifestly not the case (p. 63). The Yahi collection was comprised of artifacts acquired in three different ways: those looted by whites, those collected by museum employees from sites Ishi took them to, and those Ishi made himself while employed at the museum as a custodian and demonstrator of Yahi culture (p. 67). During Ishi's life, the museum's exhibitions were influenced by salvage ethnography of the Boasian school and thus, as Jacknis demonstrates, curators sought to repress signs of cultural change in favor of presenting visitors with "authentic" Yahi culture (p. 63). For example, photographs of Ishi wearing animal skins and furs were on display, although this is not representative of traditional Yahi attire, while other photographs of Ishi that suggest that he preferred to dress like those around him were not exhibited (pp. 70–73). Similarly, the way in which Ishi produced tools for display in exhibits was also obscured. Jacknis shows how, provided with new tools and materials as well as the museum's entire collection for inspiration, Ishi produced pieces that were creative, experimental, and at times more artistic than utilitarian, yet these pieces were displayed as representative of traditional Yahi culture without reference to his creative process (pp. 67, 69).

Other curators used the same collection to tell different stories. In 1961, five decades after Ishi's death, the Hearst Museum adapted Theodora Kroeber's book *Ishi in Two Worlds: A Biography of the Last Wild Indian in North America* into an exhibit that reiterated Alfred Kroeber's representation of Ishi as a "stone age Indian" untouched by time, although they added the theme of genocide (Jacknis, 2008, pp. 75–76). In 1990, Susan Berry retold the Ishi story in terms of cultural exploitation (Jacknis, 2008, pp. 80–81). Later, Steven Shackley focused on stone tool technology, comparing Ishi's work to those of neighboring groups and highlighting both the features in common and Ishi's own innovative techniques (Jacknis, 2008, p. 81). Most recently, Jacknis himself redid the exhibit, creating *Ishi and the Invention of Yahi Culture*, which ran from 1992 to 2001 and highlighted issues of adaptation and innovation, acknowledging the role of the museum and the Kroebers in the invention of Yahi tradition (Jacknis, 2008, pp. 81–82).

By tracing shifts in the Hearst Museum's exhibits about Ishi, we get a sense of how depictions of Native Americans have changed along with priorities and understandings within the field of anthropology. At the same time, we see from the museum professionals involved in the various displays an increasing openness to acknowledge the historical context of genocidal policy that propelled Ishi to the museum in the first place, as well as ever more sophisticated and nuanced approaches to exhibiting the collection and understanding how objects came to be collected and produced.

Finally, one of my favorite pieces that explores the constructed nature of exhibits is not a gallery analysis but an essay by Susan Vogel about a number of exhibits she designed to educate viewers about the implicit messages conveyed through visual displays. In "Always True to the Object in Our Fashion," Vogel, a museum professional wrestling with audiences' knowledge (or lack thereof) of Africa, describes specific strategies she developed through three different exhibits that address visitor expectations and pre/misconceptions (1991). In "The Art of Collecting African Art," Vogel displayed objects that were the pride of the collector alongside works that were second-tier, altered, restored, or fake (p. 193). Labels written in a personal, informal, and opinionated style encouraged visitors to examine the pieces closely and form their own opinions before reading the label for Vogel's view (p. 193.). For "Perspectives: Angles on African Art," Vogel asked ten co-curators—some Africans, some from the United States—to choose a single piece and use it to discuss what African art has come to represent (pp. 193–194). Each piece was accompanied by a label authored and signed by each curator, as well as a checklist of information on the use and meaning of the pieces for their original African owners. Some curators talked about their pieces as central to national patrimony or personal heritage, others described them as objects in an art collection, part of art history, anthropological artifact, an influence on twentieth-century art, materials for artists to draw upon, or as expressions of living religious and political beliefs (p. 194). Finally, in "Art/artifact," Vogel created an exhibition about perception and the museum experience that focused on how Westerners have classified and displayed African objects (p. 195). She did so by drawing on different installation styles, such as that of an art gallery with boutique lighting or an ethnographic museum with objects displayed in glass cases with a great deal of text.

In each of these exhibits, Vogel employed strategies that brought the curator's role in interpreting work to the fore of visitor's experience, demonstrating how a curator's own understandings of the works he or she displays and the people who produce them may "rest on unquestioned and unexamined cultural—and other—assumptions" that are worthy of visitor's consideration (1991, p. 191). She points out that although politically savvy visitors may focus on questions of provenance, funding, and profits, there are subtler messages being conveyed to visitors by what the museum collects and displays and how it does so, as well as whom it chooses to address in its programming and exhibits (pp. 198, 200). All these details, she reminds us, will tell the audience how to think about the peoples and places that are the focus of exhibits long after the visitor leaves the museum.

Interviews

Open-ended, informant-directed interviews with museum practitioners aid the researcher in discerning the tacit assumptions, goals, and factors that shape and constrain the choices that museum practitioners make in creating exhibitions. Moreover, interviews with those involved in researching, designing, and creating exhibits can give a sense of how an exhibit fits into the overall mission of the museum and the institution's long-term goals. Additionally, on a practical level, interviews allow us to learn about things that are not ongoing during the time of our research.

Although it is possible to find practitioner-authored articles about their own work or blogs that aid the researcher in achieving the same ends, the synergy created in the interview process sometimes leads to the discovery of aspects of the creation process that are entirely unexpected and that might not otherwise be discussed. Interviewing guides, docents, interpreters, or volunteers who interact with the public also has its benefits. First, it can help one gain a sense of how visitors engage with the displays, interactive elements, and public programming. Second, it can help researchers discern to what extent the goals of exhibition creators are being successfully conveyed to visitors or if something is being lost in translation. Such research can help pinpoint disconnects between curatorial intentions and outcomes and help generate ideas about how to address them. They can also be revealing of institutional culture.

An example of interview-based research is Daniel Sherman's interview with curator Ilona Katzew about

Inventing Race: Casta Painting and Eighteenth-Century Mexico at the Los Angeles County Museum of Art (LACMA) from April 4–August 8, 2003. The interview is presented in transcript form as part of a dossier that included exhibition publications and press releases, as well as reviews of the exhibit that appeared in two newspapers (Katzew & Sherman, 2008). We learn from the *Casta* Exhibition Brochure that *casta* paintings are family portraits that overtly illustrate the results of the intermingling of the races in colonial Spanish America through family portraits (pp. 292–293). Most were conceived as sets of 16 scenes painted on a single canvas or as separate surfaces arranged hierarchically, beginning with figures of Spaniards, perceived as racially "pure" (p. 293). Each additional scene depicted a family group with parents of different races and one or two of their children engaged in activities that, along with their attire, indicate their social status (p. 293). Each family portrait is accompanied by inscriptions identifying the racial mixtures of those depicted in each scene (p. 293).

During the course of the interview, Daniel J. Sherman asks Ilona Katzew a range of questions beginning with her background studying art history at New York University, how she became interested in *casta* paintings, and the genesis of the exhibit. He also asks a number of questions about exhibition style, the intended path of visitors through the exhibition, and the positioning of explanatory charts and labels. In the discussion that ensues, it becomes clear that Katzew hoped that visitors' initial puzzlement about the messages of the paintings would arouse their curiosity. To this end, she designed the exhibit so that visitors' paths would take them to a central piece multiple times in their trajectory through the exhibit, giving them the opportunity to view it anew with the benefit of the information they gleaned throughout the exhibit as a whole (Katzew & Sherman, 2008, p. 29). A question on the difference in the Spanish and English titles for the show led to a lively discussion of the relationship between *casta* painting and the development of the notion of race and hybridity in Mexico and how these concepts differ throughout time and between the cultures of Mexico and the United States (pp. 312–319). This part of the piece alone would be of interest to scholars on race, but also to those seeking to understand how notions of race are historicized, critiqued, and conveyed through art and art history to museum visitors. At Sherman's prompting, Katzew also discusses the challenges raised by creating an exhibit on the construction of race in

Los Angeles, a city with its own racial hierarchy and history of race-related violence (p. 315). Among other things, in her response, Katzew describes concerns voiced by LACMA's education department who were "in all honesty quite alarmed, thinking that viewers would come out... —especially viewers of either mixed backgrounds or African Americans—with a lower sense of self, thinking that only whites are placed at the top, and that because they're mixed they're placed at the bottom of the social hierarchy" (p. 316). Apparently, such fears motivated members of the education department to suggest censoring the tours so that the most violent, racist images would be excluded—something Katzew resisted and dealt with by offering training programs on the exhibit to other museum personnel (pp. 316–317). This conversation in particular is worthwhile both for scholars of museum studies and practitioners involved in curating exhibits on violence or oppression.

One of the exchanges in this piece that I found the most interesting was a story Daniel J. Sherman shared about his visit to the exhibit in which a small group of visitors read aloud a label on the historical practice of "blood-mending," which the exhibit explained was not an option for people of African descent (Katzew & Sherman, 2008, p. 322). One member of the group commented "see, if you have any black blood you are black," a remark that both authors interpreted as a sign that the visitor accepted this historical explanation, as if it were an authoritative statement on the biology of race (p. 322). Katzew offered another example of a family she witnessed who—much to her dismay—used the paintings as a guide to see where they fit into the racial scale (p. 322). That the exhibit could serve as a tool to reproduce the categories the curator sought to deconstruct is a powerful example of how exhibits shape understandings of subjects, albeit sometimes in unexpected and unintended ways. It's also a warning that interviews and gallery analyses might not be enough for scholars to learn how exhibits are interpreted and understood by visitors.

In their book *Learning from Museums*, authors John Falk and Lynn Dierking describe research undertaken at the National Museum of Natural History in which interviews played a crucial role (2000, pp. 3–8). Researchers first gained permission to shadow visitors during their trip to the museum and then conducted interviews with them before they left the museum and again five months later. The results were revealing. The authors present

the case of two women in their late 20s, who both lived in Chicago and worked as editors of children's textbooks, one focusing on science texts, the other on art (p. 3). They describe how the women visited several different parts of the museum during their ninety-minute visit but how they spent, on average, about fifteen minutes in each section (p. 4). Before their departure, researchers interviewed the women about their visit, why they came, what they discovered that was new to them, what they found the most interesting, and the like. The researchers were somewhat discouraged by the initial interview, noting that an optimist might conclude that the first woman came away with a greater appreciation of the variety and adaptability of spiders and an enriched understanding of the size and diversity of dinosaurs, but a pessimist might feel as if they hadn't learned much at all (p. 4). However, five months later, when researchers contacted the women for a follow-up interview, the results were far more encouraging. The interviewer asked the first woman, who edits science textbooks, if she had discussed her visit with others, if she had thought about it, or if other events in the months since her visit had brought the museum to mind (p. 5). What they learned was very revealing. The woman spoke with greater enthusiasm about her visit five months later than she had on the day of her trip. Not only did she share her enthusiasm about the exhibits she enjoyed the most with members of her family upon her return, but, in the intervening months, she mentioned several events that had caused her to think about other exhibits she had seen at which she had spent less time and that she had not discussed at all in her initial interview (p. 5). The second woman remained the less enthusiastic of the two in the second interview. However, in the second interview, she spoke about how an exhibit about amphibians she'd seen on her trip had aided her in imaging the environments described in a novel she was reading (p. 6). Both women's visits had resonated with them five months later. Additionally, in the follow-up interview, the second woman mentioned that she and her friend had also visited the National Gallery and that it was this museum visit that she had talked and thought about more in the intervening months (which isn't surprising given her interest in art) (p. 6). Clearly, this is compelling research that demonstrates the power of exhibits to capture our imagination well beyond the parameters of any one visit, while demonstrating the need for researchers to follow-up on-site interviews with visitors weeks, even months later.

A final example worth considering is curator Heather Igloliorte's discussion about the use of oral history interviews in an exhibit entitled "*We Were So Far Away*": *The Inuit Experience of Residential Schools*. Although the article is not itself based on interviews, it offers insight into how Igloliorte used oral history interviews to create an exhibit with the dual mission of healing survivors of residential schools and educating the wider public about the oppression and abuse suffered by Inuit children in these institutions. As she reports, the oral histories collected from eight survivors from the four Inuit geographic territories of Canada became the centerpieces of a touring exhibit designed for a hierarchy of audiences, including residential school survivors, Inuit communities across the Arctic and Subarctic, and the wider Canadian public (Igloliorte, 2011, p. 23). Created to "supplement, assist, and encourage the many healing initiatives that are already being undertaken in communities across the Canadian North," the author describes the strategies she developed in creating an exhibit that would care and protect survivors while conveying their stories to audiences around the country with widely varying knowledge of and experiences with Inuit residential schools. Particularly daunting was the task of how to represent survivor's testimonies without causing distress to visitors, many of whom were survivors themselves or who had family members who were (p. 31). To remind visitors that the narratives were part of lived experience, each section of the exhibit displayed a banner bearing a large close-up portrait of a survivor's face and a particular theme, such as language loss or the impact of assimilation that he or she emphasized during the interview (p. 33). Labels displaying excerpts from the interviews were displayed alongside personal photographs and items that the survivor chose as significant to her or his experience in the residential schools for the exhibit (p. 33). The complete interview transcripts were available in the exhibition catalog and a DVD recording of the interviews played in the background (p. 33). Taking a protective stance toward visitors, Igloliorte arranged for health care teams from Health Canada to be present at all exhibition openings in Northern Canada and provided Health Canada postcards with regional and national numbers for confidential and free counseling to visitors in areas of the exhibit where other resource materials were available (pp. 35–36). Igloliorte's piece gives us a lens into how curators conduct and use life history interviews in their exhibit design and demonstrates the potential for museums to heal even as they educate.

Participant Observation and Ethnographic Fieldwork

Taking an ethnographic approach allows one to observe how the messages conveyed by exhibits are shaped from the moment of inception through various levels of development by the social forces that comprise a given museum's culture. Such an approach may be best suited to a museum as a whole rather than to a single exhibit and makes for a longer finished project. Conducting fieldwork in the exhibit space at related museum events or educational outreach and with a variety of museum personnel and visitors yields an in-depth and nuanced understanding of the forces that shape knowledge production and its dissemination in and through museums. It is an approach that makes for persuasive results and fascinating reading, but there are relatively few studies that focus on a single museum in this way. Among these are Edward T. Linenthal's *Preserving Memory: The Struggle to Create America's Holocaust Museum* (2001) and Richard Handler and Eric Gable's *The New History in an Old Museum: Creating the Past at Colonial Williamsburg* (1997). The former traces the work, much of it political, that went into creating and funding the museum, finding a site, defining its mission, and assembling its permanent collection. However, it is not about the workings of the museum itself, and it is the product of historical rather than ethnographic research. Handler and Gable's landmark monograph is perhaps the most comprehensive example to date of ethnographic fieldwork conducted at a museum. Working from the premise that, "most research on museums has proceeded by ignoring much of what happens in them," the authors treated Colonial Williamsburg as a complete social world "where people of differing backgrounds continuously and routinely interact to produce, exchange, and consume messages" (Handler & Gable, 1997, p. 9). Thus, they set about examining the culture through which employees of all types at Colonial Williamsburg—museum, business administration, service, and support—along with museum visitors created messages about the past that were conveyed to the public.

Handler and Gable's study explored the meanings of the past and examined who has the power to assign value to cultural and historical productions (1997, p. 8). Focusing on Colonial Williamsburg historians' attempts to introduce a social history that consciously examined racial and class inequalities, their primary research question was why historical representations at the site changed or failed

to change in the ways that the researchers intended. During the course of their fieldwork, Handler and Gable undertook three broad areas of inquiry. First, they were concerned with the institution's representations of groups of people perceived to be different (p. 11). Among other things, this led them to focus on educational programming, the training of historical interpreters who interact most directly with the public, and how the museum dealt with slavery and African-American history. Second, they explored the ideologies and interests that informed and reinforced these representations. This led them to examine the mission of the institution, the various stakeholders the museum serves, the role of various types of museum workers in making choices about museum content, and to consider who benefits from the visions of difference on display at the museum (p. 11). Finally, Handler and Gable concentrated on the construction of audience through advertisements and programming, as well as on audience responses to the messages conveyed (p. 11). In the final product, Handler and Gable narrowed the scope of their project by focusing on five aspects of Colonial Williamsburg, including the museum's educational programs, employees who worked with artifacts and objects (both those in research and those on the business side who developed and marketed products), the hotel and restaurant workers' union that went on strike during the course of their research, and corporate culture (pp. 22–23). Finally, Anna Lawson, a graduate student at that time, researched and presented work on the Department of African American Interpretation and Presentation, for the most part contained in her as yet unpublished dissertation (1997, pp. 22–23, see also Gable, Handler & Lawson, 1992).

The approach taken by Handler and Gable will be familiar to anyone who has studied cultural anthropology: in short, they immersed themselves in the life of the museum, participating to the extent that those in power at the museum allowed them to in the everyday operations of the museum and its corporate side. Handler and Gable, along with Anne Lawson, interviewed more than 200 people employed in various aspects of the museum, including historians, curators, wait staff, house-keeping staff, janitors, gardeners, security, bus drivers, construction workers, IT workers, marketers, publicists, and the like (Handler & Gable, 1997, pp. 14–15, 21). Among these, twenty-five were key informants with whom the researchers had more than a casual relationship (p. 21). Additionally, they interviewed more than fifty visitors to the museum.

These interviews lasted from less than an hour to seven hours in length (p. 21). The team observed and recorded tours of every building open to visitors during the time of their research and attended and documented special programs including plays, lectures, and backstage demonstrations that occurred regularly during their fieldwork (p. 14). They also conducted extensive work in Colonial Williamsburg archives to get a sense of how the museum and institutional culture had changed over time.

To give readers an example of how one might apply ethnographic research methods to a museum, I suggest we take a closer look at how Handler and Gable approached the educational mission of the museum. Because Colonial Williamsburg relies in part on costumed, first-person interpreters to convey much of its message, the authors began by attending a three-week training session designed and implemented by curators, historians, and educators at Colonial Williamsburg for new historical interpreters (Handler & Gable, 1997, pp. 11, 80–81). They took copious fieldnotes on these sessions, paying attention to the narratives, artifacts, and reconstructions that new hires were encouraged to utilize in conveying these themes to visitors and the historical documentation that underpinned these tools (p. 80). At these sessions, senior staff also described the challenges various artifacts and reconstructions posed to effective interpretation and gave an overview of typical visitors' questions (p. 80).

Handler and Gable followed their participant observation of the training sessions with interviews with the historians who created the training programs and chose the themes the institution sought to convey to visitors (Handler & Gable, 1997, p. 12). Through these interviews, as well as through content analysis of works published by historians and other scholars employed at Colonial Williamsburg, Handler and Gable came to understand that those involved in research at the museum sought to convey the notion that history is constructed, shaped by ideology, and more than just the accumulation of facts about the past. The historians wanted to reshape the history presented at Williamsburg by presenting the past not as a storehouse for moral precepts transferable to the present day, but as a laboratory for critically examining social relationships (p. 67). The authors also used archival data and interviews with past employees to compare contemporary training techniques and interpreter approaches to those of the past and to get a sense of how the museum's messages had changed over time (p. 67). Later, the team joined

visitors and interpreters on tours, recording them for later transcription and coding. This allowed the authors to compare the stories that Colonial Williamsburg historians identified in interviews as central to the museum's message to those upper level staff used in training sessions and, ultimately, to those historical interpreters delivered to visitors. In this way, Handler and Gable were able to examine how messages changed as they moved through development to visitors. It also allowed them to tease out the social factors that contributed to the transformations they witnessed. Through participant observation on tours and interviews with the historical interpreters, Handler and Gable found that the historical interpreters, who were responsible for the direct transmission of messages to visitors, saw history very differently than the historians at Colonial Williamsburg. The interpreters saw history as a puzzle to be reconstructed with continuing new discoveries (p. 70). Most transmitted to visitors the view that, as new information becomes available, our understanding of the past thus becomes fuller, richer, and more accurate, a view of history that is mimetic rather than constructive (p. 70). In this way, Handler and Gable demonstrated that the constructivist view of history embraced by scholars and curators at Colonial Williamsburg, an approach that was evident in their interviews as well as in written works they published, was severely undermined in practice by what Handler and Gable describe as a "just the facts" approach taken by historical interpreters (p. 78).

What was the origin of this disconnect? In their participant observation at training sessions, Handler and Gable found at least a partial explanation for why this was the case: in training sessions, new interpreters received prepackaged primary and secondary sources without distinction or discussion of the process of selection and interpretation that went into their creation (Handler & Gable, 1997, p. 83). In short, these materials, themselves a product of selection and interpretation, were presented to them as objective facts (p. 83). Moreover, trainees were encouraged to use these materials to discover historical significance and taught that this is how curators and historians conduct their research. To wit, the authors offer the following example:

In 1990, the outbuildings at the Wythe House were furnished to tell a particular story about slavery chosen by the foundation's historians... During a training session for the Wythe House, trainees were told to investigate the slave quarters, which they

explored for about fifteen minutes. On reconvening, they were asked to figure out who lived in each room and what their lives were like. In response to leading questions from the trainers, these trainees "discovered" historical truth by induction from the artifacts. But they were never reminded that the rooms had been set up by historians and curators to tell precisely the story they had discovered. In other words, in this training session deduction was masked as induction, as trainees mimicked professional scholars and thereby learned that such experts arrive at the stories they tell by a objective process of induction from the facts at hand. (Handler & Gable, 1997, pp. 80–81)

This understanding of historical research and why history changes, along with the desire to ward off questions about anachronisms from visitors, led to what Handler and Gable called a "just the facts" approach to historical interpretation (Handler & Gable, 1997, pp. 81–83). Rather than focusing on social history and the themes identified by the research staff as central to the Colonial Williamsburg story, interpreters viewed their work as presenting objective facts to their audience (pp. 81–83). This led many interpreters to emphasize changes in the minutia of everyday life, point out inaccuracies of the past to visitors, and then explain how these errors were rectified with new data to make the site more authentic. For instance, some interpreters made a point of telling visitors that scholars used to believe that "sweetbread" referred to the brain of an animal, but historical research had revealed that instead these are glands (p. 76). Clearly, while such comments may render historical interpretation more visible they hardly make it seem relevant to the present and so are unlikely to alter visitors' understandings of how the inequalities of the past shape contemporary social relationships. As Handler and Gable note, "This emphasis on recreating the past 'as it really was' for visitors through the minute details of material life overwhelms critical social history as well as the notion that history itself is a construct" (p. 222). Moreover, Handler and Gable conclude that, "Mimetic realism, the reigning historiographical philosophy at Colonial Williamsburg, destroys history" in that it renders the interpretive work of museums invisible. In doing so, "mimetic realism destroys history's utility as a tool for social criticism by limiting its ability to teach critical thinking about differences in social rules" (p. 224).

Another book that applies ethnographic fieldwork to the museum experience is Jackie Feldman's

Above the Death Pits, Beneath the Flag: Youth Voyages to Poland and the Performance of Israeli National Identity, which examines the construction of Israeli student tours of Poland's Holocaust sites (2008). Feldman focused on excursions organized by the Israeli Ministry of Education, the largest and most influential organizers of such trips (p. 21). As part of his fieldwork Feldman enrolled in a course that prepared teachers to guide Israeli school groups in Poland; guided four tours to Poland over a three-year period himself; shadowed groups guided by others; and conducted interviews and follow-up interviews with students, teachers, and other guides who took part in the trips (pp. 21–24). He also persuaded some of the student participants to keep and share with him trip journals. Throughout his research, Feldman took fieldnotes and transcribed interviews, coding them for patterns and key themes that he then analyzed. Because the tours do little to encourage Israeli groups to interact with Poles, Feldman did not interview Polish museum professionals at the sites the tours visited. As a result, the book could be read as a sort of visitor response. However, as Feldman compellingly illustrates, the Israeli students' experiences were shaped less by the museums and memorial sites themselves than by the tours and, by extension, the State of Israel, which organizes, sponsors, and partially funds the trips.

Of particular interest to museum studies scholars is Feldman's analyses of both the ways students are prepared for the trip in their schools prior to departure and how they are encouraged to process their experiences on their return. These passages demonstrate the influence of prior knowledge, experience, and framing to shape visitors' experiences. Feldman's analyses of the ceremonies that tour groups perform at Holocaust sites are also persuasive and offer us some insights into how we might study the ways visitors interact with museums and memorial sites.

One such example he offers is the ceremony at Block 27 at Auschwitz, which, although not required by the Ministry of Education, has become a standard part of most Israeli students' tours (Feldman, 2008, p. 204). Most of the ceremonies that are part of the tour feature what Feldman describes as a strong state presence in that, typically, students raise the Israeli flag, read aloud poems with themes of sacrifice and victory, and sing the national anthem before leaving the site. This is not the pattern followed at the Block 27 ceremony. Instead, students gather on the ground floor of Block 27, a darkened exhibition area at Auschwitz I, to read the names

of family and friends killed in the Holocaust, light candles, and say Kaddish (pp. 205–208). Feldman describes both the ceremony and the exhibit at Block 27 in detail, then considers why it works, why students are moved and why they find it so memorable weeks and months after their return (pp. 204–208). He begins with a consideration of the exhibition in Block 27, which is a darkened space lit only by memorial candles left by visitors and from a glass panel set into the floor through which visitors can see stone slabs, ashes of Holocaust victims, and a metal Star of David (p. 205). The only text in the room is "And the Lord spoke to Cain, the voice of your brother's blood cries out to me from the earth," which is mounted on the wall and spelled out in metal letters in Hebrew and Polish (p. 205). Students enter and sit on the floor around the illuminated panels, which he points out is akin to sitting around an open tomb or sitting *shiva* (p. 205). Thus, the experience is visually and bodily connected to the traditional rites of mourning in Judaism. Feldman suggests that the darkness and the manner in which the Block is constructed create a space where students can cry openly "away from the critical gaze of their peers," while the reading of the names, lighting candles, and the singing of the song "Every Person Has a Name" reify the presence of individual death and personalize it (p. 206). Unlike other parts of the tour, the State of Israel has little presence in this particular ceremony. For example, rather than singing the national anthem, as is typical before leaving a site, the students simply return quietly to their buses at the conclusion of the ceremony (p. 206).

Drawing on his interviews with student participants and tour guides, Feldman demonstrates that it is this ritual that transforms the students from onlookers to an "extended family of mourners" (p. 208). Although his research does not include interviews with Auschwitz Museum personnel or other information that might give us insight into who designed the space, why it was arranged in this way, or how other groups of visitors interact with this space, it seems safe to assume that Block 27 was designed for reflection and mourning. What Feldman's research gives us then, is an example of how the Israeli tour groups use the space to do just that and the things they do to make it meaningful to their own lives and experiences.

Analysis and Write-Up

Like the pieces from which I have drawn case studies, museum studies projects in the social

sciences are typically written up as articles, chapters in edited volumes, and, occasionally, as monographs. Limitations on length are the determining factor in the extent to which various aspects of the methodology are covered in the final write-up. However, four key issues must be addressed in any methods section. The first of these is research purpose: what were the primary research questions, how did the researcher go about answering them, where and with whom, and why. This is the place to discuss why one has focused on a particular exhibit or institution and who one's key informants were, if interviews or participant observation has been part of the research process. The second theme that must be addressed in any strong methodology section is methodological decision making; in other words, how did the research questions change as a result of new findings, and how were new questions developed in response to findings? Were new methods developed, or were previous ones adjusted to answer them? The third concern is to explain the arrangements one has made with museums and informants, whether employees or visitors, to ensure that one's work is conducted in an ethical way. Finally, in a methods section, it is important to describe the procedures one used to analyze research.

Although any write-up of museum studies research will present an analysis of the data collected, fields within the social sciences differ considerably in the extent to which the procedures used for analysis are described in the final product. Authors with a background in cultural anthropology rarely formally discuss the process they go through to transform fieldnotes and interview transcripts into a final, publishable piece of work. Qualitative sociologists, in contrast, often devote pages to the topic, outlining the specific codes they used to discern the patterns they then describe and discuss in their results. Although the first approach can leave the novice who is seeking to understand the process mystified, the latter can rapidly begin to feel redundant, particularly to the reader who is more interested in the exhibit or museum than in the intricacies of the process the researcher went through to understand it. This does leave authors quite a bit of leeway in writing their methods sections, so I suggest taking one's cue on this point from other pieces published in the journal to which one is submitting one's work or from the editors of the volume.

Disciplinary differences in writing styles aside, the procedure for analysis is the same. Researchers take their fieldnotes collected in galleries, at museum events, in meetings, and the like and look

for patterns. Following transcription, interviews are typically handled in a similar fashion. It is relatively rare for interviews to be presented in transcription form without analysis, although this is the case in the piece I discussed by Daniel Sherman and Ilona Katzew. More typical by far is for the researcher to conduct a number of interviews with museum workers or visitors and look for themes in the interview transcripts, then to select and present examples that illustrate pertinent points. This is the approach taken in the second piece I described by John Falk and Lynn Dierking, in their research with museum visitors. What specifically one looks for will depend on the questions one has posed, but, as patterns emerge, the point is to note them. There are a variety of ways one can do this, and many researchers facilitate the process with the use of software that helps them locate such passages quickly and extract and organize them with memo notes as to how they might contribute to the final write-up. These patterns and notes then become the basis of one's final write-up.

To give an example from a project I am working on at a museum in Warsaw, while mapping the permanent exhibit, I noted that there was a large steel sculpture about 2 meters wide in the center of the museum. It is inscribed with the dates of every day of the sixty-three-day uprising, to which the museum is devoted, like so: 1 VIII, 2 VIII, . . . , and at the top with the symbol of the Polish Home Army, which is a P with a split bottom that forms a W, signifying the slogan "*Polska Walczaca*" or Fighting Poland. The structure is covered in what look like simulated bullet holes, and it rises from the first floor to transect the mezzanine and the third floor. During the course of my research, I noticed that visitors could cover various bullet holes to produce a variety of sounds including gunfire, bombs dropping, broadcasts from wartime underground radio stations, songs, and what sounded like prayers. I noted this, too, and wrote the following questions, "What would it be like to have lived through the uprising and to hear these sounds in a museum setting?" "Might ex-combatants and other survivors find this disturbing?" Later, following a tour of the museum with a representative of the historical research division, I noted, "G took time to point out the museum's 'heart,' the big wall running through the middle that literally beats like a human heart—whose heart beat did they use for this, I wonder?" Because my tour had ended just as the museum was closing and the first floor was virtually empty, I noticed for the first time that the sculpture

was emitting a pulse that provided a sort of undertone for the entire first floor of the exhibit space.

In subsequent interviews with other museum employees, I was frequently asked “Did you see the heart of the museum?” It took me a while to realize that when museum personnel spoke about “the heart of the museum,” they were not talking about its ethos, or ideology, or even of an exhibit that they saw as central to the museum’s message, but about the sculpture with a pulse. Over time, I came to realize that this “heart” is a point of pride for museum personnel and that they regard the piece as a monument. Indeed, on the latest version of the museum website, the structure is referred to as a monument, “a symbol of our memory and a tribute to the Warsaw Uprising and its participants” (http://www.1944.pl/o_muzeum/ekspozycja/parter/5_monument/?q=Serca). Interestingly, although the text refers to the structure as the “monument,” I found this part of the site with the search term *serca* (heart). Because this is an ongoing project, I code my interviews and fieldnotes for references to the “heart,” whether it’s employed to refer to the monument or used in reference to more abstract ideas about the museum’s mission and work. Eventually, when I write up my research, I will draw on these data about the heart to talk about the personnel’s conceptualizations of their own work at the museum.

Conclusion

Far from exhaustive, I offer the methods I have outlined and explored in the preceding pages as a starting point. Gallery analyses, interviews, and participant observation, used separately or in tandem with one another, are a solid base with which to start one’s study of an exhibition, program, or a museum as an institution. These methods might also be applied to areas of museum work that are not as yet well represented in the literature. For example, many museums offer classes that make use of their collections to visitors, and many more have kindergarten through twelfth-grade outreach programs or generate curricular materials that supplement a variety of subjects that are part of state teaching standards. The study of such formal educational programs and curricular materials offered by museums to visitors, schools, and homeschoolers would be a worthwhile project best accomplished through a combination of content analysis and interviews with museum educators, classroom teachers, and the students they serve, as well as through participant observation in the learning environment, whether that be field

trips to museums or the use of museum-generated materials in the classroom.

One could approach a museum website as an online exhibit and apply the same methods of gallery analysis to examine how museums use technology and social media to connect with physical and virtual visitors. As with any content analysis, this is an approach that pairs well with interviews to arrive at a fuller understanding of how visitors understand museum-generated sites and social media and make use of them in their daily lives. Such an approach might help museums and those who study them understand ways that museums create communities.

My hope is that this chapter will spark more thinking about museums, the learning that takes place within them, and the knowledge they create and circulate, as well as the communities they serve and create. May it also generate new ideas about how to go about studying them.

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Text, Arts-Based, and
Internet Methods



Lindsay Prior

Abstract

In this chapter, the focus is on ways in which content analysis can be used to investigate and describe interview and textual data. The chapter opens with a contextualization of the method and then proceeds to an examination of the role of content analysis in relation to both quantitative and qualitative modes of social research. Following the introductory sections, four kinds of data are subjected to content analysis. These include data derived from a sample of qualitative interviews (N = 54), textual data derived from a sample of health policy documents (N = 6), data derived from a single interview relating to a “case” of traumatic brain injury, and data gathered from 54 abstracts of academic papers on the topic of “well-being.” Using a distinctive and somewhat novel style of content analysis that calls upon the notion of semantic networks, the chapter shows how the method can be used either independently or in conjunction with other forms of inquiry (including various styles of discourse analysis) to analyze data, and also how it can be used to verify and underpin claims that arise out of analysis. The chapter ends with an overview of the different ways in which the study of “content”—especially the study of document content—can be positioned in social scientific research projects.

Key Words: content analysis, discourse analysis, documents, interviews, narrative analysis, case study, semantic networks

What is Content Analysis?

In his 1952 text on the subject of content analysis, Bernard Berelson traces the origins of the method to communication research and then lists what he calls six distinguishing features of the approach. As one might expect, the six defining features reflect the concerns of social science as taught in the 1950s, an age in which the calls for an “objective,” “systematic,” and “quantitative” approach to the study of communication data were first heard. The reference to the field of “communication” was of course nothing less than a reflection of a substantive social scientific interest over the previous decades in what was called public opinion, and specifically attempts to understand why and how a potential of source of critical, rational judgement on political leaders (i.e., the views of the public) could be turned

into something to be manipulated by dictators and demagogues. In such a context, it is perhaps not so surprising that in one of the more popular research methods texts of the decade, the terms content analysis and communication analysis are used interchangeably (see Goode & Hatt, 1952:325).

Academic fashions and interests naturally change with available technology, and these days we are more likely to focus on the individualization of communications through Twitter and the like, rather than of mass newspaper readership or mass radio audiences, yet the prevailing discourse on content analysis has remained much the same as it was in Berelson’s day. Thus Neuendorf (2002:1), for example, continues to define content analysis as “the systematic, objective, quantitative analysis of message characteristics.” Clearly the centrality of

communication as a basis for understanding and using content analysis continues to hold, but in this article I will try to show that, rather than locate the use of content analysis in disembodied “messages” and distanced “media,” we would do better to focus on the fact that communication is a building block of social life itself and not merely a system of messages that are transmitted—in whatever form—from sender to receiver. To put that statement in another guise, we need to note that communicative action (to use the phraseology of Habermas, 1987) rests at the very base of the lifeworld, and one very important way of coming to grips with that world is to study the content of what people say and write in the course of their everyday lives.

My aim is to demonstrate various ways in which content analysis (henceforth CTA) can be used and developed to analyze social scientific data as derived from interviews and documents. It is not my intention to cover the history of CTA or to venture into forms of literary analysis or to demonstrate each and every technique that has ever been deployed by content analysts. (Many of the standard textbooks deal with those kinds of issues much more fully than is possible here. See, for example, Babbie, 2013; Berelson, 1952; Bryman, 2008, Krippendorf, 2004; Neuendorf, 2002; and Weber, 1990). Instead I seek to recontextualize the use of the method in a framework of network thinking and to link the use of CTA to specific problems of data analysis. As will become evident, my exposition of the method is grounded in real world problems. Those problems are drawn from my own research projects and tend to reflect my particular academic interests—which are almost entirely related to the analysis of the ways in which people talk and write about aspects of health, illness, and disease. However, lest the reader be deterred from going any further, I should emphasize that the substantive issues that I elect to examine are secondary if not tertiary to my main objective—which is to demonstrate how CTA can be integrated into a range of research designs and add depth and rigour to the analysis of interview and inscription data. To that end, in the next section I aim to clear our path to analysis by dealing with some issues that touch on the general position of CTA in the research armory, and especially its location in the schism that has developed between quantitative and qualitative modes of inquiry.

The Methodological Context of Content Analysis

Content analysis is usually associated with the study of inscription contained in published reports,

newspapers, adverts, books, web pages, journals, and other forms of documentation. Hence, nearly all of Berelson’s (1952) illustrations and references to the method relate to the analysis of written records of some kind, and where speech is mentioned it is almost always in the form of broadcast and published political speeches (such as State of the Union addresses). This association of content analysis with text and documentation is further underlined in modern textbook discussions of the method. Thus Bryman (2008) for example, defines content analysis as “an approach to the analysis of documents and *texts*, that seek to quantify content in terms of pre-determined categories” (2008:274, emphasis in original), while Babbie (2013) states that content analysis is “the study of recorded human communications” (2013:295), and Weber refers to it as a method to make “valid inferences from text” (1990:9). It is clear then that CTA is viewed as a text-based method of analysis, though extensions of the method to other forms of inscriptional material are also referred to in some discussions. Thus Neuendorf (2002), for example, rightly refers to analyses of film and television images as legitimate fields for the deployment of CTA, and by implication analyses of still—as well as moving—images such as photographs and billboard adverts. Oddly, in the traditional or standard paradigm of content analysis, the method is solely used to capture the “message” of a text or speech; it is not used for the analysis of a recipient’s response to or understanding of the message (which is normally accessed via interview data and analyzed in other and often less rigorous ways; see, e.g., Merton, 1968). So in this article I suggest that we can take things at least one small step further by using CTA to analyse speech (especially interview data) as well as text.

Standard textbook discussions of CTA usually refer to it as a “non-reactive” or “unobtrusive” method of investigation (see, e.g., Babbie, 2013:294), and a large part of the reason for that designation is due to its focus on already existing text (i.e., text gathered without intrusion into a research setting). More importantly, however, (and to underline the obvious) CTA is primarily a method of *analysis* rather than of data collection. Its use therefore has to be integrated into wider frames of research design that embrace systematic forms of data collection as well as forms of data analysis. Thus routine strategies for sampling data are often required in designs that call upon CTA as a method of analysis. These latter can either be built around random sampling methods, or even techniques of

“theoretical sampling” (Glaser & Strauss, 1967) so as to identify a suitable range of materials for content analysis. CTA can also be linked to styles of ethnographic inquiry and to the use of various purposive or non-random sampling techniques. For an example, see Altheide (1987).

Of course, the use of CTA in a research design does not preclude the use of other forms of analysis in the same study, for it is a technique that can be deployed in parallel with other methods or with other methods sequentially. For example, and as I will demonstrate in the following sections, one might use CTA as a preliminary analytical strategy to get a grip on the available data before moving into specific forms of discourse analysis. In this respect it can be as well to think of using CTA in, say, the frame of a priority/sequence model of research design as described by Morgan (1998).

As I shall explain, there is a sense in which content analysis rests at the base of all forms of qualitative data analysis, yet the paradox is that the analysis of content is usually considered to be a quantitative (numerically based) method. In terms of the qualitative/quantitative divide, however, it is probably best to think of CTA as a hybrid method, and some writers have in the past argued that it is necessarily so (Kracauer, 1952). That was probably easier to do in an age when many recognised the strictly drawn boundaries between qualitative and quantitative styles of research to be inappropriate. Thus in their widely used text on *Methods in Social Research*, Goode and Hatt (1952:313), for example, asserted that, “[M]odern research must reject as a false dichotomy the separation between ‘qualitative’ and ‘quantitative’ studies, or between the ‘statistical’ and the ‘non-statistical’ approach.” It was a position advanced on the grounds that all good research must meet adequate standards of validity and reliability whatever its style, and it is a message well worth preserving. However, there is a more fundamental reason why it is nonsensical to draw a division between the qualitative and the quantitative. It is simply this: all acts of social observation depend on the deployment of qualitative categories—whether gender, class, race, or even age; there is no descriptive category in use in the social sciences that connects to a world of “natural kinds.” In short, all categories are made, and therefore when we seek to count “things” in the world, we are dependent on the existence of socially constructed divisions. How the categories take the shape that they do—how definitions are arrived at, how inclusion and exclusion criteria are decided upon, and

how taxonomic principles are deployed—constitute interesting research questions in themselves. From our starting point, however, we need only note that “sorting things out” (to use a phrase from Bowker & Star, 1999) and acts of “counting”—whether it be of chromosomes or people (Martin and Lynch, 2009)—are activities that connect to the social world of organized interaction rather than to unmediated observation of the external world.

Of course, some writers deny the strict division between the qualitative and quantitative on grounds of empirical practice rather than of ontological reasoning. For example, Bryman (2008) argues that qualitative researchers also call upon quantitative thinking but tend to use somewhat vague, imprecise terms rather than numbers and percentages—referring to frequencies via the use of phrases such as “more than” and “less than.” Kracauer (1952) advanced various arguments against the view that CTA was strictly a quantitative method, suggesting that very often we wished to assess content as being negative or positive with respect to some political, social, or economic thesis and that such evaluations could never be merely statistical. He further argued that we often wished to study “underlying” messages or latent content of documentation and that in consequence we needed to interpret content as well as count items of content. Morgan (1993) has argued that, given the emphasis that is placed on “coding” in almost all forms of qualitative data analysis, the deployment of counting techniques is essential and that we ought therefore to think in terms of what he calls qualitative as well as quantitative content analysis. Naturally, some of these positions create more problems than they seemingly solve (as is the case with considerations of “latent content”), but given the twentieth-first-century predilection for “mixed-methods” research (Creswell, 2007), it is clear that CTA has a role to play in integrating quantitative and qualitative modes of analysis in a systematic rather than merely an ad hoc and piecemeal fashion. In the sections that follow, I will provide some examples of the ways in which “qualitative” analysis can be combined with systematic modes of counting. First, however, we need to focus on what is analyzed in CTA.

Units of Analysis

So what is the unit of analysis in CTA? A brief answer to that question is that analysis can be focused on words, sentences, grammatical structures, tenses, clauses, ratios (of say, nouns to verbs), or even “themes.” Berelson (1952) gives some

examples of all of the above and also recommends a form of thematic analysis (c.f., Braun and Clarke, 2006) as a viable option. Other possibilities include counting column length (of speeches and newspaper articles), amounts of (advertising) space, or frequency of images. For our purposes, however, it might be useful to consider a specific (and somewhat traditional) example. Here it is. It is an extract from what has turned out to be one of the most important political speeches of the current century.

Iraq continues to flaunt its hostility toward America and to support terror. The Iraqi regime has plotted to develop anthrax and nerve gas and nuclear weapons for over a decade. This is a regime that has already used poison gas to murder thousands of its own citizens, leaving the bodies of mothers huddled over their dead children. This is a regime that agreed to international inspections then kicked out the inspectors. This is a regime that has something to hide from the civilized world. States like these, and their terrorist allies, constitute an axis of evil, arming to threaten the peace of the world. By seeking weapons of mass destruction, these regimes pose a grave and growing danger. They could provide these arms to terrorists, giving them the means to match their hatred. They could attack our allies or attempt to blackmail the United States. In any of these cases, the price of indifference would be catastrophic." —George W. Bush, State of the Union address, January 29, 2002

A number of possibilities arise for analysing the content of a speech such as the one above. Clearly, words and sentences must play a part in any such analysis, but in addition to words there are structural features of the speech that could also figure. For example, the extract takes the form of a simple narrative—pointing to a past, a present, and an ominous future (catastrophe)—and could therefore be analysed as such. There are, in addition, a number of interesting oppositions in the speech (such as those between “regimes” and the “civilised” world), as well as a set of interconnected present participles such as “plotting,” “hiding,” “arming,” and “threatening” that are associated both with Iraq and with other states that “constitute an axis of evil.” Evidently, simple word counts would fail to capture the intricacies of a speech of this kind. Indeed, our example serves another purpose—to highlight the difficulty that often arises in dissociating content analysis from discourse analysis (of which narrative analysis and the analysis of rhetoric and trope are subspecies). So how might we deal with these problems?

One approach that can be adopted is to focus on what is referenced in text and speech. That is, to concentrate on the characters or elements that are recruited into the text and to examine the ways in which they are connected or co-associated. I shall provide some examples of this form of analysis shortly. Let us merely note for the time being that in the previous example we have a speech in which various “characters”—including weapons in general, specific weapons (such as nerve gas), threats, plots, hatred, evil and mass destruction—play a role. Be aware that we need not be concerned with the veracity of what is being said—whether it is true or false—but simply with what is in the speech and how what is in there is associated. (We may leave the task of assessing truth and falsity to the jurists). Be equally aware that it is a text that is before us and not an insight into the ex-President’s mind, nor his thinking, nor his beliefs, nor any other subjective property that he may have possessed.

In the introductory paragraph, I made brief reference to some ideas of the German philosopher Jürgen Habermas (1987). It is not my intention here to expand on the detailed twists and turns of his claims with respect to the role of language in the “lifeworld” at this point. However, I do intend to borrow what I regard as some particularly useful ideas from his work. The first, is his claim—influenced by a strong line of twentieth-century philosophical thinking—that language and culture are constitutive of the lifeworld (1987:125), and in that sense we might say that things (including individuals and societies) are made in language. That of course is a simple justification for focusing on what people *say* rather than what they “think” or “believe” or “feel” or “mean” (all of which have been suggested at one time or another as points of focus for social inquiry and especially qualitative forms of inquiry). Second, Habermas argues that speakers and therefore hearers (and one might add writers and therefore readers), in what he calls their speech acts, necessarily adopt a pragmatic relation to one of three worlds: entities in the objective world, things in the social world, and elements of a subjective world. In practice, Habermas (1987:120) suggests all three worlds are implicated in any speech act but that there will be a predominant orientation to one of these. To rephrase this in a crude form, when speakers engage in communication, they refer to things and facts and observations relating to external nature, to aspects of interpersonal relations, and to aspects of private inner subjective worlds (thoughts, feelings, beliefs, etc.). One of the problems with

locating CTA in “communication research” has been that the communications referred to are but a special and limited form of action (often what Habermas would call strategic acts). In other words, television, newspaper, video, and internet communications are just particular forms (with particular features) of action in general. Again we might note in passing that the adoption of the Habermasian perspective on speech acts implies that much of qualitative analysis in particular has tended to focus only on one dimension of communicative action—the subjective and private. In this respect, I would argue that it is much better to look at speeches such as George W Bush’s 2002 State of the Union address as an “account” and to examine what has been recruited into the account; and how what has been recruited is connected or co-associated rather than to use the data to form insights into his (or his adviser’s) thoughts, feelings, and beliefs.

In the sections that follow, and with an emphasis on the ideas that I have just expounded, I intend to demonstrate how CTA can be deployed to advantage in almost all forms of inquiry that call upon either interview (or speech-based) data or textual data. In my first example, I will show how CTA can be used to analyze a group of interviews. In the second example, I will show how it can be used to analyze a group of policy documents. In the third, I shall focus on a single interview (a “case”), and in the fourth and final example, I will show how CTA can be used to track the biography of a concept. In each instance, I shall briefly introduce the context of the “problem” on which the research was based, outline the methods of data collection, discuss how the data were analyzed and presented, and underline the ways in which content analysis has sharpened the analytical strategy.

Analyzing a Sample of Interviews: Looking at Concepts and Their Co-Associations in a Semantic Network

My first example of using CTA is based on a research study that was initially undertaken in the early 2000s. It was a project aimed at understanding why older people might reject the offer to be immunized against influenza (at no cost to them). The ultimate objective was to improve rates of immunization in the study area. The first phase of the research was based on interviews with 54 older people in South Wales. The sample included people who had never been immunized, some who had refused immunization, and some who had accepted immunization. Within each category, respondents

were randomly selected from primary care physician patient lists, and the data were initially analyzed “thematically” and published accordingly (Evans, Prout, Prior, et al., 2007). A few years later, however, I returned to the same data set to look at a different question—how (older) lay people talked about colds and flu, especially how they distinguished between the two illnesses and how they understood the causes of the two illnesses (see Prior, Evans, & Prout, 2011). Fortunately, in the original interview schedule, we had asked people about how they saw the “differences between cold and flu” and what caused flu, so it was possible to reanalyze the data with such questions in mind. In that frame, the example that follows demonstrates not only how CTA might be used on interview data, but also how it might be used to undertake a secondary analysis of a pre-existing data set (Bryman, 2008).

As with all talk about illness, talk about colds and flu is routinely set within a mesh of concerns—about causes, symptoms, and consequences. Such talk comprises the base elements of what has at times been referred to as the “explanatory model” of an illness (Kleinman, Eisenberg, & Good, 1978). In what follows, I shall focus almost entirely on issues of causation as understood from the viewpoint of older people; the analysis is based on the answers that respondents made in response to the question, “How do you think people catch flu?”

Semi-structured interviews of the kind undertaken for a study such as this are widely used and are often characterized as akin to “a conversation with a purpose” (Kahn & Cannell, 1957:97). One of the problems of analyzing the consequent data is that, although the interviewer holds to a planned schedule, the respondents often reflect in a somewhat unstructured way about the topic of investigation, so it is not always easy to unravel the web of talk about, say, “causes” that occurs in the interview data. In this example, causal agents of flu, inhibiting agents, and means of transmission were often conflated by the respondents. Nevertheless, in their talk people did answer the questions that were posed, and in the study referred to here, that talk made reference to things such as “bugs” (and “germs”) as well as viruses; but the most commonly referred to causes were “the air” and the “atmosphere.” The interview data also pointed toward means of transmission as “cause”—so coughs and sneezes and mixing in crowds figured in the causal mix. Most interesting perhaps was the fact that lay people made a nascent distinction between facilitating factors (such as bugs and viruses) and inhibiting factors (such as

being resistant, immune, or healthy), so that in the presence of the latter, the former are seen to have very little effect. Here are some shorter examples of typical question-response pairs from the original interview data.

(R:32): “How do you catch it [the flu]? Well, I take it its through ingesting and inhaling bugs from the atmosphere. Not from sort of contact or touching things. Sort of airborne bugs. Is that right?”

(R:3): “I suppose it’s [the cause of flu] in the air. I think I get more diseases going to the surgery than if I stayed home. Sometimes the waiting room is packed and you’ve got little kids coughing and spluttering and people sneezing, and air conditioning I think is a killer by and large I think air conditioning in lots of these offices”.

(R:46): “I think you catch flu from other people. You know in enclosed environments in air conditioning which in my opinion is the biggest cause of transferring diseases is air conditioning. Worse thing that was ever invented that was. I think so, you know. It happens on aircraft exactly the same you know.”

Alternatively, it was clear that for some people being cold, wet, or damp could also serve as a direct cause of flu; thus: Interviewer: “OK, good. How do you think you catch the flu?”

(R:39): “Ah. The 65 dollar question. Well, I would catch it if I was out in the rain and I got soaked through. Then I would get the flu. I mean my neighbour up here was soaked through and he got pneumonia and he died. He was younger than me: well, 70. And he stayed in his wet clothes and that’s fatal. Got pneumonia and died, but like I said, if I get wet, especially if I get my head wet, then I can get a nasty head cold and it could develop into flu later.”

As I suggested earlier, despite the presence of bugs and germs, viruses, the air, and wetness or dampness, “catching” the flu is not a matter of simple exposure to causative agents. Thus some people hypothesized that within each person there is a measure of immunity or resistance or healthiness that comes into play and that is capable of counteracting the effects of external agents. For example, being “hardened” to germs and harsh weather can prevent a person getting colds and flu. Being “healthy” can itself negate the effects of any causative agents,

and healthiness is often linked to aspects of “good” nutrition and diet and not smoking cigarettes. These mitigating and inhibiting factors can either mollify the effects of infection or prevent a person “catching” the flu entirely. Thus (R:45) argued that it was almost impossible for him to catch flu or cold “[c]os I got all this resistance.” Interestingly respondents often used possessive pronouns in their discussion of immunity and resistance (“my immunity” and “my resistance”)—and tended to view them as personal assets (or capital) that might be compromised by mixing with crowds.

By implication, having a weak immune system can heighten the risk of contracting cold and flu and might therefore spur one on to take preventive measures such as accepting a flu jab. There are some, of course, who believe that it is the flu jab that can cause the flu and other illnesses. An example of what might be called lay “epidemiology” (Davison, Davey-Smith, & Frankel, 1991) is evident in the following extract.

(R:4): “Well, now it’s coincidental you know that [my brother] died after the jab, but another friend of mine, about 8 years ago, the same happened to her. She had the jab and about six months later, she died, so I know they’re both coincidental, but to me there’s a pattern.”

Normally, results from studies such as this are presented in exactly the same way as has just been set out. Thus the researcher highlights given themes that are said to have emerged out of the data and then provides appropriate extracts from the interviews to illustrate and substantiate the relevant themes. However, one very reasonable question that any critic might ask about the selected data extracts concerns the extent to which they are “representative” of the material in the data set as a whole. Maybe, for example, the author has been unduly selective in his or her use of both themes and quotations. Perhaps, as a consequence, the author has ignored or left out talk that does not fit their arguments or extracts that might be considered dull and uninteresting compared to more exotic material. And these kinds of issues and problems are certainly common to the reporting of almost all forms of qualitative research. However, the adoption of CTA techniques can help to mollify such problems. This is so because by using CTA we can indicate the extent to which we have used all or just some of the data, and we can provide a view of the content of

the entire sample of interviews rather than just the content and flavor of merely one or two interviews. In this light, we need to consider Figure 18.1. The figure is based on counting the number of references in the 54 interviews to the various “causes” of the flu, though references to the flu jab (i.e., inoculation) as a cause of flu have been ignored for the purpose of this discussion). The node sizes reflect the relative importance of each cause as determined by the concept count (frequency of occurrence). The links between nodes reflect the degree to which causes are co-associated in interview talk and are calculated according to a co-occurrence index (see, e.g., SPSS, 2007:183).

Given this representation, we can immediately assess the relative importance of the different causes as referred to in the interview data. Thus we can see that such things as (poor) “hygiene” and “foreigners” were mentioned as a potential cause of flu—but mention of hygiene and foreigners was nowhere near so important as references to “the air” or to “crowds” or to “coughs and sneezes.” In addition, we can also determine the strength of the connections that

interviewees made between one cause and another. Thus there are relatively strong links between “resistance” and “coughs and sneezes,” for example.

In fact, Figure 18.1 divides causes into the “external” and the “internal,” or the facilitating and the impeding (lighter and darker nodes). Among the former I have placed such things as crowds, coughs, sneezes, and the air while among the latter I have included “resistance,” “immunity,” and “health.” That division, of course, is a product of my conceptualizing and interpreting the data, but whichever way we organize the findings, it is evident that talk about the causes of flu belongs in a web or mesh of concerns that would be difficult to represent by the use of individual interview extracts alone. Indeed, it would be impossible to demonstrate how the semantics of causation belong to a culture (rather than to individuals) in any other way. In addition I would argue that the counting involved in the construction of the diagram functions as a kind of check on researcher interpretations and provides a source of visual support for claims that an author might make about, say, the relative importance of “damp”

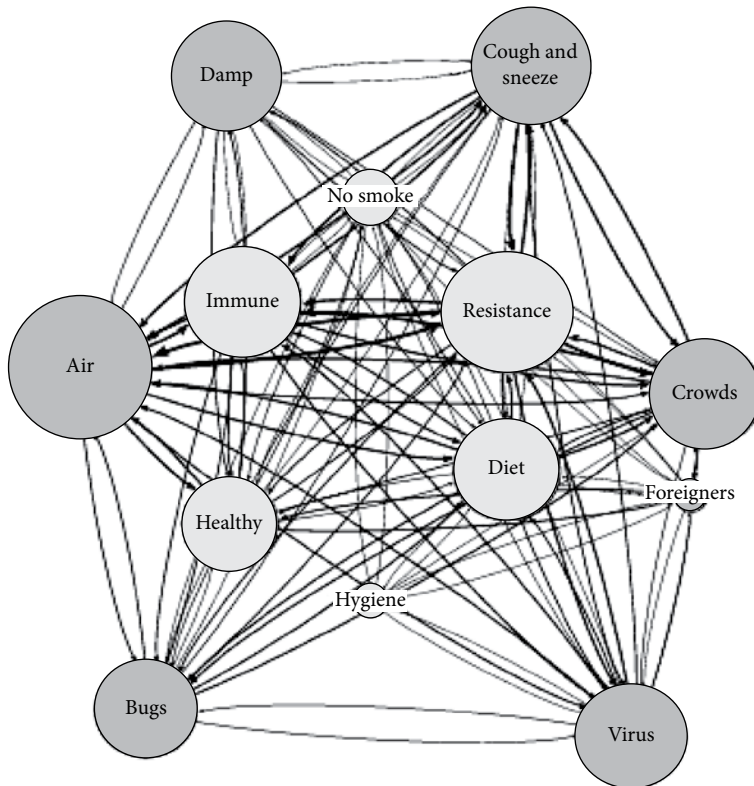


Figure 18.1 What causes flu? A lay perspective. Factors listed as causes of colds and flu in 54 interviews. Node size is proportional to number of references “as causes.” Line thickness is proportional to co-occurrence of any two “causes” in the set of interviews.

Source: Prior et al. (2011).

and “air” as perceived causes of disease. Finally, the use of CTA techniques allied with aspects of conceptualization and interpretation has enabled us to approach the interview data as a set and to consider the respondents as belonging to a community rather than regarding them merely as isolated and disconnected individuals, each with their own views. It has also enabled us to squeeze some new findings out of old data, and I would argue that it has done so with advantage. There are of course other advantages to using CTA to explore data sets, which I highlight in the next section.

Analyzing a Sample of Documents: Using Content Analysis to Verify Claims

Policy analysis is a difficult business. For a start, it is never entirely clear where (social, health, economic, environmental) policy actually is. Is it in documents (as published by governments, think tanks, and research centres), in action (what people actually do), or in speech (what people say)? Perhaps it rests in a mixture of all three realms. Yet wherever it may be, it is always possible, at the very least, to identify a range of policy texts and to focus on the conceptual or semantic webs in terms of which government officials and other agents (such as politicians) talk about the relevant policy issues. Furthermore, in so far as policy is recorded—in speeches, pamphlets, and reports—we may begin to speak of specific policies as having a history or a pedigree that unfolds through time (think, e.g., of US or UK health policies during the Clinton years or the Obama years). And in so far as we consider “policy” as having a biography or a history, we can also think of studying policy narratives.

Though firmly based in the world of literary theory, narrative method has been widely used for both the collection and the analysis of data concerning ways in which individuals come to perceive and understand various states of health, ill health, and disability (Frank, 1995; “Hydén, 1997). Narrative techniques have also been adapted for use in clinical contexts and allied to concepts of healing (Charon, 2006). In both social scientific and clinical work, however, the focus is invariably on individuals and on how individuals “tell” stories of health and illness. Yet narratives can also belong to collectives—such as political parties and ethnic and religious groups—just as much as to individuals, and in the latter case there is a need to collect and analyse data that are dispersed across a much wider range of materials than can be obtained from the personal interview.

In this context, Roe (1994) has demonstrated how narrative method can be applied to an analysis of national budgets, animal rights, and environmental policies.

An extension of the concept of narrative to policy discourse is undoubtedly useful (Newman & Vidler, 2006), but how might such narratives be analyzed? What strategies can be used to unravel the form and content of a narrative, especially in circumstances where the narrative might be contained in multiple (policy) documents, authored by numerous individuals, and published across a span of time rather than in a single, unified text such as a novel? Roe (1994), unfortunately, is not in any way specific about analytical procedures apart from offering the useful rule to “never stray too far from the data” (1994:xii). So in this example I will outline a strategy for tackling such complexities. In essence, it is a strategy that combines techniques of linguistically (rule) based content analysis with a theoretical and conceptual frame that enables us to unravel and identify the core features of a policy narrative. My substantive focus is on documents concerning health service delivery policies published 2000–2009 in the constituent countries of the UK (that is, England, Scotland, Wales, and Northern Ireland—all of which have different political administrations).

Narratives can be described and analyzed in various ways, but for our purposes we can say that they have three key features: they point to a chronology, they have a plot and they contain “characters.”

Chronology: All narratives have beginnings; they also have middles and endings, and these three stages are often seen as comprising the fundamental structure of narrative text. Indeed, in his masterly analysis of time and narrative, Ricoeur (1984) argues that it is in the unfolding chronological structure of a narrative that one finds its explanatory (and not merely descriptive) force. By implication, one of the simplest strategies for the examination of policy narratives is to locate and then divide a narrative into its three constituent parts—beginning, middle, and end.

Unfortunately, while it can sometimes be relatively easy to locate or choose a beginning to a narrative, it can be much more difficult to locate an end point. Thus in any illness narrative, a narrator might be quite capable of locating the start of an illness process (in an infection, accident, or other event) but unable to see how events will be resolved in an ongoing and constantly unfolding life. As a consequence, both narrators and researchers usually find themselves in the midst of an emergent present—a

present without a known and determinate end (see, e.g., Frank, 1995). Similar considerations arise in the study of policy narratives where chronology is perhaps best approached in terms of (past) beginnings, (present) middles, and projected futures.

Plot: According to Ricoeur (1984), our basic ideas about narrative are best derived from the work and thought of Aristotle who in his *Poetics* sought to establish “first principles” of composition. For Ricoeur, as for Aristotle, plot ties things together. It “brings together factors as heterogeneous as agents, goals, means, interactions, circumstances, unexpected results” (1984:65) into the narrative frame. For Aristotle, it is the ultimate untying or unraveling of the plot that releases the dramatic energy of the narrative.

Character: Characters are most commonly thought of as individuals, but they can be considered in much broader terms. Thus the French semiotician A. J. Greimas (1970), for example, suggested that, rather than think of characters as people, it would be better to think in terms of what he called “actants” and of the functions that such actants fulfill within a story. In this sense geography, climate, and capitalism can be considered as characters every bit as much as aggressive wolves and Little Red Riding Hood. Further, he argued that the same character (actant) can be considered to fulfill many functions and the same function performed by many characters. Whatever else, the deployment of the term actant certainly helps us to think in terms of narratives as functioning and creative structures. It also serves to widen our understanding of the ways in which concepts, ideas, and institutions, as well “things” in the material world can influence the direction of unfolding events every bit as much as conscious human subjects. Thus, for example, the “American people,” “the nation,” “the constitution,” “the West,” “tradition,” and “Washington” can all serve as characters in a policy story.

As I have already suggested, narratives can unfold across many media and in numerous arenas—speech and action, as well as text. Here, however, my focus is solely on official documents—all of which are UK government policy statements as listed in Table 18.1. The question is how might CTA help us unravel the narrative frame?

It might be argued that a simple reading of any document should familiarize the researcher with elements of all three policy narrative components (plot, chronology, and character). However, in most policy research, we are rarely concerned with a single and unified text as is the case with a novel, but

rather with multiple documents written at distinctly different times by multiple (usually anonymous) authors that notionally can range over a wide variety of issues and themes. In the full study, some 19 separate publications were analyzed across England, Wales, Scotland, and Northern Ireland.

Naturally, to list word frequencies—still less to identify co-occurrences and semantic webs in large data sets (covering hundreds of thousand of words and footnotes)—cannot be done manually but rather requires the deployment of complex algorithms and text-mining procedures. To this end I analyzed the 19 documents using “Text Mining for Clementine” (SPSS, 2007).

Text-mining procedures begin by providing an initial list of concepts based on the lexicon of the text but which can be weighted according to word frequency and which take account of elementary word associations. For example, learning disability, mental health, and performance management indicate three concepts, not six words. Using such procedures on the aforementioned documents gives the researcher an initial grip on the most important concepts in the document set of each country. Note that this is much more than a straightforward concordance analysis of the text and is more akin to what Ryan & Bernard (2000) have referred to as “semantic analysis” and Carley (1993) has referred to as “concept” and “mapping” analysis.

Table 18.1 The document set analyzed in Figure 18.2

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1. Department of Health (2000). *The NHS plan. A plan for investment, a plan for reform*. London: Stationery Office.

 2. Department of Health (2003). *Building on the best. Choice, responsiveness and equity in the NHS*. London: Stationery Office.

 3. Department of Health (2004). *The NHS Improvement Plan. Putting people at the heart of public services*. London: Department of Health.

 4. Department of Health (2004). *Choosing health. Making healthier choices easier*. London: Stationery Office.

 5. Department of Health (2005). *Creating a patient led NHS. Delivering the NHS Improvement Plan*. London: Department of Health.

 6. Department of Health (2006). *Our Health Our Care Our Say. A new direction for community services*. London: Stationery Office.
-

So the first task was to identify and then extract the core concepts, thus identifying what might be called “key” characters or actants in each of the policy narratives. For example, in the Scottish documents such actants included “Scotland” and the “Scottish people,” as well as “health” and the “NHS,” among others; while in the Welsh documents it was “the people of Wales” and “Wales” that figured largely—thus emphasizing how national identity can play every bit as important a role in a health policy narrative as concepts such as “health,” “hospitals,” and “wellbeing.”

Having identified key concepts it was then possible to track concept clusters in which particular actants or characters are embedded. Such cluster analysis is dependent on the use of co-occurrence rules and the analysis of synonyms, whereby it is possible to get a grip on the strength of the relationships between the concepts, as well as the frequency with which the concepts appear in the collected texts. In Figure 18.2, I provide an example of a concept cluster. The diagram indicates the nature of the conceptual and semantic web in which various actants are discussed. The diagrams further indicate strong (solid line) and weaker (dotted line) connections between the various elements in any specific mix, and the numbers indicate frequency counts for the individual concepts. Using *Clementine*, the researcher is unable to specify in advance which clusters will emerge from the data. One cannot, for example, choose to have an NHS cluster. In that

respect, these diagrams not only provide an array in terms of which concepts are located, but also serve as a check on and to some extent validation of the interpretations of the researcher. Of course none of this tells us what the various narratives contained within the documents might be. They merely point to key characters and relationships both within and between the different narratives. So having indicated the techniques used to identify the essential parts of the four policy narratives, it is now time to sketch out their substantive form.

It may be useful to note that Aristotle recommended brevity in matters of narrative—deftly summarising the whole of the *Odyssey* in just seven lines. In what follows, I attempt—albeit somewhat weakly—to emulate that example by summarising a key narrative of English health services policy in just four paragraphs. The citations are of Department of Health publications (by year) as listed in Table 18.1. Note how the narrative unfolds in relation to the dates of publication. In the English case (though not so much in the other UK countries), it is a narrative that is concerned to introduce market forces into what is and has been a state-managed health service. Market forces are justified in terms of improving opportunities for the consumer (i.e., the patients in the service), and the pivot of the newly envisaged system is something called “patient choice” or “choice.” This is how the story unfolds as told through the policy documents between 2000–2008 (see Table 18.1).

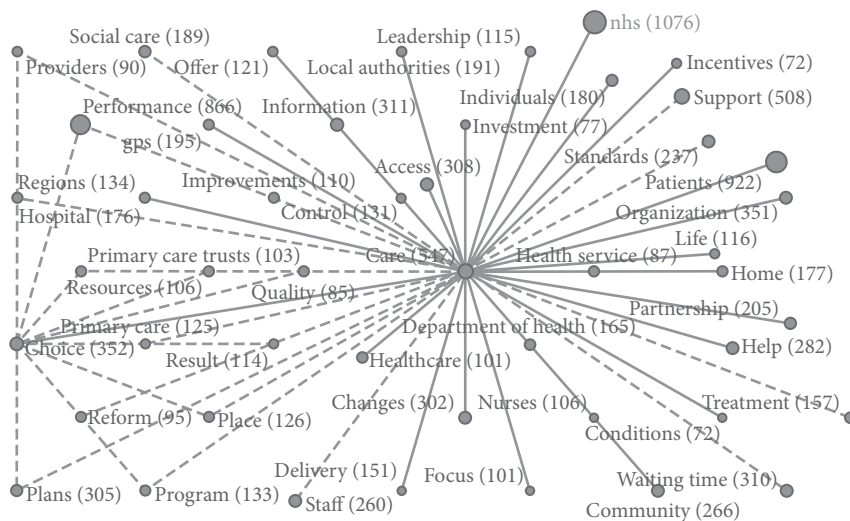


Figure 18.2 Concept cluster for “care” in six English policy documents, 2000–2007. Line thickness is proportional to the strength co-occurrence co-efficient. Node size reflects relative frequency of concept, and (numbers) refer to the frequency of concept. Solid lines indicate relationships between terms within the same cluster, and dotted lines indicate relationships between terms in different clusters. Source: Prior and Peckham (2012).

(1) The advent of the NHS in 1948 was a “seminal event” (2000:8), but under successive Conservative administrations the NHS was seriously underfunded (2006:3). The (New Labour) government will invest (2000) or already has (2003:4) invested extensively in infrastructure and staff, and the NHS is now on a “journey of major improvement” (2004:2). But “more money is only a starting point” (2000:2), and the journey is far from finished. Continuation requires some fundamental changes of “culture” (2003:6). In particular, the NHS remains unresponsive to patient need, and “[a]ll too often, the individual needs and wishes are secondary to the convenience of the services that are available. This ‘one size fits all’ approach is neither responsive, equitable nor person-centred” (2003:17). In short, the NHS is a 1940s system operating in a twenty-first-century world (2000:26). Change is therefore needed across the “whole system” (2005:3) of care and treatment.

(2) Above all, we have to recognize that we “live in a consumer age” (2000:26). People’s expectations have changed dramatically (2006:129), and people want more choice, more independence, and more control (2003:12) over their affairs. Patients are no longer, and should not be considered as, “passive recipients” of care (2003:62), but wish to be and should be (2006:81) actively “involved” in their treatments (2003:38, 2005:18)—indeed, engaged in a partnership (2003:22) of respect with their clinicians. Furthermore, most people want a personalized service “tailor made to their individual needs” (2000:17, 2003:15, 2004:1, 2006:83)—“[a] service which feels personal to each and every individual within a framework of equity and good use of public money” (2003:6).

(3) To advance the necessary changes, “patient choice” needs to be and “will be strengthened” (2000:89). “Choice” must be made to “happen” (2003), and it must be “real” (2003:3, 2004:5, 2005:20, 2006:4). Indeed, it must be “underpinned” (2003:7) and “widened and deepened” (2003:6) throughout the entire system of care.

(4) If “we” expand and underpin patient choice in appropriate ways and engage patients in their treatment systems, then levels of patient satisfaction will increase (2003:39), and their choices will lead to a more “efficient” (2003:5, 2004:2, 2006:16) and effective (2003:62, 2005:8) use of resources. Above all, the promotion of

choice will help to drive up “standards” of care and treatment (2000:4, 2003:12, 2004:3, 2005:7, 2006:3). Furthermore, the expansion of choice will serve to negate the effects of the “inverse care law,” whereby those who need services most tend to get catered for the least (2000:107, 2003:5, 2006:63), and it will thereby help in moderating the extent of health inequalities in the society in which we live. “The overall aim of all our reforms,” therefore, “is to turn the NHS from a top down monolith into a responsive service that gives the patient the best possible experience. We need to develop an NHS that is both fair to all of us, and personal to each of us” (2003:5).

We can see how most—though not all—of the elements of this story are represented in Figure 18.2. In particular we can see strong (co-occurrence) links between “care” and “choice” and how partnership, performance, control, and improvement have a prominent profile. There are of course some elements of the web that have a strong profile (in terms of node size and links) but to which we have not referred; access, information, primary care, and waiting times are four. As anyone well versed in English health care policy would know, these have important roles to play in the wider, consumer-driven narrative. However, by rendering the excluded as well as included elements of that wider narrative visible, the concept web provides a degree of verification on the content of the policy story as told herein and on the scope of its “coverage.”

In following through on this example, we have of course moved from content analysis to a form of discourse analysis (in this instance narrative analysis). That shift underlines aspects of both the versatility of CTA and some of its weaknesses—versatility in the sense that CTA can be readily combined with other methods of analysis and in the way in which the results of the CTA help us to check and verify the claims of the researcher. The weakness of the diagram compared to the narrative is that CTA on its own is a somewhat one-dimensional and static form of analysis, and while it is possible to introduce time and chronology into the diagrams, the diagrams themselves remain lifeless in the absence of some form of discursive overview. (For a fuller analysis of these data see, Prior, Hughes, & Peckham, 2012).

Analyzing a Single Interview: The Role of Content Analysis in a Case Study

So far I have focused on using content analysis on a sample of interviews and on a sample of

documents. In the first instance, I recommended CTA for its capacity to tell us something about what is seemingly central to interviewees and for demonstrating how what is said is linked (in terms of a concept network). In the second instance, I reaffirmed the virtues of co-occurrence and network relations, but this time in the context of a form of discourse analysis. I also suggested that CTA can serve an important role in the process of verification of a narrative and its academic interpretation. In this section, however, I am going to link the use of CTA to another style of research—case study—to show how CTA might be used to analyze a single “case.”

Case study is a term used in multiple and often ambiguous ways. However, Gerring (2004:342) defines it as “an intensive study of a single unit for the purpose of understanding a larger class of (similar) units.” As Gerring points out, case study does not necessarily imply a focus on $N = 1$, although that is indeed the most logical number for case study research (Ragin & Becker, 1992). Naturally, an N of 1 can be immensely informative, and whether we like it or not we often have only one N to study (think, e.g., of the 1986 Challenger shuttle disaster, or of the 9/11 attack on the World Trade Center). In the clinical sciences, of course, case studies are widely used to represent the “typical” features of a wider class of phenomena, and often used to define a kind or syndrome (as is in the field of clinical genetics). Indeed, at the risk of mouthing a tautology, one can say that the distinctive feature of case study is its focus on a case in all of its complexity—rather than on individual variables and their inter-relationships, which tends to be a point of focus for large N research.

There was a time when case study was central to the science of psychology. Breuer and Freud’s (2001) famous studies of “hysteria” (orig. 1895) provide an early and outstanding example of the genre in this respect, but as with many of the other styles of social science research, the influence of case studies waned with the rise of much more powerful investigative techniques—including experimental methods—driven by the deployment of new statistical technologies. Ideographic studies consequently gave way to the current fashion for statistically driven forms of analysis that focus on causes and cross-sectional associations between variables rather than ideographic complexity.

In the example that follows, we will look at the consequences of a traumatic brain injury (TBI) on just one individual. The analysis is based on

an interview with a person suffering from such an injury, and it was one of 32 interviews carried out with people who had experienced a TBI. The objective of the original research was to develop an outcome measure for TBI that was sensitive to the sufferer’s (rather than the health professional’s) point of view. In our original study (see Morris, Prior, Deb et al., 2005), interviews were also undertaken with 27 carers of the injured with the intention of comparing their perceptions of TBI to those of the people for which they cared. A sample survey was also undertaken to elicit views about TBI from a much wider population of patients than was studied via interview.

In the introduction, I referred to Habermas and the concept of the “lifeworld.” Lifeworld (*Lebenswelt*) is a concept that first arose out of twentieth-century German philosophy. It constituted a specific focus for the work of Alfred Schutz (see, e.g., Schutz and Luckman, 1974). Schutz described the lifeworld as “that province of reality which the wide-awake and normal adult simply takes-for-granted in an attitude of common sense” (1974:3). Indeed, it was the routine and taken-for-granted quality of such a world that fascinated Schutz. As applied to the worlds of those with head injuries, the concept has particular resonance because head injuries often result in that taken-for-granted quality being disrupted and fragmented, ending in what Russian neuropsychologist A.R. Luria once described as “shattered” worlds (Luria, 1975). As well as providing another excellent example of a case study, Luria’s work is also pertinent because he sometimes argued for a “romantic science” of brain injury—that is, a science that sought to grasp the world view of the injured patient by paying attention to an unfolding and detailed personal “story” of the head injured as well as to the neurological changes and deficits associated with the injury itself. In what follows, I shall attempt to demonstrate how CTA might be used to underpin such an approach.

In the original research, we began analysis by a straightforward reading of the interview transcripts. Unfortunately, a simple reading of a text or an interview can, strangely, mislead the reader into thinking that some issues or themes are actually more important than is warranted by the actual contents of the text. How that comes about is not always clear, but it probably has something to do with a desire to develop “findings” and our natural capacity to overlook the familiar in favor of the unusual. For that reason alone, it is always useful to subject any text to some kind of concordance analysis—that

is, generating a simple frequency list of words used in an interview or text. Given the current state of technology, one might even speak these days of using text-mining procedures such as the aforementioned *Clementine* to undertake such a task. By using *Clementine*, and as we have seen, it is also possible to measure the strength of co-occurrence links between elements (i.e., words and concepts) in the entire data set (in this example, 32 interviews), though for a single interview these aims can just as easily be achieved using much simpler, low-tech strategies.

By putting all 32 interviews into the database, a number of common themes emerged. For example, it was clear that “time” entered into the semantic web in a prominent manner, and it was clearly linked to such things as “change,” “injury,” “the body,” and what can only be called the “I was.” Indeed, time runs through the 32 stories in many guises, and the centrality of time is of course a reflection of storytelling and narrative recounting in general—chronology, as we have noted, being a defining feature of all story telling (Ricoeur, 1984). Thus sufferers recounted both the events surrounding their injury and provided accounts as to how the injuries affected their present life and future hopes. As to time present, much of the patient story circled around activities of daily living—walking, working, talking, looking, feeling, remembering, and so forth.

Understandably, the word and the concept of “injury” featured largely in the interviews, though it was a word most commonly associated with discussions of physical consequences of injury. There were many references in that respect to injured arms, legs, hands, and eyes. There were also references to “mind”—though with far lesser frequency than with references to the body and to body parts. Perhaps none of this is surprising. However, one of the most frequent concepts in the semantic mix was the “I was” (716 references). The statement “I was,” or “I used to” was in turn strongly connected to terms such as “the accident” and “change.” Interestingly, the “I was” overwhelmingly eclipsed the “I am” in the interview data (the latter with just 63 references). This focus on the “I was” appears in many guises. For example, it is often associated with the use of the passive voice: “I was struck by a car;” “I was put on the toilet;” “I was shipped from there then, transferred to [Cityville];” “I got told that I would never be able...;” “I was sat in a room,” and so forth. In short, the “I was” is often associated with things, people, and events acting upon

the injured person. More importantly, however, the appearance of the “I was” is often used to preface statements signifying a state of loss or change in the person’s course of life—that is, as an indicator for talk about the patient’s shattered world. For example, Patient 7122 stated, “The main (effect) at the moment is I’m not actually with my children, I can’t really be their mum at the moment. I was a caring Mum, but I can’t sort of do the things that I want to be able to do like take them to school. I can’t really do a lot on my own. Like crossing the roads.”

Another patient stated, “Everything is completely changed. The way I was... I can’t really do anything at the moment. I mean my German, my English, everything’s gone. Job possibilities is out the window. Everything is just out of the window... I just think about it all the time actually every day you know. You know it has destroyed me anyway, but if I really think about what has happened I would just destroy myself.”

Each of these quotations in its own way serves to emphasize how life has changed and how the patient’s world has changed. In that respect, we can say that one of the major outcomes arising from TBI may be substantial “biographical disruption” (Bury, 1982), whereupon key features of an individual’s life course are radically altered forever. Indeed, as Becker (1997:37) argues in relation to a wide array of life events, “When their health is suddenly disrupted, people are thrown into chaos. Illness challenges one’s knowledge of one’s body. It defies orderliness. People experience the time before their illness and its aftermath as two separate entities.” Indeed, this notion of a cusp in personal biography is particularly well illustrated by Luria’s patient Zasetsky; the latter often refers to being a “newborn creature” (Luria, 1975:24, 88), a shadow of a former self (1975:25), and as having his past “wiped out” (1975: 116).

However, none of this tells us about how these factors come together in the life and experience of one individual. When we focus on an entire set of interviews, we necessarily lose the rich detail of personal experience and tend instead to rely on a conceptual rather than a graphic description of effects and consequences (to focus on, say, “memory loss,” rather than loss of memory about family life). The contents of Figure 18.3 attempt to correct that vision. It records all of the things that a particular respondent (Patient 7011) used to do and liked doing. It records all of the things that he says that can no longer do (at one year after injury), and it records all of the consequences that he suffered from his head injury

at the time of interview. Thus we see references to epilepsy (his “fits”), paranoia (the patient spoke of his suspicions concerning other people, people scheming behind his back, and his inability to trust others), deafness, depression, and so forth. Note that, although I have inserted a future tense into the web (“I will”), such a statement never appeared in the transcript. I have set it there for emphasis and to show how for this person the future fails to connect to any of the other features of his world except in a negative way. Thus he states at one point that he cannot think of the future because it makes him feel depressed (see Fig. 18.3). The line thickness of the arcs reflect the emphasis that the subject placed on the relevant “outcomes” in relation to the “I was” and the “now” during the interview. Thus we see that factors affecting his concentration and balance loom large but that he is also concerned about his being dependent on others, his epileptic fits, and his being unable to work and drive a vehicle. The schism in his life between what he used to do, what cannot now do, and his current state of being is nicely represented in the CTA diagram.

What have we gained from executing this kind of analysis? For a start, we have moved away from a focus on variables, frequencies, and causal connections (e.g., a focus on the proportion of people with TBI who suffer from memory problems or memory problems and speech problems) and refocused on how the

multiple consequences of a TBI link together in one person. In short, instead of developing a narrative of acting variables, we have emphasized a narrative of an acting individual (Abbott, 1992:62). Second, it has enabled us to see how the consequences of a TBI connect to an actual lifeworld (and not simply an injured body). So the patient is not viewed just as having a series of discrete problems such as balancing, or staying awake, which is the usual way of assessing outcomes, but is seen as someone struggling to come to terms with an objective world of changed things, people, and activities (missing work is not, for example, routinely considered an “outcome” of head injury). Third, by focusing on what the patient was saying, we gain insight into something that is simply not visible by concentrating on single outcomes or symptoms alone—namely, the void that rests at the center of the interview, what I have called the “I was.” Fourth, we have contributed to understanding a type, for the case that we have read about is not simply a case of “John” or “Jane” but a case of TBI, and in that respect it can add to many other accounts of what it is like to experience head injury—including one of the most well documented of all TBI cases, that of Zatetsky. Finally, we have opened up the possibility of developing and comparing cognitive maps (Carley, 1993) for different individuals, and thereby gained insight into how alternative cognitive frames of the world arise and operate.

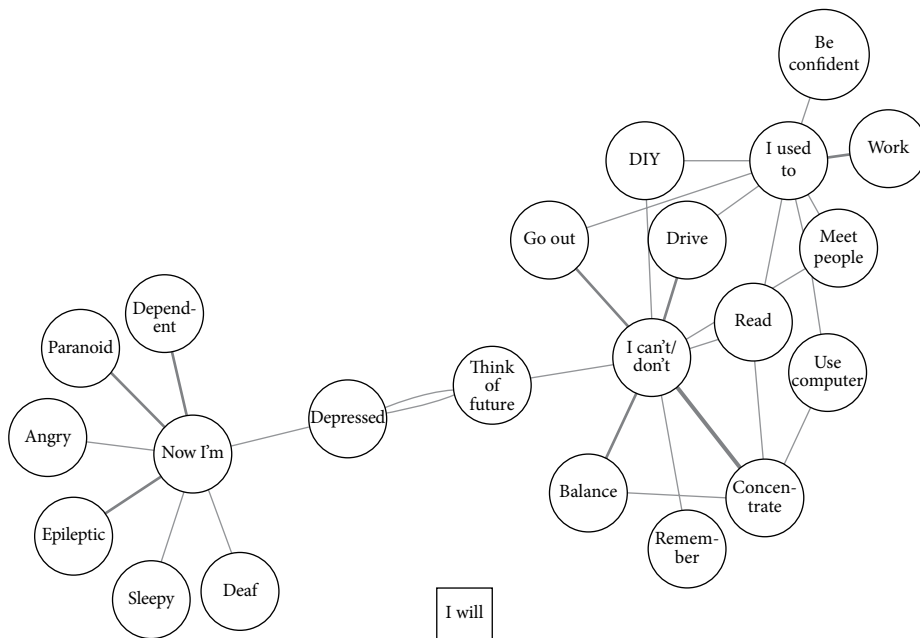


Figure 18.3 The shattered world of patient 7011. Thickness of lines (arcs) are proportional to the frequency of reference to the “outcome” by the patient during interview.

Tracing the Biography of a Concept

In the previous sections, I emphasised the virtues of CTA for its capacity to link into a data set in its entirety—and how the use of CTA can counter any tendency of a researcher to be selective and partial in the presentation and interpretation of information contained in interviews and documents. However, that does not mean that we always have to take an entire document or interview as the data source. Indeed, it is possible to select (on rational and explicit grounds) sections of documentation and to conduct the CTA on the chosen portions. In the example that follows, I do just that. The sections that I chose to concentrate on are titles and abstracts of academic papers—rather than the full texts. The research on which the following is based is concerned with a biography of a concept and is being conducted in conjunction with a PhD student of mine, Joanne Wilson. Joanne thinks of this component of the study more in terms of a “scoping study” than of a biographical study, and that too is a useful framework for structuring the context in which CTA can be used. Scoping studies (Arksey & O’Malley, 2005) are increasingly used in health related research to “map the field” and to get a sense of the range of work that has been conducted on a given topic. Such studies can also be used to refine research questions and research designs. In our investigation the scoping study was centred on the concept of “well-being.” During the past decade or so, “well-being” has emerged as an important research target for governments and corporations as well as for academics, yet it is far from clear to what

the term refers. Given the ambiguity of meaning, it is clear that a scoping review, rather than either a systematic review or a narrative review of available literature, would be best suited to our goals.

The origins of the concept of well-being can be traced at least as far back as the fourth century B.C., when philosophers produced normative explanations of the good life (e.g., eudaimonia, hedonia, and harmony). However, contemporary interest in the concept seemed to have been regenerated by the concerns of economists and most recently psychologists. These days governments are equally concerned with measuring well-being to inform policy and conduct surveys of well-being to assess that state of the nation (see, e.g., Office for National Statistics [ONS], 2012)—but what are they assessing?

We adopted a two-step process to address the research question, “What is the meaning of ‘well-being’ in the context of public policy?” First, we explored the existing thesauri of eight databases to establish those higher-order headings (if any) under which articles with relevance to well-being might be catalogued. Thus we searched the following databases: Cumulative Index of Nursing and Allied Health Literature [CINAHL], EconLit, Health Management Information Consortium [HMIC], MEDLINE, Philosopher’s Index, PsycINFO, Sociological Abstracts, and Worldwide Political Science Abstracts (WPSA). Each of these databases adopts keyword-controlled vocabularies. In other words, they use inbuilt statistical procedures to link core terms to a set lexis of phrases that depict the concepts contained in the database. Table 18.2 shows each database and its associated taxonomy.

Table 18.2 List of databases and associated taxonomies

Database	Taxonomy
CINAHL	CINAHL headings; ESBCO
EconLit	Journal of Economic Literature; American Economic Association
HMIC	DH-Data Thesaurus; Department of Health
MEDLINE	Medical Subject Headings; US National Library of Medicine
Philosopher’s Index	The Philosopher’s Index Thesaurus; Bowling State University
PsycINFO	Thesaurus of Psychological Index Terms; American Psychological Association
Sociological Abstracts	Thesaurus of Sociological Indexing Terms; Bowling State University
WPSA	Thesaurus of Political Indexing Terms; CSA Illumina

CINAHL = Cumulative Index of Nursing and Allied Health Literature; HMIC = Health Management Information Consortium; WPSA = Worldwide Political Science Abstracts.

The contents of the table point toward a linguistic infrastructure in terms of which academic discourse is conducted, and our task was to extract from this infrastructure the semantic web wherein the concept of “well-being” is situated. We limited the thesaurus terms to “well-being” and its variants (i.e., wellbeing or well being). If the term was returned, it was then exploded to identify any associated terms.

To develop the conceptual map, we conducted a free-text search for well-being and its variants within the context of public policy across the same databases. We orchestrated these searches across five separate timeframes: January 1990 to December 1994, January 1995 to December 1999, January 2000 to December 2004, January 2005 to December 2009, and January 2010 to October 2011. Naturally, different disciplines use different words to refer to well-being, each of which may wax and wane in usage over time. The searches thus sought to quantitatively capture any changes in the use and subsequent prevalence of well-being and any referenced terms (i.e., to trace a biography).

It is important to note that we did not intend to provide an exhaustive, systematic search of all the relevant literature. Rather we wanted to establish the prevalence of well-being and any referenced (i.e., allied) terms within the context of public policy. This has the advantage of ensuring that any identified words are grounded in the literature (i.e., they represent words actually used by researchers to talk and write about well-being in policy settings). The searches were limited to abstracts to increase specificity, albeit at some expense to sensitivity, with which we could identify relevant articles.

We also employed inclusion/exclusion criteria to facilitate the process by which we selected articles, thereby minimizing any potential bias arising from our subjective interpretations. We included independent, standalone investigations relevant to the study’s objectives (i.e., concerned with well-being in the context of public policy), which focused on well-being as a central outcome or process and which made explicit reference to “well-being” and “public policy” in either the title or the abstract. We excluded articles that were irrelevant to the study’s objectives, used noun adjuncts to focus on the well-being of specific populations (i.e., children, elderly, women) and contexts (e.g., retirement village), or that focused on deprivation or poverty unless poverty indices were used to understand well-being as opposed to social exclusion. We also excluded book reviews and abstracts describing a compendium of studies.

Using these criteria, Joanne Wilson conducted the review and recorded the results on a template developed specifically for the project, organized chronologically across each database and timeframe. Results were scrutinized by two other colleagues to ensure the validity of the search strategy and the findings. Any concerns regarding the eligibility of studies for inclusion were discussed amongst the research team. I then analyzed the co-occurrence of the key terms in the database. The resultant conceptual map is shown in Figure 18.4.

The diagram can be interpreted as a visualization of a conceptual space. So when academics write about “well-being” in the context of public policy, they tend to connect the discussion to the other terms in the matrix. “Happiness,” “health,” “economic,” and “subjective,” for example, are relatively dominant terms in the matrix. The node size of these words suggest that references to such entities is only slightly less than reference to well-being itself. However, when we come to analyse how well-being is talked about in detail, we see specific connections come to the fore. Thus the data imply that talk of “subjective well-being” far outweighs discussion of “social well-being,” or “economic well-being.” Happiness tends to act as an independent node (there is only one occurrence of happiness and well-being), probably suggesting that “happiness” is acting as a synonym for wellbeing. Quality of life (QoL) is poorly represented in the abstracts, and its connection to most of the other concepts in the space is very weak—confirming, perhaps, that QoL is unrelated to contemporary discussions of well-being and happiness. The existence of “measures” points to a distinct concern to assess and to quantify expressions of happiness, well-being, economic growth, and gross domestic product. More important and underlying this detail, there are grounds for suggesting that there are in fact a number of tensions in the literature on well-being.

On one hand, the results point toward an understanding of well-being as a property of individuals—as something that they feel or experience. Such a discourse is reflected through the use of words like “happiness,” “subjective,” and “individual.” This individualistic and subjective frame has grown in influence over the past decade in particular, and one of the problems with it is that it tends toward a somewhat content-free conceptualisation of well-being. To feel a sense of well-being one merely states that one is in a state of well-being; to be happy, one merely proclaims that one is happy (cf. ONS, 2012). It is reminiscent of the conditions portrayed

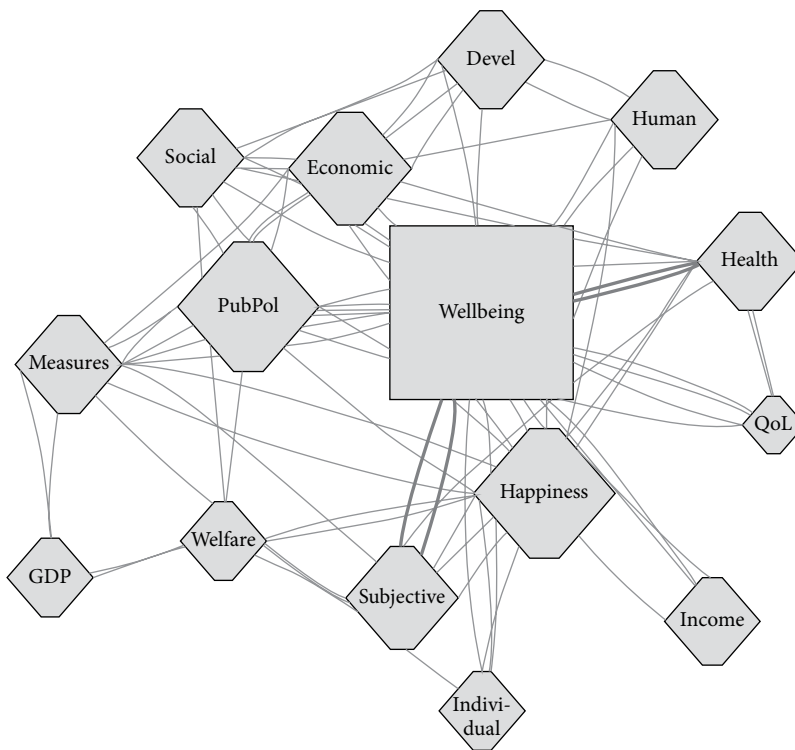


Figure 18.4 The position of a concept in a network—a study of “well-being.” Node size is proportional to the frequency of terms in 54 selected abstracts. Line thickness is proportional to the co-occurrence of two terms in any phrase of three words (e.g., subjective well-being, economics of well-being, well-being and development).

in Aldous Huxley’s *Brave New World*, wherein the rulers of a closely managed society gave their priority to maintaining order and ensuring the happiness of the greatest number—in the absence of attention to justice or freedom of thought or any sense of duty and obligation to others, many of whom were systematically bred in “the hatchery” as slaves.

On the other hand, there is some intimation in our web that the notion of well-being cannot be captured entirely by reference to individuals alone and that there are other dimensions to the concept—that well-being is the outcome or product of, say, access to reasonable incomes, to safe environments, to “development,” and to health and welfare. It is a vision hinted at by the inclusion of those very terms in the network. These different concepts necessarily give rise to important differences concerning how well-being is identified and measured and therefore what policies are most likely to advance well-being. In the first kind of conceptualization, we might improve well-being merely by dispensing what Huxley referred to as “soma” (a super drug that ensured feelings of happiness and elation); in the other case, however, we would need to invest in economic, human, and social capital as

the infrastructure for well-being. In any event and even at this nascent level, we can see how content analysis can begin to tease out conceptual complexities and theoretical positions in what is otherwise routine textual data.

Putting the Content of Documents in Their Place

I suggested in my introduction that CTA was a method of analysis—not a method of data collection nor a form of research design. As such, it does not necessarily inveigle us into any specific forms of either design or of data collection, though designs and methods that rely on quantification are dominant. In this closing section, however, I want to raise the issue as to how we should position a study of content in our research strategies as a whole. For we need to keep in mind that documents and records always exist in a context, and that while what is “in” the document may be considered central, a good research plan can often encompass a variety of ways of looking at how content links to context. Hence in what follows I intend to outline how an analysis of content might be combined with other ways of looking at a record or text, and even how the

analysis of content might even be positioned as secondary to an examination of a document or record. The discussion calls upon a much broader analysis as presented in Prior (2011).

I have already stated that basic forms of CTA can serve as an important point of departure for many different types of data analysis—for example, as discourse analysis. Naturally, whenever “discourse” is invoked, there is at least some recognition of the notion that words might actually play a part in structuring the world rather than merely reporting on it or describing it (as is the case with the 2002 State of the Nation address that was quoted in Section “Units of Analysis”). Thus, for example, there is a considerable tradition within social studies of science and technology for examining the place of scientific rhetoric in structuring notions of “nature” and the position of human beings (especially as scientists) within nature (see, e.g., work by Bazerman, 1988; Gilbert & Mulkay, 1984; and Kay, 2000). Nevertheless, little if any of that scholarship situates documents as anything other than as inert objects, either constructed by or waiting patiently to be activated by scientists.

However, in the tradition of the ethnomethodologists (Heritage, 1991) and some adherents of discourse analysis, it is also possible to argue that documents might be more fruitfully approached as a “topic” (Zimmerman and Pollner; 1971) rather than a “resource” (to be scanned for content), in which case the focus would be on the ways in which any given document came to assume its present content and structure. In the field of documentation, these latter approaches are akin to what Foucault (1970) might have called an “archaeology of documentation” and are well represented in studies of such things as how crime, suicide, and other statistics and associated official reports and policy documents are routinely generated. That too is a legitimate point of research focus, and it can often be worth examining the genesis of, say, suicide statistics or statistics about the prevalence of mental disorder in a community as well as using such statistics as a basis for statistical modeling.

Unfortunately, the distinction between topic and resource is not always easy to maintain—especially in the hurly-burly of doing empirical research (see, e.g., Prior, 2003). Putting an emphasis on “topic,” however, can open up a further dimension of research, and that concerns the ways in which documents function in the everyday world. And as I have already hinted, when we focus on function, it becomes apparent that documents serve not

merely as containers of content but very often as active agents in episodes of interaction and schemes of social organization. In this vein, one can begin to think of an ethnography of documentation. Therein, the key research questions revolve around the ways in which documents are used and integrated into specific kinds of organizational settings, as well as with how documents are exchanged and how they circulate within such settings. Clearly, documents carry content—words, images, plans, ideas, patterns, and so forth—but the manner in which such material is actually called upon and manipulated, and the way in which it functions, cannot be determined (though it may be constrained) by an analysis of content. Thus, Harper’s (1998) study of the use of economic reports *inside* the International Monetary Fund provides various examples of how “reports” can function to both differentiate and cohere work groups. In the same way, Henderson (1995) illustrates how engineering sketches and drawings can serve as what she calls conscription devices on the workshop floor.

Of course, documents constitute a form of what Latour (1986) would refer to as “immutable mobiles,” and with an eye on the mobility of documents, it is worth noting an emerging interest in histories of knowledge that seek to examine how the same documents have been received and absorbed quite differently by different cultural networks (see, e.g., Burke, 2000). A parallel concern has arisen with regard to the newly emergent “geographies of knowledge” (see, e.g., Livingstone, 2005). In the history of science, there has also been an expressed interest in the biography of scientific objects (Latour, 1987:262) or of “epistemic things” (Rheinberger, 2000)—tracing the history of objects independent of the “inventors” and “discoverers” to which such objects are conventionally attached. It is an approach that could be easily extended to the study of documents and is partly reflected in the earlier discussion concerning the meaning of the concept of well-being. Note how in all of these cases a key consideration is how words and documents as “things” circulate and translate from one culture to another; issues of content are secondary.

Clearly, studying how documents are used and how they circulate can constitute an important area of research in its own right. Yet even those who focus on document use can be overly anthropocentric and subsequently overemphasize the potency of human action in relation to written text. In that light, it is interesting to consider ways in which we might reverse that emphasis and instead to study

the potency of text and the manner in which documents can influence organizational activities as well as reflect them. Thus Dorothy Winsor (1999) has, for example, examined the ways in which work orders drafted by engineers not only shape and fashion the practices and activities of engineering technicians but construct “two different worlds” on the workshop floor.

In light of this, I will suggest a typology (Table 18.3) of the ways in which documents have come to be and can be considered in social research.

While accepting that no form of categorical classification can capture the inherent fluidity of the world, its actors, and its objects, Table 18.3 aims to offer some understanding of the various ways in which documents have been dealt with by social researchers. Thus approaches that fit into cell 1 have been dominant in the history of social science generally. Therein documents (especially as text) have been analyzed and coded for what they contain in the way of descriptions, reports, images, representations, and accounts. In short, they have been scoured for evidence. Data-analysis strategies concentrate almost entirely on what is in the “text” (via various forms of content analysis). This emphasis on content is carried over into cell 2 type approaches with the key differences that analysis is concerned with how document content comes into being. The attention here is usually on the conceptual architecture and socio-technical procedures by means of which written reports, descriptions, statistical data, and so forth are generated. Various kinds of discourse analysis have been used to unravel the conceptual issues, while a focus on socio-technical and rule-based procedures by means of which clinical, police, social work, and other forms of records and reports are constructed has been well represented in the work of ethnomethodologists (see Prior, 2011). In contrast, and in cell 3, the research focus is on the ways in which documents are called upon as a resource by various and different kinds

of “user.” Here concerns with document content or how a document has come into being are marginal, and the analysis concentrates on the relationship between specific documents and their use or recruitment by identifiable human actors for purposeful ends. I have already pointed to some studies of the latter kind in earlier paragraphs (e.g., Henderson, 1995). Finally, the approaches that fit into cell 4 also position content as secondary. The emphasis here is on how documents as “things” function in schemes of social activity and with how such things can drive, rather than be driven by, human actors. In short, the spotlight is on the *vita activa* of documentation, and I have provided numerous example of documents as actors in other publications (see Prior, 2003; 2008; 2011).

Conclusion

Content analysis was a method originally developed to analyze mass media “messages” in an age of radio and newspaper print, and well before the digital age. Unfortunately, it struggles to break free of its origins and continues to be associated with the quantitative analysis of “communication.” Yet as I have argued, there is no rational reason why its use has to be restricted to such a narrow field, for it can be used to analyze printed text and interview data (as well as other forms of inscription) in various settings. What it cannot overcome is the fact that it is a method of analysis and not a method of data collection. However, as I have shown, it is an analytical strategy that can be integrated into a variety of research designs and approaches—cross-sectional and longitudinal survey designs, ethnography and other forms of qualitative design, and secondary analysis of pre-existing data sets. Even as a method of analysis it is flexible and can be used either independent of other methods or in conjunction with them. As we have seen, it is easily merged with various forms of discourse analysis and can be used as an exploratory method or as a means of verification. Above

Table 18.3 Approaches to the study of documents

FOCUS OF RESEARCH APPROACH	DOCUMENT AS RESOURCE	DOCUMENT AS TOPIC
CONTENT	(1) Approaches that focus almost entirely on what is “in” the document.	(2) “Archaeological” approaches that focus on how document content comes into being.
USE AND FUNCTION	(3) Approaches that focus on how documents are used as a resource by human actors for purposeful ends.	(4) Approaches that focus on how documents function in, and impact on schemes of social interaction and social organization.

Source: Prior (2008).

all, perhaps, it crosses the divide between “quantitative” and “qualitative” modes of inquiry in social research and offers a new dimension to the meaning of mixed-methods research. I recommend it.

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Photography as a Research Method

Gunilla Holm

Abstract

This chapter discusses the development of photography as a research method in social sciences. It describes the different types of photographs used, such as archival photographs and photographs taken by the researcher, and it focuses especially on photographs taken by participants. The uses of different approaches to obtain photographs and issues of interest concerning each approach are presented. The most common approaches to analyze photographs, such as content analysis, discourse analysis, and ethnographic analysis are described. Interesting and challenging questions about the interpretation and presentation of photographs are raised, such as the impact of the researcher's and participants' habitus on the interpretation of photographs. Finally, ethical issues in research using photographs are considered, especially the meaning of informed consent and confidentiality in photographic research is emphasized.

Key Words: Photography, participatory photography, documentary photographs, interpretation, habitus, visual ethnography, visual research, metaphorical photographs, confidentiality

We encounter numerous photographs or visual pictures many times every day. They might range from photos on billboards to mug shots in a newspaper or photos of family members on a person's work desk. We notice and process most of them on a superficial level, but some have more of an impact on us. They affect us more profoundly, emotionally or intellectually. Overall, our culture is becoming more and more visual, with images saturating our environment not only through the more traditional modes like TV, newspapers, and magazines, but also through smartphones with cameras and social media like Facebook. Despite living in a visual age (Gombrich, 1996) and the visual saturation of our culture, photographs are underutilized in social science research.

This chapter explores how photography has been used in social science research and what the current developments are. Commonly, we refer to visual methods and visual research, but here we can distinguish between two major kinds, namely,

film/video research and research using photography. Within both fields are many different ways of using videos and still photos. For example, with regard to video, the researcher can do the videotaping, but in recent research family members also act as co-researchers, videotaping another family member at home in the absence of the researcher (Sahlström, Pörn, & Slotte-Lüttge, 2008). Likewise, for photography, photos can be taken by the researcher or the research participants or existing photographs can be used. Videos and photographs require different kinds of analyses and are reported in different ways. Although there is a considerable variety and complexity of work arising from the two methods, in this chapter I give an in-depth discussion only of the use of photographs in social science research.

Even though some thought that digital photography might be the end of photography, it simply changed photography and made it even more popular. Mirzoeff (2009, pp. 2–3) estimates that there are “more than 3 billion photos on the file-sharing site

Flickr, and over 4 billion on the social networking site Facebook. . . Media estimates of the number of advertisements seen per day range from hundreds to the now widely used figure of 3,000"; furthermore, Mirzoeff estimates that in 2008, people took 478 billion photos using their mobile phones (p. 250).

The emphasis on visual images and on visual culture is also evident in the numerous textbooks on visual culture produced in the last fifteen years. A classic text in social sciences first published in 1999 is Evans and Hall's *Visual Culture: The Reader* (Evans & Hall, 2010). The book theorizes photography and provides theoretical perspectives on it, as well as providing a gender and race perspective on photographs. The difference between a visual and a textual research culture is well expressed by Kress and van Leeuwen (2006, p. 2) in their statement "(b)ut even when we can express what seem to be the same meanings in either image—form or writing or speech, they will be *realized* differently. For instance, what is expressed in language through the choice between different word classes and clause structures, may, in visual communication, be expressed through the choice between different uses of colour or different compositional structures. And this will affect meaning. Expressing something verbally or visually makes a difference." This difference is important in visual research. Different data and different results are obtained through different ways of doing the research. The search for a better understanding has led to a rapid increase in the use of photography in social science research. The visual culture research includes many different kinds of visuals, such as art pictures, graphs, and maps (for a comprehensive overview of different kinds of visuals, see Margolis & Pauwels, 2011; Reavey, 2011).

There has been a proliferation of books on general visual research methods including those by Emmison and Smith (2007), Margolis and Pauwels (2011), Mitchell (2011), Reavey (2011), Spencer (2011), and Stanczak (2007), as well as methodology books such those by as Pink (2007) and Rose (2012). Likewise, much has been published on specific aspects of visual research, such as visual ethnography (Pink, 2012) and the analysis of visual data (Ball & Smith, 1992; Banks, 2007). We also see the increasing popularity of visual research methods in social sciences; in addition to journals like *Visual Anthropology*, *Visual Anthropology Review*, *Visual communication*, and *Visual Studies*, many other journals now also publish photographs. In addition, the Society for Visual Anthropology and the International Visual Sociology Association

provide avenues and conferences for presenting visual research.

Across the social sciences, photography as a research method has a long history in fields such as anthropology and sociology, but it is fairly new to psychology and education. However, in sociology, photography continues to hold a marginal position, mainly because it is considered too subjective. In anthropology, film has been more important. Harper (2004) describes gathering information as a function for photography in social sciences. Here he uses Bateson and Mead's book *Balinese Character, A Photographic Essay* (1942) as an example; these researchers "used 759 photographs (selected from more than 25 000 taken during their fieldwork) to support and develop their ethnographic analysis" (Harper, 2004, p. 232). In his discussion of photography in sociology, Harper describes photography as being used mostly with the researcher as the photographer. A similar tendency can be seen in anthropology. Although earlier anthropologists and sociologists like Collier and Collier (1986), Prosser (1996), and Grady (1996) wanted to make photography fit into a "scientific" framework by providing exact methods for how to use photographs in research, contemporary ethnographers like Pink (2007) reject this approach. Pink argues for developing the way photography is used in research based on the questions and context of the study. The method can develop in the field, and she does not see the text as superior to the photographs, but as complementary and working together.

The field of psychology has engaged with photographs throughout its history, starting with Darwin's use of photographs in his work. "A historical analysis of the role of the visual within psychology can reveal its instrumental effects in providing the context for 'the psychological' to become observable and therefore, measurable and more 'scientific'" (Reavey, 2011, p. 2). As Reavey (2011) points out in her book *Visual Methods in Psychology*, qualitative research in psychology is a marginal field. The use of visual methods is thus at the margins of a marginal field of study. Contributing to this marginality, according to Reavey (p. xxvii), is the notion that photography as a method has been considered more appropriate for "use with children and those deemed less 'able' to communicate thoughts and feelings. . . In this sense, the 'visual' has traditionally been given the status of a naïve or more simplistic form of communication." Overall, qualitative research in psychology has focused on language- and text-based materials. In experimental psychology, photos are sometimes

used as material for memorization or evaluation tasks (Mavica & Barenholtz, 2012; Mandal, Bryden, & Bulman-Fleming, 1996). In psychology related to health, education, and similar fields, there is some research using photography as a method (e.g., Brazg, Bekemeier, Spigner, & Huebner, 2011; Clements, 2012), but a search of psychology databases gives very few studies using photography.

In education, photos have been used in archival research related to, for example, school and space (Grosvenor, Lawn, N6voa, Rousmaniere, & Smaller, 2004) and schooling and the marginalized (Devlieger, Grosvenor, Simon, Van Hove, & Vanobbergen, 2008; Grosvenor, 2007a; 2007b). Photography has also been used with preschool children to obtain an understanding of the children's perceptions of their own surroundings and communities (Clark & Moss, 2001; Einarsdottir, 2005; Serriere, 2010). The photographs are helpful especially if the children's language is not yet well developed (Clark, 2004; Prosser & Burke, 2008). Other examples of research in education using photography as a research method are studies focusing on elementary school students' views on school and gender issues (Newman, Woodcock, & Dunham, 2006) and on high school students' views on quality teachers (Marquez-Zenkov, Harmon, van Lier, & Marquez-Zenkov, 2007) as well as on themselves as high school students (Holm, 1995; 1997). Lodge (2009) argues that children and youth are often not heard in research on schools, but that photography offers possibilities for engaging young people in the research. She sees photography as especially useful for engaging those usually silenced or marginalized in the school community (See also Wilson et al., 2007, on the empowerment of students). Joanou (2009) points out that there are increased ethical considerations when working with marginalized groups of children, using as an example her study on children living and working on the streets in Lima.

Using photography in research with children is the fastest growing application of its kind. Most of this research is done in relation to the school setting, but research is also done on children's perspectives on, for example, their outdoor environment (Clark, 2007) and their city (Ho, Rochelle, & Yuen, 2011). Others discuss more generally the topic of using photography with children ranging from two years old to teenagers and children's photographic behavior (Sharples, Davison, Thomas, & Rudman, 2003; Stephenson, 2009; Warming, 2011).

In this chapter, I discuss photography as a research method, review the different types of photographs

used in research (e.g., archival photographs, photographs taken by the researcher), and focus especially on photographs taken by participants. The uses of different approaches to obtain photographs and issues of interest concerning each approach will be presented. The most common approaches used to analyze photographs are briefly described, and interesting and challenging questions about the interpretation and presentation of photographs are raised. Finally, ethical issues in research using photographs are considered.

Photography as a Research Method in Qualitative Research

In this chapter, a distinction is made between images and photographs. As stated earlier with regard to visual culture, images can also include such things as artwork, cartoons, drawings, and maps. In research studies, children are often asked to draw pictures on which interviews are then based (Ganesh, 2011). Drawings in combination with texts focused on schooling were also the focus of Holm's (1994) analysis of the teen magazine *Seventeen* (1966–89). In this study, the text and drawings placing an emphasis on how girls should behave and look made a strong counternarrative with regard to the importance of education for girls. The emphasis was on conforming to norms, on being stylish and pleasing, and on not challenging or upsetting male students. Skorapa (1994) analyzes how cartoons about schooling can either challenge or perpetuate stereotypes and the dominant ideology of schooling. Cartoons are not only amusing, but also often deal with cultural tensions, changes, and conflicts (Provenzo & Beonde, 1994).

In 1997, Jipson and Paley (1997a) published an unusual book called *Daredevil Research: Re-creating Analytic Practice* in which several of the chapters on postmodern research challenge our notions of how research should be reported. Many of the chapters incorporate or build on visual images. In Paley's (1997) chapter "Neither Literal nor Conceptual," the text blends with abstract black-and-white images. In another chapter by Jipson and Paley (1994b), text blocks are imposed in the middle of the pages, which in turn are a map of the space. In yet another chapter, the text itself constitutes an image by being written in one to four interweaving curving columns (Jipson & Wilson, 1997). Although we rarely see this kind of experimenting with the use of visual images, these examples and other more arts-based visual research (see e.g., Knowles & Cole, 2008; Jipson & Paley, 1997c) provide a sense of the endless possibilities of using images. Furthermore, photography itself provides a

lot of options; the kinds and uses of photographs are numerous. Due to the myriad of possibilities and the increasingly common use of photography, this chapter is limited to the use of photography in social science research.

One of the difficulties in using photography as a research method is the ambiguity that exists in photographs. Although photographs traditionally were thought of as portraying reality—the simple truth—this is no longer the case among researchers, even though many viewers still consider photographs as showing the truth. We acknowledge that photographs are constructed; they are made. Harper (2004) argues that this construction and subjectivity can be seen very clearly in photographs by early British anthropologists because they are all taken from the colonizers' perspective. Interestingly, Chaplin (1996) argues that photographs are both taken and made as opposed to just made or constructed. They are taken in the sense that they give researchers the information and details they need, more like a record or a document, but the researcher also makes decisions on what to photograph and how to set it up and process it.

The photographer always has a reason for taking the photograph. There is an intention behind the photograph. The photographer wants to see something in particular or wants to send us a "message." If the photographer is also a participant in the research, then the intended communication is connected to the researcher's intentions as well. The researcher also influences the process by having selected a particular photo to be viewed by others. Consequently, there is also the intended audience; for whom is the photo taken? And, finally, there are the individual viewers. Photographers and researchers have their aims and intentions, but they cannot influence or control how the viewer interprets the photo. Sometimes the intended audience is only the researcher, and most of the photos will be seen and analyzed only by the researcher. These photographs are taken exclusively for the research and the researcher.

Whatever the case, without an accompanying text, many photographs can carry multiple meanings for the viewer (Evans & Hall, 2010; see also Grosvenor & Hall, 2012). The possibility for multiple meanings and the ambiguity attached to photographs has made many, especially positivist, researchers uncomfortable with using or accepting photographs in books and articles. A good example of this is the disappearance of photos from the *American Journal of Sociology* under the direction of positivist editor Albion Small, even though the journal earlier had published numerous articles with photographs (Stasz, 1979).

Photographs as Illustrations or for Documentary Purposes

Photography can be considered a data collection method, but not all photographs are used as data to be analyzed. The most common uses for photographs in social science research have been as illustrations and documentation. Documentary photography has a long history in fields like anthropology and sociology, as discussed earlier, but also in fields like history, where archival photographs are often used. In cultural studies, a good example of historical analysis of documentary photographs is Steet's (2000) study of the construction of the Arab world in the magazine *National Geographic*. She analyzes one hundred years of photographs in the magazine, visually (and textually) constructing men and women as well as patriarchy and Orientalism in the Arab world. In contrast to Steet's extensive material, Magno and Kirk (2008) analyzed only three photographs when examining how development agencies use photos of girls to promote their agencies' work concerning the education of girls. However, they used an elaborate analysis template with seven categories: surface meaning, narrative, intended meaning, ideological meaning, oppositional reading, and coherency (coherency meaning in this case whether the photographs and the text argued for the same thing). Banks (2007, p. 11) explains the difference between using photographs as illustrations and as anthropological visual research in that photographs as illustrations "are not subject to any particular analysis in the written text, nor does the author claim to have gained any particular insights as a result of taking or viewing the images."

Wang (1999) describes a nontraditional approach to documentary photography as underpinning the photo-voice method. She sees photo-voice as theoretically grounded in critical consciousness and feminist theory and as an effort by "community photographers and participatory educators to challenge assumptions about representation and documentary authorship" (p. 185). Photo-voice is an approach to using photography as a method for collecting data that is merged with social activism and political change, and particularly with community involvement. The main goals of photo-voice are, according to Wang, Cash and Powers (2000, p. 82) "(a) to enable people to record and reflect their community's strengths and concerns, (b) to promote critical dialogue and knowledge about important community issues through large and small group discussion of photographs, and (c) to reach policy makers and people who can be mobilized

for change.” Wang has used this approach mostly for health-related research. Other researchers, like Duits (2010), claim to use photo-voice but without the community improvement goal; these kind of studies more closely resemble participatory photography research.

ARCHIVAL PHOTOS

Many archival photos were originally taken for documentary or illustrative purposes. The most frequent use of archival photos is in some form of historical research. Today, digital archives are becoming common, making it easier to search for particular kinds of photos. However, it is also very demanding to work with thousands of photos on a particular topic (Park, Mitchell, & de Lange, 2007). Photos are commonly of interest in newspaper or magazine research because they often are perceived as documenting or illustrating “objective” reality or providing evidence of historical events. For example, Martins (2009) includes a couple of photographs in her study of deaf pupils in a boarding school, illustrating and providing “proof” of the kinds of exercises the pupils had to do. A similar use of photos can be seen in Amsing and de Beer’s (2009) article on the intelligence testing of children with mental disabilities. Photos of the test and a testing situation show the reader “how things were done” in the testing of these children. However, in contemporary historical research, photos are critically examined with regard to how they construct an argument in interaction with the text in a particular context. Grosvenor and Hall (2012, p. 26) emphasize the importance of examining archival photos in relation to the text when creating meaning because “(w)ords when used with images can anchor meanings; change the words and the original meaning can be displaced, even though the image that it captures remain the same.” A common problem with archival photos is that they are anonymous, in the sense that nothing is known about them; neither the photographer’s intentions nor the context in which they are taken (Martin & Martin, 2004). Hence, the use of these kinds of photos for research is limited.

Photo albums are also a form of private or family archives. Because family albums and photos are very familiar to us as researchers, it is important to be aware of one’s own notions and constructions of families, of what one sees as a “normal family.” A reflexive approach is necessary so that the researcher does not impose his or her own views of families on the interpretation of albums. These kind of albums also bring forth an ethical issue, since photos often contain

images of family members and other people who have not given permission for their photos to be part of a research project (Allnutt, Mitchell, & Stuart, 2007).

Collier and Collier (1986) describe documentary photographs as “precise records of material reality.” Photographs document the world for further analysis at a later stage. However, Collier and Collier argue that many anthropologists have used photographs as illustrations but not as documentary data for research. Most anthropology and sociology researchers have themselves been photographers and often these photographs have been taken as illustrations or for documentary purposes.

Photographs Taken by Researchers

Traditionally, photographic surveys (see Collier & Collier, 1986) of, for example, visual aspects of workplaces or institutions were used as a way to systematically document and produce material to analyze so that the researcher could draw conclusions about working conditions, types of work, and the like. As Pink (2007) points out, the photos taken in these kinds of surveys do not say anything about whether the objects or physical surroundings are meaningful to participants or what meaning they hold for participants. In this case, the researchers decide on what they find interesting or potentially important enough to photograph. Photographs taken by researchers can also be used in photo-elicitation interviews, but, even so, it is still the researcher who sets the tone for what is important to discuss. It becomes the researcher’s interpretation of “reality” that is considered important and analyzed. In a well-known context, the researcher can provide both descriptive meaning as well as stories about each object (see Riggins, 1994), and this can make researcher-produced photographs very valuable for understanding processes. For example, Mitchell and Allnutt (2008, p. 267) describe how it is possible in photo documentary research to follow “social transitions or change by identifying shifts in material objects, dress, and so on.” Rieger (2011) calls the study of social change “rephotography” and suggests it for studying social change with regard to places, participants, processes, or activities. Hence, in this way, detailed photographic surveys produce data to be analyzed rather than photographs for documentary and illustrative purposes.

There is no agreement on what the best approach is for researchers to take photos in the research setting. Some argue that by taking photographs immediately, at the beginning of the study when entering the scene, the camera can function as an

opening device to create contact with the participants. Others argue that it is necessary for participants to get to know the researcher first, in order for them to feel comfortable with the camera and with being photographed.

Photographs Taken by Participants

Having participants take photographs, also called *participatory photography*, is the most frequently used photography method in social sciences today. Photographs taken by the participants for the purpose of, for example, photo-elicitation interviews encourage the participants to take a more active role in the research by indicating what is meaningful for them to discuss in the interview. It also gives participants more control over the interview (Clark-Ibáñez, 2004; Majumdar, 2011). Some researchers prefer to call this type of photography, in which participants construct and take the photos, *photo production* (Majumdar, 2011; Reavey, 2011). Radley (2011) points out that photos produced by participants also allow for interviewing about the circumstances of the production, which will give a more comprehensive insight into the participants' intentions. However, even if the participants produce the photographs, the researcher's presence is evident because the participants take the photos for the purpose of the research. In this chapter, I am not making a distinction between photographic interviews and photo-elicitation interviews. Most

researchers less familiar with participatory photography tend to use the term photo-elicitation interviews as covering all kinds of participatory photography. The focus here is instead on the issues surfacing in participatory photography.

Clear instructions about the purpose of the research and the photographs need to be given to participating photographers. Even so, participants often deviate from the instructions. In a study in England on young people's constructions of self and the connection to consumer goods, they were supposed to photograph goods they considered important. Instead, they all photographed mostly their friends. Hence, the participants redefined their task (Croghan, Griffin, Hunter, & Phoenix, 2008). Commonly, participants are asked to take photos during the study, but frequently they contribute photos that were taken previously, but which they think exemplify the topic. For example, in a study of language minority ninth graders' perceptions of their identifications and belonging to the Swedish language minority group in Finland, we (Holm, Londen, & Mansikka, 2014) found this to be common. Because the study was done in the fall, they found it difficult to photograph some things they thought were important for their identification. Therefore, they brought in many photos of, for example, flowers and parties taken in the summer that they believed exemplified being part of the language minority group (Figures 19.1 and 19.2).



Figure 19.1 The flowers portray the beautiful Swedish language.



Figure 19.2 Crayfish is something we eat with our friends. We always do it with Swedish-speaking Finns.

Likewise, participants most often are asked to be the photographers themselves, but it is quite common for participants to ask others to take photos of them as well. In a study with doctoral students about what it means to be a doctoral student, several students asked others to photograph them instead, or they used previously taken photos in which they themselves were included (Holm, 2008*a*). The

photo in Figure 19.3 is taken by a friend of a student who is a participating doctoral student.

The time of the year influences the study in other ways as well. Especially in countries with dark, gloomy winter weather, wintertime photo projects will produce more indoor photos and dark, gray outdoor photos. In a study on elementary students' perceptions of what community means to them



Figure 19.3 Photo of a doctoral student taken by a friend.



Figure 19.4 The time of year affects how photographs may look; this classroom photo was taken during the wintertime, which gives it a gloomy look.

and what their community consists of, the children took many outdoor photos of friends, their homes, and family cars, but the days when they happened to have a camera were overcast winter days. The indoor photos of their classrooms, schoolmates, and teachers are also quite gloomy despite the smiling faces. Hence, looked on out of context, there is a somewhat downcast mood over the photos even though their neighborhood is a very lush, green one with a vibrant street and porch life in the summer. Consequently, photos taken in summertime

would have looked much more upbeat and cheery (Figures 19.4 and 19.5).

The importance of clearly communicating to the participants in multiple ways the purpose of the research and the participants' task cannot be overemphasized. The study of students' perceptions of the meaning of community and their own community was originally a comparative study between a school in the United States and a school in Finland. The students in Finland were Finnish speakers attending an English-language school, and the researcher was



Figure 19.5 Outdoor wintertime scenes may hide the true nature of an environment.

American. The students understood that their task was to photograph their school community instead of their community in general, which resulted in photographs mostly of their friends at school.

A weak common verbal language can also be overcome if participants express themselves through photographs. Veintie and Holm (2010) did a study of how Indigenous teacher education students from three different tribes thought of knowledge and learning in an intercultural bilingual teacher education institute in Ecuadorian Amazonia. Spanish was the common language, but it was also the second or third language for everybody. To ease the limitations for the students to express Indigenous thinking about these concepts in Spanish, the students took photographs that were then used as the basis for interviews. As researchers, we assumed that many photos about learning and knowledge would be related to schools and the teacher education institute because they were very prominent in the small community. Instead, the photographs portraying learning always involved people and actions and were mostly taken in the community (see Figure 19.6). In interviews, students also explained that learning is not given, but that learners are given the opportunities to observe and practice what is to be learned. Students also expressed knowledge as lived knowledge. Therefore, many students could not participate in the study because their families and homes were too far away to be photographed. Knowledge was grounded in their communities and

their ancestors. The school-based photographs were only a small part of the pictures showing learning and knowledge. Instead, learning and knowledge were based on social interaction. Images like books, newspapers, internet, and television were completely absent because they held no meaning and were not present in the students' lives. These aspects of knowing and learning would have most likely not emerged if the students would have only been interviewed.

ETHICAL ISSUES IN PARTICIPATORY PHOTOGRAPHY

Access to research sites for qualitative and especially ethnographic research can be difficult. Many institutional review boards (IRBs) and sites like workplaces, schools, and organizations are cautious about providing access, particularly to vulnerable populations like children and the ill. The very openness of the qualitative, ethnographic design may raise concerns. It is impossible to know in advance exactly what questions will be asked or what situations will be observed. Likewise, the analysis may be perceived as being too open and imprecise. These issues are often amplified with regard to including photography as a research method. The cautiousness is justified because the risks are higher when participants can be identified. There is no confidentiality if a photograph includes a person's face. If the researcher is also the photographer, there is of course more control over what will be photographed, and



Figure 19.6 An Ecuadorian student's photo of an object that represents community knowledge and learning.

the researcher can use his or her ethical judgment in each situation and refrain from taking photographs that could potentially harm or compromise participants or the research site. Conversely, if the participants are the photographers very clear instructions can be given about what should be photographed, but there is no guarantee that participant photographers will keep to the topic or particular settings. It then becomes the responsibility of the researcher to exclude potentially harmful or compromising photographs from being published or publically presented.

Getting official permission and access is a first step, but securing informed consent from participants or the people who participants include in their photographs can be complicated. It is difficult to know if participants fully understand how their own photographs or the photographs of others might be used later. Publishing photographs in a book is easier to grasp, but the lack of control over photos on websites or explaining that they might be shown and discussed in conferences across the world is more complicated. Institutional review boards seem to perceive the risks for taking advantage of children as lower if the children themselves take the pictures (Holm, 2008b), which means that it is somewhat easier to get IRB approval for these kinds of studies. However, children taking photographs requires informed consent from guardians, beyond the informed assent of the children themselves. Involving children means more difficulties in gathering guardians' consent and children's assent forms. In most studies, some guardians will not give their consent despite their children wanting to participate; conversely, some guardians will give their consent but their children will not want to participate. One way to avoid having to exclude children who want to participate is to make the photography assignment part of a school or organization project in which all children participate, but only those with their guardian's permission participate in the actual research.

Guardians are a form of gatekeepers, but more unpredictable gatekeepers are institutions such as schools, day care facilities, hospitals, and businesses or organizations. For example, in an ethnographic study of a school for pregnant and parenting teenage girls, the girls were going to photograph their lives as pregnant and/or parenting teenagers. However, after the study was set up, the school principal suddenly decided that the girls could not take photos of any males or of their children in diapers or taking baths. This restricted the girls so much that, in

the end, they mostly took photos of each other posing at school. The restrictions were understandable, because there were several fights in school about the fathers of the babies (e.g., one man had fathered three children with three different girls) or the girls' boyfriends who sometimes switched from one girl to another. Likewise, the restriction about not taking nude pictures of babies was understandable because the principal wanted to protect the babies from potential abuse, especially in light of the fact that several girls had themselves been abused in different ways. However, the restrictions were imposed on the girls without an explanation of why the rules had suddenly changed. These kinds of rules imposed from above reinforced the general management and control attitude of the school with regard to the girls' schooling (Holm, 1995).

Certain studies are difficult to do without the participants acting as co-researchers/photographers. Janhonen-Abruquah (2010) studied the daily transnational lives of immigrant women. The women kept photographic diaries of their everyday mundane activities, revealing the importance of cross-border communication between women in extended families living in different parts of the world. The women decided on what and who they photographed. Due to the often fairly private family situations portrayed, Janhonen-Abruquah decided to blur the faces in the photographs to protect the participants' identities (Figure 19.7). This allowed photos of people to be used without obtaining permissions from everybody included, which would have been difficult for the women to do. However, if someone familiar with the women reads the study, it might be possible for him or her to recognize people in the photos based on surroundings or other features. Although this is a feasible way to deal with a difficult situation, it also objectifies the people in the photographs (Wiles, Prosser, Bagnoli, Clark, Davies, Holland & Renold 2008) and makes them more remote and less interesting. Conversely, the alternative is not to use any photos, but merely describe them. In Newman, Woodcock, and Dunham's (2006) study on bullying it was also essential to blur or box out faces to protect the children, but the photographs still give a sense of the bullying that gives additional information and understanding compared to a mere written description.

A similar situation emerged in the study of elementary school students' sense of community. They had to take their own photos because much of their community was located at home, centered around



Figure 19.7 Researcher (right) discussing with a research participant (left).

Reprinted with permission from Janhonen-Abreuquah, H. (2010). *Gone with the Wind?: Immigrant Women and Transnational Everyday Life in Finland*. University of Helsinki. <http://urn.fi/URN:ISBN:978-952-10-6136-3>

their families, pets, toys, and bedrooms—places not accessible to the researcher.

PREPARATIONS FOR PARTICIPATORY ETHNOGRAPHY

Even though many people have some experience with cameras and photography, it is useful to have a session before the project to talk about the basics of photography. Even taking photographs for a research purpose requires some planning. For example, it might be useful to talk about how light and colors influence how a photograph is perceived (see Holm, 2008a). Likewise, it is useful to talk about literal and metaphorical photos. How does one take photographs of abstract or missing things? Can the photographers manipulate their photos, now that it is fairly easily done if they have access to computers? Can the photographers bring an unlimited number of photographs, or do they have to pick a certain number of the most important ones? How will the participants deliver their photos to the researcher?

The issue of manipulation is no more important when using photography as a data collection method than in using other methods in qualitative research. Unethical researchers can always manipulate data. Interview and observation sections can be left out as easily as photographs are left unanalyzed. However, all manipulation is not the same. If it is

the participants who manipulate/edit their own photographs, it could also be considered part of the data. Unedited and edited photographs could, for example, be compared to study differences between the current and desired situations. The difference between posing for a photo where clothing, pose, expression, and surroundings are arranged and editing a photograph can be marginal. They are both ways of arranging the photo to convey an intended message. The researcher manipulating photos for the purpose of misrepresentation is a very different issue. With digital photography, the total number of photos can become unmanageable. In a study in four countries on consumer behaviors of poor people, the group of researchers took 10,400 photos but analyzed only 612. In these kinds of cases, the question arises of why exactly these 612 were selected for analysis (Lindeman et al., 2010). A detailed description of the elimination process would help dispel thoughts of manipulation due to the selection of certain photos.

If a group of people are to take photographs, a brainstorming session is useful at the beginning of the project in which participants generate ideas about what kinds of things might be possible to photograph. This is not about telling participants what to photograph but rather to encourage them to explore as a group possibilities for constructing

and producing photos related to the research theme (Holm et al., 2014). In the study on doctoral students' perceptions of their studies mentioned earlier, we did not have a brainstorming session. When students as a group viewed everybody's photos, there was real disappointment that they had not thought about photographing certain themes they considered very important. They also discovered that, as a group, they had forgotten certain themes altogether, such as the importance of fellow doctoral students, seminars, and professors. In other words, they were so overwhelmed by the life outside the university that, in most cases, they forgot to photograph the actual university scene (Holm, 2008*a*).

Photography works well as a method for research and advocacy using the kind of concrete portrayal/documentation of problems used in photo-voice. Many researchers argue that young people are especially comfortable with and knowledgeable about photography. Many also argue that it is easier for young people and children to photograph and then discuss difficult and complicated issues. Especially when dealing with less verbal students or students with another first language, photography might be a good method (Cremin, Mason, & Busher, 2011; Lodge, 2009; Sensoy, 2011; Wilson et al., 2007).

HABITUS AND METAPHORICAL PHOTOGRAPHS

Bourdieu (1990*a*; 1990*b*) and Sweetman (2009) also argue that photography can be used for exploring

abstract and difficult-to-grasp concepts like habitus. Following their claims that photography is a possible way to explore habitus, we (Holm et al., 2014) set out to study the habitus of Swedish-language minority speaking teenagers in Finland. How do these teenagers see themselves as being a member of a language minority group, and how do they perceive the entire group? The photos they took can be divided into two kinds. One kind was of literal depictions of Swedish-speaking theaters, newspapers, street signs, and the like (Figure 19.8).

The other kind was metaphorical photos showing, for example, being a minority group member, community, togetherness, feeling threatened, and being worried about the future of the language group (Figures 19.9–11).

Interestingly, in interviews, students had difficulty explaining what it means to belong to a language minority group. They had focused mostly on the language, whereas with the photos, they brought forth a variety of different aspects. In the photos, the language was just one aspect among many. The students also tended to use photographs of nature for their metaphorical visual statements. They often said in interviews that language minority members stick together and that they have a sense of belonging. In the photos, this was expressed through nature, as in Figures 19.12 and 19.13.

The students photographed more deep-seated thoughts about the group's future and stereotypes



Figure 19.8 A literal photograph. One can understand both languages; street signs are in both Finnish and Swedish.

"Finland-Swedes= A minority"

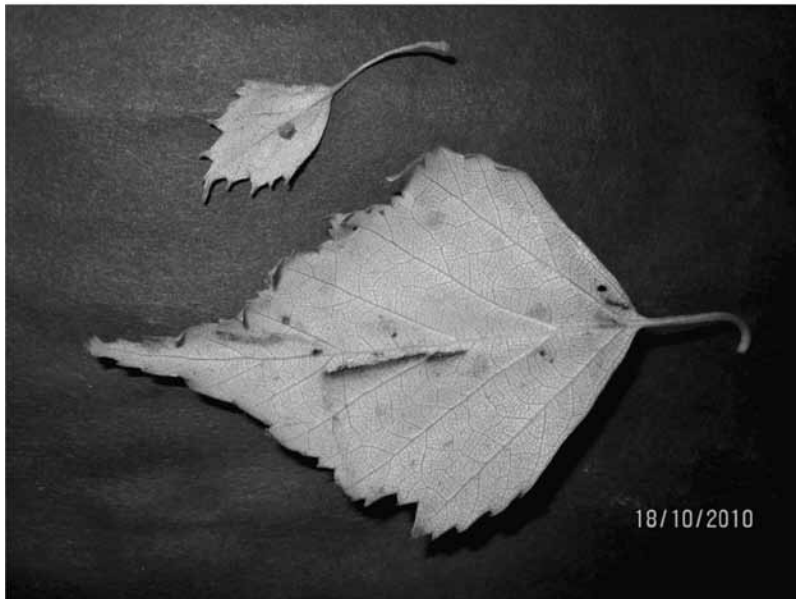


Figure 19.9 A metaphorical photograph showing the proportion of Swedes to Finns in Finland.

about the group, as well as their attachment to nature and the archipelago where many of their families originated. Likewise, Croghan, Griffin, Hunter, and Phoenix (2008) found that young people took photographs of sensitive issues related

to their identity positions such as religion and race, issues that were not brought forth in interviews.

This kind of literal and metaphorical division can also be seen in photos taken by Palestinian children and youth living in refugee camps in Lebanon



Figure 19.10 A metaphorical photograph; Finland-Swedes are melting away slowly in Finland.



Figure 19.11 Finland-Swedes are like trees in a storm. Often we just bend, but if it is storming too hard we will break.



Figure 19.12 I think this little path is like the Finland-Swedes, all the rest around are the others in Finland.



Figure 19.13 A lone swan in the big sea like a Finland-Swede.

(Mikander, 2010). They took photos to show what their lives are like. In this case, too, the children and the researcher had no common language. Here, too, there were many photos portraying their thinking, habits, and ways of being. An example of a literal photo is one of a living room wall. Interestingly, in this case, the viewer's eye is drawn to the picture of Yasser Arafat, but the child who took the photo took it to show the hole in the wall. She wanted to show how they continue to live without permanent wiring, as if their housing was temporary (Figure 19.14).

In Palestine, young people's ways of thinking about their future can best be told through a series of photographs of a burning cigarette (Figure 19.15). They start out with full lives, with seemingly a lot of possibilities and hope. Their lives shrink with age and in a metaphorical way stop when they finish school because they do not have opportunities for further education. Dreams about future families are also hampered by the severe housing shortage. Hence, their life prospects are very limited.

Other abstract aspects of life, like absence, seem to be more difficult to photograph. In a study in which doctoral students photographed their lives as doctoral students, four photos of four different students' families were very similar, but depicted different things. One was a Chinese wedding picture; another of a Korean mother, father, and child; a third one of a Ugandan mother with four children; and the fourth one of an American father with two children. In all photos, the people looked happy.

Without an accompanying text, it was impossible to know how different their intended messages were. The American photo indicated that, for this doctoral student, her husband and children were her first priority even if the doctoral studies require much of her time. However, all the other photos indicated that the international students were studying alone in the United States and were missing their families, which had remained in their home countries. Hence, the question for them had been "How do you photograph the absence of someone?" Many of the issues, like ethical questions and habitus, brought up here in relation to participatory photography are also important for other kinds of photography in social sciences. However, they are often brought to the forefront in participatory photography because the participants are in charge of taking the photographs.

Analysis and Interpretation

No one "best" specific method exists for analyzing or interpreting photographs. In social science studies, most researchers use the same methods for photographs as for text. Early books on visual research methods (see, e.g., Collier & Collier, 1986) tended to give fairly precise instructions on how to organize, categorize, and compare photos in order to be able to conduct a good analysis. All researchers have to organize and group their photographs in some way, especially when we talk about hundreds and thousands of digital photos. However,



Figure 19.14 A Palestinian child's photograph of a wall in her home; although the eye is drawn to the picture of Yasser Arafat, the child's focus is on the hole in the wall.

researchers develop their own styles, often in connection with how they analyze their textual data. Many researchers use various software programs to organize photos; others group them by hand. However, categorizing or grouping photos is just a beginning, as with textual data. According to Harper (2003, p. 195), taking and analyzing photographs is aided by theory, just as when collecting and analyzing any other kind of material. He also sees photographs as helping to build theory by forcing us to look at specific things in the field or to confirm theory. "Indeed, the power of the photo lies in its ability to unlock the subjectivity of those who see the image differently from the researcher." Theory, the researcher's own and the participants' previous knowledge and experiences, previous research, and the participants' descriptions of the photographs all contribute to an understanding of the photographs.

How the analysis of photographs is done is not discussed much, if at all, in most research reports and visual research books, even though Ball and Smith wrote about analyzing visual data already in 1992. However, there is literature on various kinds of content analysis, iconography, semiotic analyses, and interpretive and other methods (see, e.g., Margolis & Pawels, 2011; Rose, 2012). As Spencer (2011) points out, how a research study is designed, data collected, and results understood depends on the underlying paradigm. Therefore some researchers

simply state that a study was analyzed based on a particular paradigm.

Content Analysis

A mostly quantitative content analysis is used for large numbers of photographs because it gives basic information about the frequencies of certain types of photos, on the basis of which various comparisons can be made. Rose (2012) gives fairly detailed steps to be followed to conduct a reliable content analysis. She emphasizes a careful selection of images and rigorous coding. However, Rose cautions that a high frequency count does not mean that the occurrence is necessarily important. In addition, frequencies neither indicate how strongly a photo exemplifies a category nor anything about the mood of photos. The intentions of the photographer are also excluded from a content analysis. Even though the analysis is quantitative, there is also a qualitative element in the interpretation of the frequencies and the presentation of the results.

Margolis and Rowe (2011) describe their use of a grounded theory approach to content analysis, which differs substantially from the one discussed by Rose. In their approach, the coding is theoretically based, which also allows them to pay attention to absent categories. Their categories overlapped, as opposed to the usual requirement of mutually exclusivity, and they also expanded the number of

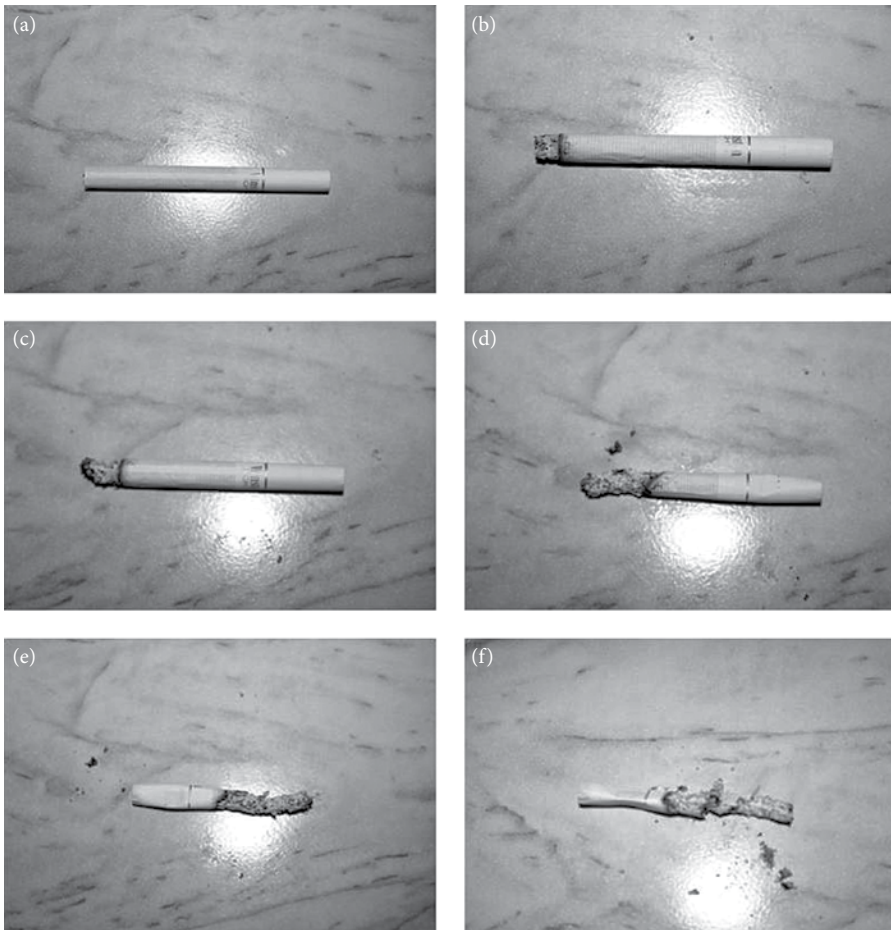


Figure 19.15 The life opportunities of a youth in Palestine are metaphorically depicted as a burning cigarette.

categories, as well as merged categories during the analysis.

Discourse Analysis

In popular culture studies, as well as in other social science research, various forms of discourse analyses are used in the analysis of photographs in relation to text. There is no specific visual discourse analysis, but Spencer analyzes specific images as examples of the use of discourse analysis. Rose (2012, p. 195) makes a distinction between discourse analysis (DA) I and (DA) II, describing DA I as paying “rather more attention to the notion of discourse as articulated through various kinds of visual images and verbal texts than it does to the practices entailed by specific discourses.” Discourse analysis II she describes as working “with similar sorts of material, but is much more concerned with their production by, and their reiteration of, particular institutions and their practices, and their production of particular human subjects” (p. 227).

Rose gives highly detailed and in-depth descriptions, with examples of how to conduct these kinds of discourse analyses. However, here it can be useful to remember that there are many different ways of doing discourse analysis (e.g., see Laclau & Mouffe, 1985).

Ethnographic Analysis

Many researchers use some kind of interpretivist analysis without being specific about it. Pink (2007, p. 117) summarizes the ethnographic approach very well:

The academic meanings that ethnographers give to visual images are also arbitrary and are constructed in relation to particular methodological and theoretical agendas. Individual researchers classify and give meanings to ethnographic images in relation to the academic culture or discipline with which they identify their work. Moreover, ethnographers are themselves subjective readers of ethnographic images and their personal experiences and aspirations also inform the

meanings they invest in photographs and videos. A reflexive approach to classifying, analyzing and interpreting visual research materials recognizes both the constructedness of social science categories and the politics of researchers' personal and academic agendas.

Hence, an ethnographic approach entails using one's already established or newly developed ways of organizing data. This organization and categorizing or beginning analysis might be quite intuitive and begin in the field. In many cases, the field and academic work intersect on a weekly basis, which influences how the researcher sees the data. In the academic setting, photographs are interpreted more closely from particular paradigms and theoretical frameworks and thus receive different meanings than in the field. In this kind of ethnographic approach, text and photographs are equally important and interact and inform the understanding of each other, as well as the relations between the two. The categorization in this approach differs from earlier approaches (see Collier & Collier, 1986) in that photos might be grouped in several different ways. They can, for example, be grouped according to the content, symbolic meaning, or origins of the photographers. Neither is the sequential order in which the photographs are taken necessarily important for the analysis because the photographers' or participants' thinking might not be linear. Rather, the way participants think about the way the photographs connect to themselves and their worlds might be more important.

At times, text and photographs might produce different but connected stories. Harper (2004, p. 232) describes, with regard to Agee and Evan's work on sharecroppers during the Depression, how the text and photos are juxtaposed and where "neither form repeats or replaces each other. Rather they develop in tandem." In my research on the schooling of teenage mothers, the photos told the story of happy, playful girls posing alone or with other girls, but always without children. This was the story they wanted to show to outsiders. The text, on the other hand, told the story of the girls' more private thoughts about their unhappy childhoods of abuse and abandonment, as well as their worries about being young mothers, often without any support network. Together, the two stories give a much fuller view of the girls than either one separately (Holm, 1995).

Issues in Interpretation

The context of the production of the photos can be important. In our study of minority language teenagers' perceptions of their own identifications,

the geographical region in which they lived and produced their photos was closely tied to their identifications. Likewise, the larger societal context with regard to the general standing of the language minority group turned out to influence how worried the teenagers were about the future of the entire group. The academic context in which the photos are interpreted produces interpretations different from the ones in the field.

The interpretation of the photos will always vary somewhat from person to person depending on previous experiences. An interesting question arising here is how much the researcher needs to know and understand of the context in which the photo is taken. How much of the historical context do we need to understand in order to interpret archival photos? On one level, we can of course make some sense of photos of people living in difficult circumstances (as, for instance, during the Depression), but without the knowledge of this historical context our interpretation will be very superficial. Likewise, how much of the context do we need to know and understand of the participants who have taken photographs?

As researchers, we found in our study of the Swedish-speaking students' photographs (see earlier description of the study; Holm et al., 2014) that having a habitus similar to the participating photographers facilitated the understanding of their photographs. Metaphorical photographs were especially easier to interpret. For example, photographs of the feeling of being harassed or that the future is somewhat insecure for the minority group immediately rang a bell in us. We had all had that feeling or experience at one point, although in different settings. Figure 19.16 shows the sun disappearing like the Swedish language is doing according to the student, and this feeling of doom is familiar to all Finland-Swedes, like the participants and the researchers in this case, who live in areas where the Finnish language is dominant. Without the text (or without an interview about the photos), this photo would simply be a photo of a beautiful sunset. Outsiders would get some sense of the situation from the text, but for the researchers living in the same societal context, the photo immediately brings to mind the larger debates about abolishing compulsory Swedish-language instruction from schools, hostile comments by members of an anti-Swedish (and anti-immigration) party, personal comments that Swedish speakers should emigrate to Sweden, and the like. Hence, knowing the societal context helps the researchers to more fully understand the deep thinking of the student taking the photo.



Figure 19.16 Swedish is disappearing from Finland (photo taken by Eva, a student participant).

In analyzing and interpreting photographs taken by participants, it is important to pay attention to photographs not taken as well, since they can be important. They can be missing because it is too difficult or painful to find ways of showing one's thoughts, as Frith (2011) found in her study of women in chemotherapy who did not have enough energy to take photographs when they were feeling most ill. Other issues might be too intimate or sensitive. Missing photos can also be due to restrictions placed on the participating photographers by gatekeepers (Holm, 1997).

There are numerous books about different kinds of analyses of photographs and visual data in general. However, most researchers do not recount in their articles what kind of analysis has been used. In the methods section of articles, researchers discuss what kind of data was collected and how it was collected, but few proceed to discuss what was done with the data after it was collected. Mostly, the data were "analyzed." Some use phrases like photographs "can be read," "in line with the social constructionist paradigm," "we looked for salient patterns/images/issues," and the like. The reason for this lack of discussion about the actual analysis might be that there is not one specific approach and that the field is relatively new for many researchers. Many researchers treat photos in the same way as verbal texts, but often not even basic information is provided about how this was done. Some researchers mention that photographs were categorized, but usually there is nothing more explicitly said about the analysis or interpretation.

Presentation of Research Using Photography as a Research Method

In social science studies, the most common way to present research using photography is still to translate most of the photographs into text, although more journals are willing to publish a few photographs as part of an article. However, only journals like *Visual Studies*, *Critique of Anthropology*, and *Visual Communication* will publish photo-essays in which most of the article consists of photos accompanied by short texts or captions and with the participants' story (Banks, 2007). There also tend to be more photos in books and book chapters than in journals. Pink (2007) discusses the possibilities of hypermedia presentations both in the form of CD ROM, DVD, and internet-based formats. Hypermedia holds a lot of potential for presenting multimodal data, but, as Pink also points out, has increased risk for manipulation of data that might change the importance and meaning of photographs, even though CD ROM and DVD provide limited access. E-journals are ideal venues because some of them, like *Forum: Qualitative Social Research*, are open-access journals and publish photography-based articles. Hypermedia online journal articles, like a special issue of *Sociological Research Online*, edited by Halford and Knowles, go a step further than regular online publishing by including, for example, live video clips. Although some researchers publish their work using photographs on websites, this is not a realistic option—at least not as the only venue—because most

researchers today work in institutions requiring publishing in refereed journals.

Ethical Issues in Photography as a Method

Ethical issues have emerged throughout the chapter with regard to gaining access, securing informed consent, and promising confidentiality. Of foremost concern in photographic research is whether participants understand what informed consent means and for what purposes the photographs can be used. Institutional review boards are especially strict with regard to protecting participants from harmful or compromising photographs. However, many argue that it is not possible to foresee all possible situations in advance but that giving consent should be ongoing during the entire study (Pauwels, 2008; Wiles et al., 2012). It is possible to produce consent forms in which participants specify what kind of uses they give consent to. For example, some participants may allow their photographs to be used for analysis but not for publication. Other participants might not want anonymity but instead want the viewers to know who they are and/or that they have taken a particular photograph (Grinyer, 2002; Wiles et al., 2008), although this is not always possible if others are involved in the study. Conversely, there can be difficulties with photo release forms if someone is suspicious of signing forms (Banks, 2007) or cannot understand the language or meaning of the form.

Ultimately, the researcher must make judgments about ethical issues surfacing during the course of the study. Respecting participants' rights to refuse to be photographed or to photograph certain things has to be respected at all times. Likewise, it has to be possible to withdraw from the study at any time. In describing the difficulties of taking photos of very poor consumers in four different countries, Lindeman et al. (2010, p. 9) describe how the fieldworkers were torn about doing what the study required in just a couple of weeks fieldwork or respecting people's right not to want to be photographed or have their poor homes photographed. "The issue of interfering in peoples' lives was also present when we wanted to take photos and videos. In principle we always asked for permission before filming or taking pictures, but in some instances we also had to take sneak picture of things of high importance to the research." In the pressure to collect data quickly, they made poor ethical decisions.

Researchers using previously taken photos as well researchers working with new photos face questions of ownership and copyright (Pink, 2007; Rose,

2012). With regard to new photos, some researchers try to prevent potential problems by stating the ownership on the forms for permission to conduct research. This might be a good idea, especially if the participants take the photos and think of them as their own.

Overall, collaborative research in which the photographs are more of a co-production might be a more ethical approach to visual research. Giving copies to and discussing them with the participants whenever possible is also a way to give the participants a better sense of which photos will be used and how they will be used. In using photography as a research method, the one aspect present in all studies and throughout the studies from the beginning to the end is the responsibility of the researcher to make good ethical judgments to produce research that does not harm participants in any way.

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Arts-Based Research Practice: Merging Social Research and the Creative Arts

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Abstract

Arts-based research (ABR) is a rapidly growing methodological genre. ABR adapts the tenets of the creative arts in social research in order to make that research publicly accessible, evocative, and engaged. This chapter provides a retrospective and prospective overview of the field, including a review of some of the pioneers of ABR, methodological principles, robust examples of ABR within different artistic genres, assessment criteria, and the future of the field.

Key Words: Arts-based research, creative arts, qualitative research, public scholarship

An Overview of Arts-Based Research

Arts-based research (ABR) emerged between the 1970s and the 1990s and now constitutes a significant methodological genre (Sinner, Lego, Irwin, Gouzouasis, & Grauer, 2006). Arts-based research adapts the tenets of the creative arts in social science research projects. These practices developed out of a convergence of factors within a transdisciplinary context and have been further propelled by technological advances. This partnership between artistic forms of expression and the scientific process integrates science and art to create new synergies and launch fresh perspectives (McNiff, 1998).¹

For social researchers, the appeal of the arts is broad because these forms can promote autonomy, raise awareness, activate the senses, express the complex feeling-based aspects of social life, illuminate the complexity and sometimes paradox of lived experience, jar us into seeing and thinking differently, and transform consciousness through evoking empathy and resonance (Barone & Eisner, 2012; Finley, 2008; Leavy, 2009). Free from academic jargon and other prohibitive barriers, the arts have the potential to reach a broad range of people and to be emotionally and/or politically evocative for diverse audiences.

Arts-based research draws on the oppositional, subversive, transformational, and otherwise resistive capabilities of the arts (Leavy, 2009). Tom Barone and Elliot Eisner (1997) observe that ABR permits ambiguity and frees the researcher to use aesthetic form and expressive language. Suzanne Thomas (2001, p. 274) writes: “Art as inquiry has the power to evoke, to inspire, to spark the emotions, to awaken visions and imaginings, and to transport others to new worlds.” The arts can assist researchers as they “aim to portray lives,” illuminating untold stories (Cole & Knowles, 2001, p. 211).

Because ABR requires a novel worldview and covers such expansive terrain, some consider it an emergent research paradigm (Gerber et al., 2012; Huss & Cwikel, 2005; Rolling, 2010). An ABR ontology would recognize that artistic, intersubjective realities are emergent and shifting, dialectical, hard to pin down, and difficult to convey in standard modes (Hogan & Pink, 2010; G. Sullivan, 2010). Epistemologically, artistic form aids sensorial experience because, through the arts, we come to witness and to know embodied sounds, movements, images, and stories (McNiff, 2008; Neilsen, 2004; Pink, 2009). Art can access inner life

through stories, metaphors, and symbols, which are recognized as both real and valuable (Saldaña, 2011). From this philosophical perspective, necessary knowledge includes expressions of lived moments and openness to further emergent meanings and new ideas. Multisensorial communication of this knowledge has the potential to elicit transformative change (Cole & Knowles, 2008). Such an arts-based axiology values not one “objective” truth, but many personal and/or intersubjective truths that may manifest across intertwined social, emotional, cognitive, embodied, artistic, and spiritual realms (Bickel, 2005). Traditional definitions of art and beauty are expanded to emphasize an aesthetic that is participatory, which aims to increase human understanding, capacity for empathy, and positive transformation (Finley, 2003; McNytre, 2004). On principle, this research paradigm generates knowledge and provokes inspiration through art-making practices and processes (Barone & Eisner, 2012; McNiff, 1998). It is resistant to oppression by dominant paradigms and prevailing authorities, and it generates fresh energy and insights from our human capacity for creativity (Leavy, 2009).

With broad applications throughout social sciences disciplines and beyond, challenging traditional practices of both science and art, the arts as research is a substantial area to cover. In this chapter, we discuss pioneers of this methodological domain, identify recent advances, and detail the specific strengths of this form of research. In addition, we provide robust examples of ABR in literary, performance, and visual art genres and conclude with our vision of future directions. As a caveat, we note that this chapter provides a broad overview of a complex topic and is necessarily incomplete.

Pioneers in Art-Based Research

Cultures throughout the globe have evolved art-making practices to discover, transform, and celebrate life; in this sense, art as a tool for research is nothing new (Davies, 2005; Dissanayake, 2000, 2003; Kossak, 2012). We have even found the specific term *artistic inquiry* dating as far back as 1859, in the Archaeological Institute of Great Britain and Ireland’s catalogue of antiquities. In Europe, Swiss psychologist Carl Jung (2009) created art imagery as inquiry between 1914 and 1930, in a vivid self-exploration only recently published as *The Red Book*, which he identified as pivotal to his widely significant later work. In 1940, American philosopher of art Theodore M. Greene used the phrase *artistic inquiry* to reference artists engaged

in “knowledge-seeking and knowledge-yielding enterprises which deserve to be ranked with science as a method of inquiry into truth” (Tomas, 1940, p. 459). At the time, the dominant mindset rejected such conceptions of truth. The idea of art-as-research *as a scientific endeavor* was only revived after the social justice/postmodern movements of the latter half of the twentieth century shattered dualistic, positivist conceptions of truth and science and broadened what was considered acceptable within formal academic research (Leavy, 2009; G. Sullivan, 2010). Although this work was and is going on internationally, holistic arts-based approaches, often conducted by women, indigenous people, and people of color in local and ephemeral modes of practice, have historically been delegitimized and marginalized (Clover, 2010; 2011; Finley, 2011; Kapitan, Litell, & Torres, 2011).

In the 1970s, art educator Elliot Eisner (1975) began popularizing *arts-based education research* (ABER); he has continued to be a vocal advocate for the arts in research over the next forty years (Barone, 2005; Knowles & Cole, 2008). Simultaneously, creative arts therapists were developing artistic inquiry as a tool for self-discovery for clients and therapists alike in psychotherapeutic contexts (Allen, 1995; 2012; Fish, 2007; 2012; Moon, 2012; Wadson, 2003). These practices, wherein researchers utilized art making as a primary form of inquiry, were identified as *art-based research* by Shawn McNiff (1986; 1998; 2011) and others who pioneered work in this area (Hervey, 2000; Kapitan, 2003; Landy, 1993). Arts-based therapists and arts-based educators called for ABR, identifying that worldwide practices of coming to know through aesthetic means were of value and rejecting the modernist art versus science divide (Eisner, 1981; McNiff, 1998).

At the same time, qualitative researchers influenced by postmodernism in social science fields such sociology and anthropology were seeking ethical, aesthetic, and transformative ways to conceive and represent experiential findings (Finley, 2011). New kinds of qualitative research across disciplines were innovated in the 1990s utilizing the power of the aesthetic, such as the development of *autoethnography* by leading figures Carolyn Ellis and Art Bochner (Ellis, 2009; Ellis & Bochner, 1996) and *photovoice* by Caroline Wang and Mary Ann Burris (Wang & Burris, 1997). These research practices blurred boundaries between social sciences and the humanities and combined to build a larger movement for more artful, personal, and humane practices in research (Bochner & Ellis, 2003).

Art-based inquiry was linked to other qualitative research paradigms, such as narrative, hermeneutic, heuristic, and phenomenologically based perspectives, as art making in all its modalities was conceptualized as a form of direct experience (Kapitan, 2010). Interest in examining and learning from intersubjective lived experience grew throughout many kinds of interpretive and cultural studies. Through various historic postmodern and postexperimental developmental moments since the 1970s, researchers addressed challenges in representation and positionality, using multiple methods to make sense of phenomena situated in natural settings (Denzin & Lincoln, 2011a). In this context, art-based and narrative approaches were embraced as potentially able to provide the participatory, ethical, multi-voiced processes that practical-minded researchers concerned with social justice sought (Finley, 2008; Leavy, 2009; McLean & Kelly, 2010).

Arts-informed dissertation research grew rapidly and, with it, new understandings of the barriers, challenges, and rewards of such work (Atkins, 2012; Knowles, Promislow, & Cole, 2008; Sinner et al., 2006). In individual genres, such as fiction, poetry, music, dance, theater, and the visual arts, creative researchers forged synergistic blends of art and science. Fiction as research practice, pioneered by Stephen P. Banks and Anna Banks (1998), Douglas Gosse (2005), and Patricia Leavy (2011) has grown exponentially in recent years. In 2005, Gosse published his research-informed novel, *Jackytar*, based on award-winning research. In 2008, Leavy launched the *Social Fictions* book series, which exclusively publishes fiction-based research, and in 2013, she published the first introductory book on this topic, titled *Fiction as Research Practice*. Poetry as a form of research representation also took off with the work of researchers Cynthia Canon Poindexter (2002), Laurel Richardson (2002) and Monica Prendergast, Carl Leggo and Pauline Sameshima (2009), and Sandra Faulkner (2009). For instance, Faulkner (2009) published a book about how poetry can be used to represent research; extensive examples from her own work illustrated how she used poetry to represent her research on identity in people who identified as lesbian, gay, bisexual, transsexual, or queer (LGBTQ) and Jewish. Likewise, Laurel Richardson's (2000) and Anne Sullivan's (2000) use of writing as inquiry provided powerful examples of how to shift the dialogue using lyrical form.

Others explored the possibilities of music as research (Richardson, 1993). Terry Jenoure

(2002) used the principles of jazz improvisation in her musical portraiture work. Dancer and educator Don Blumenfeld-Jones (1995) innovated dance as a mode of research representation at the American Educational Research Association (AERA), whereas dance movement therapists and educators identified ways that dance could extend the knowledge possibilities of embodiment to research (Bagley & Cancienne, 2001; 2002; Hervey, 2000). In 2000, Johnny Saldaña, Macklin Finley, and Susan Finley's (2005) dramatic play *Street Rat*, regarding street-involved youth in the US city of New Orleans, provided an important example of theater-as-research (Barone & Eisner, 2012). Performance ethnography, pioneered by Johnny Saldaña (1999), Norman Denzin (2003), and others, exploded in various forms over the next decade in the US (Saldaña, 2011; Valle & Connor, 2012). Using film to represent research findings is another way that researchers contribute to what Kip Jones (2012) deems "performative social science." Theory and praxis in the visual arts grew as well, with increased attention to the visual in ethnography and across methodologies serving a more complex understanding of culturally mediated form (Pink, 2007; Pink, Hogan, & Bird, 2011; G. Sullivan, 2010).

With this wave of methodological development came multiple terms for these practices. Variations in focus and nomenclature arose, such as those who identified as practicing "a/r/tography," in which researchers were to "ask oneself questions that linger between, amid, and/or within visual/textual, theoretical/analytical, and pedagogical/curricular matters" (Springgay, Irwin, & Kind, 2005, p. 902). Emphasizing inquiry and reflective practice through visual art creation, research, and teaching, a/r/tography "frames its methodology around renderings...conceptual organizers of ideas used by a/r/tographers to interpret qualities deemed significant during an artistic or creative process" (Beare, 2009, p. 164).

Following Patton's (2002, p. 85) partial lexicology of autoethnography, we constructed a table that incompletely lists these terms (see Table 20.1). Each term can be somewhat differently defined, and this has led to confusion about the differences between art or art creation processes as a subject of research, as a means to produce data, as a means to analyze data, as a means to represent data, and/or multiple varieties and combinations of these uses, even as thought leaders tried to provide clarity in this "new era in academic research" (Finley,

Table 20.1 Partial lexicology of terms for arts-based research

A/r/tography	Arts in qualitative research
Alternative forms of representation	Arts-based educational research (ABER)
Aesthetically based research	Arts-based health research (ABHR)
Aesthetic research practice	Arts-Based Research Practices
Art as inquiry	Arts-Informed Inquiry
Art practice as research	Arts-Informed Research
Art-based enquiry	Critical Arts-Based Inquiry
Art-Based Inquiry	Living Inquiry
Art-Based Research	Performative Inquiry
Artistic Inquiry	Practice-Based Research
Arts-based research (ABR)	Research-Based Art (RBA)
Arts based social research (ABSR)	Research-Based Practice
Arts-based qualitative inquiry	Scholartistry
	Transformative Inquiry through Art

2011; McNiff, 2011; Sinner et al., 2006, p. 1226). The consequences of this excess of creativity include complex definitions and unclear boundaries in the realm of ABR that inhibit researchers and prevent access by local co-creators (Finley, 2011; Ledger & Edwards, 2011).

Recent Advances in Arts-Based Research

The excess of terms also reflects the burgeoning state of the art. As we write, new textbooks are being published, new research is being conducted, and new book series are in development. It is an exciting time of growth in this area, and we cannot possibly accurately list all publications here. The recent advancements in the literature in the past fifteen years have pushed ABR to a new level of influence in academia. In fact, there has been so much published on ABR that we can only point to a handful of examples. In 2002, Carl Bagley and Mary Beth Cancienne released the edited volume *Dancing the Data*, which covers five genres of ABR. In 2004, Ardra L. Cole, Lorri Neilson, J. Gary Knowles, and Teresa C. Luciani compiled the edited volume *Provoked by Art: Theorizing Arts-Informed Research*, which seeks to pull together the theoretical contributions of this new research paradigm. Graeme Sullivan (2010) wrote an important text, first published in 2005, on visual arts practice as research, in which he outlined the postmodern philosophical grounds linking ABR, arts-informed research, and a/r/tography. Also in 2005, Paul's text, *Introduction to the Philosophies of Research and Criticism in Education and the Social Sciences* included ABR as one of nine distinct philosophical perspectives. In 2008, a significant moment in the history of ABR occurred with the publication of a 700-page

overview, the *Handbook of the Arts in Qualitative Research* (Knowles & Cole, 2008), firmly establishing the presence of arts in research in the landscape of academe. This text covered a wide range of approaches and disciplines, identified methodologies as well as issues and challenges, addressed both new and folk media, and in so doing presented the scope as well as significance of the arts-in-research. The recent publication of widely accessible texts such as Leavy's *Method Meets Art: Arts-Based Research Practice* (2009) and Barone and Eisner's *Arts Based Research* (2012) has increased the teaching of ABR across the academy. There has also been a proliferation of books that cover specific arts-based practices, some of which have already been noted.

Strengths of Arts-Based Research

A HOLISTIC AND TRANSDISCIPLINARY APPROACH

Arts-based research practices developed in a transdisciplinary methods context in which disciplinary, methodological, and theoretical borders were crossed (Leavy, 2011). Arts-based approaches therefore do not belong to the domain of any one discipline alone. Furthermore, their practical value lies in their ability to integrate and expand on existing disciplines and synergies between disciplines.

At their best, ABR practices are employed as part of a holistic or integrated approach to research (Hunter et al., 2002; Leavy, 2009; 2011). A holistic approach to research design explicitly links each phase of a research project while merging theory with practice. A problem or research topic is considered comprehensively, without compartmentalizing different components of the issue based on artificial disciplinary boundaries. Rather, various bodies of knowledge and expertise may be brought

to bear on the topic. Methodologically, a holistic approach is a process-oriented view of research (Hesse-Biber & Leavy, 2010; Leavy, 2009; 2011). In this regard, the intuition and flexibility needed to foster ABR is enabled through evolving or responsive approaches to research design. In other words, these approaches involve reflexive and responsive approaches in which new insights, new learning, and unexpected data (and possibly ongoing group conversation) propel cycles of returning to reanalyze data and/or revise aspects of the research design as needed.

EVOKE AND EXPLORE META-COGNITIVE EXPERIENCE

Arts-based research can be particularly useful for research goals that aim to describe, explore, or discover social, emotional, and other meta-cognitive experiences (Leavy, 2009; McNiff, 1998; G. Sullivan, 2010). Arts-based methods generate data beyond the scope of typical qualitative research interviews or participant-observation processes, creating opportunities for enhanced engagement among those participating at all phases of the research process, including the research audience (Boydell, Gladstone, Volpe, Allemang, & Stasiulis, 2012). These forms of art-as-research have exceptional “capacity to capture the essence of the experience, at the same time as stopping us in our tracks, binding us once and forever with something far greater than ourselves,” keeping our deepest, most soulful experience front and center (Kenny, 2002; Meekums, 2011, p. 383). Because art can provide unique access to interior life, it becomes a conduit to intimate self-knowledge and knowledge of others, accessing meta-verbal ways of knowing that are particularly effective in evoking empathy and transformative understanding (Gerber et al., 2012; Hogan & Pink, 2010; McNiff, 2008).

The results of such research are often emotionally and politically evocative because of the aesthetic power of the work (Barone & Eisner, 2012). The immediacy of art provides viscerally felt sensorial experience, an embodied knowledge that is effective at communicating emotional aspects of social life (Leavy, 2009). This empathetic understanding can provide deep insight that is empirically different from cognitively acquired knowledge. Subjective and intersubjective states such as embodied, emotional, aesthetic, relational, spiritual, and intuitive understandings are accessed by using “a larger spectrum of creative intelligence and communications”

that expands the scope of research generation, interpretation, and re-presentations to present findings that can feel more accurate, vibrant, and authentic than more conventional presentations of scientific knowledge (Bickel, 2005; Jaggar, 1989; McNiff, 1998, p. 30).

ETHICS/SOCIAL JUSTICE

The communicative power in these artistic research practices is not lost on those who want their research to have strong impact. This work is known for raising critical awareness of injustice and oppression in participatory and action-oriented ways (Leavy, 2009). It is “an organizing system that informs by eroding predeterminations, un-naming categories, and swamping the pretense of objectivity” (Rolling, 2010, p. 108). For example, Clover (2011) documented her participation in an extensive feminist ABR project with homeless (street-involved) women in Canada. Over the course of the eighteen-month project, trust and a sense of community grew along with newly formed identities as artists, which provided those involved with individual and collective empowerment to challenge stereotypes. Worldwide, this is one of many ABR projects that have given voice to subjugated perspectives, stimulated dialogue, and enacted transformation (Leavy, 2009).

Many researchers interested in novel, ethical, and noncoercive ways to promote hopeful dialogue, make room for critical consciousness, disrupt taken-for-granted assumptions, “problematize dominate ideologies” (Leavy, 2011, p. 106), and enact social change come to ABR research practices to further these aims (Denzin & Giardina, 2009; Knowles & Cole, 2008; Morita, 2006). As Israeli art therapists and arts-based researchers Huss and Cwikel learned through their work with Bedouin women:

by handing over creativity (the contents of the research) and its interpretation (an explanation of the contents) to the research participant, the participant is empowered, the relationship between researcher and research participant is intensified and made more equal, and the contents are more culturally exact and explicit, using emotional as well as cognitive ways of knowing. (2005, p. 45)

These practices are congruent with social justice goals because employing arts-based ways of knowing provides opportunities to conduct research that liberates voice and embraces a pluralist community of inquiry (Bradbury & Reason, 2008).

INNOVATIVE DISSEMINATION PRACTICES

Traditional conceptions of dissemination have been criticized as produced by technical rationalist ontologies that limit more nuanced approaches to the complex ethical, political, and communicative issues embedded in dissemination practices (Barnes, Clouder, Pritchard, Hughes, & Purkis, 2003). Aesthetic form provides a distinct means of expression for the construction, translation, communication, and dissemination of knowledge (Langer, 1953). As a result, a particular strength of artful research practices is the way that meaning is communicated with aporetic, aesthetic force. For example, in recent survey research on dissemination practices, Lafrenière and Cox (2012b) found that a theater performance was more effective in communicating research findings than traditional forms of dissemination because it evoked empathy and other complex emotional responses, as well as generating almost five times more questions. Further research is needed to explore in depth how the special action of the arts uniquely produces and disseminates knowledge and which art forms might be best suited for communicating specific kinds of data (Boydell et al., 2012).

Additionally, the wider movement toward greater community engagement in academia has involved the emergence of “public scholarship,” in which research is conducted in partnership with communities, raising “practitioner voices as cogenerators of knowledge” (Giles, 2008, p. 104). Arts-based research facilitates this movement to involve the wider community through means such as theater productions, poetry readings, art shows, and online experiences (Bagley & Cancienne, 2002; Leavy, 2009). However, arts-based practices are also often local, ephemeral, and undocumented, commonly only presented to limited audiences or not even identified as research (Finley, 2011; Lafrenière & Cox, 2012a; Ledger & Edwards, 2011). Yet these forms can provide unique opportunities for enhanced public engagement and learning, livening content and making it accessible, useful, and potentially desired in communities beyond academia (Boydell et al., 2012; Keen & Todres, 2007). The transformative capacities of this work present both opportunities and challenges. “When research results are presented as art, and public access to the work is both enabled and deliberately arranged, our recontextualization of research participants’ stories and lives become audible, visible, felt by them, in visceral and potentially lasting ways” (Sinding, Gray, & Nisker, 2008, p. 465). This can

produce powerful and unpredictable consequences. Therefore, although artful communication strategies can provide exemplars of best practices for qualitative research dissemination, they also present unique ethical issues involving the politics of representation, authority, and voice (Keen & Todres, 2007; La Jevic & Springgay, 2008).

Genres of Arts-Based Research with Exemplars

As noted, there are numerous genres in which ABR is practiced, and artist-researchers have been known to blend and bend any clear-cut genres. Moreover, each arts genre is so rich that it requires books devoted solely to the practice at hand. Therefore, we have decided to focus our review on three major genres of ABR: literary, performance, and visual art. For in-depth treatments of additional genres, such as music and dance, please see Leavy’s (2009) introductory textbook, *Method Meets Art*.²

Literary Genres

Historically, qualitative research has centered on words and writing as opposed to numerical findings, as is often also the case with quantitative research. Therefore, the recent turn to literary genres of writing is an extension of what qualitative researchers have long done. Literary approaches to writing qualitative research are meant to produce well-written, engaging, and evocative texts that diverse readers can connect with. Traditional social science writing often lacks the qualities that would characterize good and engaging writing (Banks & Banks, 1998). By using the tools of literary writing, researchers are able to write more engaging and resonant texts. The major practices within this genre are poetic inquiry and fiction as research practice (also referred to as fiction-based research).

POETIC INQUIRY

Poetic inquiry is the creation of poetry as a research practice (Furman, 2006). In this literary arts approach, poetry is written by researchers as a tool useful for transforming raw data from surveys, interviews, transcripts, participant-observation, reflexive memos, or even original artworks into new forms to find, condense, and present essential elements. The tenets of qualitative research merge with the literary norms of poetry to produce new texts open to multiple interpretations, authorships, and perspectives (Leavy, 2009). Faulkner (2009) notes that, if handled with skill, this method as means to re-present data of lived experience is more powerful

and emotionally moving than traditional academic prose. Her text on the subject considers the use, craft, and practice of research poetry and highlights research poetry's capacity to increase the understanding of sensorial, social-emotional nuance; promote empathic experiences; and address social justice. "Poetry makes writing conspicuous and pays attention to particulars in opposition to transparent invisible scientific writing that focused on comparative frameworks" writes Faulkner (2009, p. 25).

The research purpose and aims drive the choice of the type of poetry. Furman's (2006) qualitative health research is illustrative as he demonstrates three different poetic forms to examine his experience as an emergency room patient. The first poem, a free verse about his frightening but ultimately non-life-threatening emergency room visit, acts as raw data (akin to an interview). He then carefully coded the poem for thematic elements using traditional open and axial coding methods. This coding produced a congruence of themes such as the fear of dying, fear of medical procedures, and the desire to live. Using these themes, he re-presented the data in two traditional poetic forms, the *pantoum* and the *tanka*. In the *pantoum*, Furman created lyric verse in quatrains, which employed the repetitious structure to powerfully portray his experience. Finally, Furman uses the short poetic form of the *tanka* (similar to a *haiku*) to focus on the emotional essence:

Tears, pouring down lips
who will attend the funeral?
Feigned calmness and tears
they force tubes through your nostrils
you contemplate life without you.

(2006, p. 565)

Attention to the poetic craft is highlighted in this work. We can see and feel how Furman's poem successfully "evoked a snippet of human experience that is artistically expressed as in a heightened state" (Leavy, 2009, p. 64) because the selection of words, space, and form is skillfully constructed.

A researcher knowledgeable in the variety of literary forms of poetry can determine which forms are most useful for his or her research purposes. As Furman (2006) demonstrated, different kinds of poems evoke very different cultural meanings. For example, *found poetry* shapes interview quotes yet keeps the participant's words front and center, *free verse* employs creative images to re-story the data, the *modern sonnet* adapts a formal structured

poem traditionally concerning love, and *rap* and *spoken word* poetry defy convention and incite raw truth-telling through use of rhythm and rhyme (Faulkner, 2009; Furman, Lietz, & Langer, 2008). Various forms of poetry can also be used during data analysis processes for artistic translation, reflective contemplation, and subsequent meaning-making (Kusserow, 2008; Manders & Chilton, 2013). Details on the technique of poetic transcription using coding processes derived from grounded theory (Glaser & Strauss, 1967) can be found in Leavy (2009, pp. 75–91). Attention to the poet's craft, especially including the revision process, can aid discovery and deep empathetic understandings of complex research data (Faulkner, 2009; Poindexter, 2002).

FICTION AS RESEARCH PRACTICE

Fiction as a social research practice is a natural extension of what many researchers and writers have long been doing. There has always been a winding road between research practice and the writing of fiction (R. Franklin, 2011). Stephen Banks (2008, pp. 155–156) writes that "the zone between the practices of fiction writers and non-fiction writers is blurry" because fiction "is only more or less 'fictional.'" Furthermore, there has long been an explicit merging of the tenets of fiction and non-fiction in genres such as historical fiction/historical novels, creative nonfiction, and autoethnography.

Fiction writers conduct extensive research to achieve verisimilitude, similarly to social scientists (Banks, 2008; Berger, 1977). *Verisimilitude* refers to the creation of a realistic, authentic, and life-like portrayal, and it is the goal of both fiction and established social science practices like ethnography. Fiction writers and qualitative researchers both seek to build believable representations of existing or possible worlds (Visweswaran, 1994, p. 1) and to truthfully or authentically portray the human experience. The material writers use in fiction comes from real life and genuine human experience. Similarly, qualitative researchers very much shape every aspect of their investigation, imbuing it with meaning and marking it with their fingerprint.

Today, innovative researchers propelled by changes in the qualitative paradigm and arts-based researchers are harnessing the unique capabilities of fiction as a means of engaging in effective and publicly accessible research practice. These new practices are called *fiction-based research* (Leavy, 2013).

Wolfgang Iser (1997) has been at the forefront of theorizing about the relationship between the

empirical worlds we study and the fictional worlds we create. His concept of “overstepping” indicates that a “literary work oversteps the real world which it incorporates” (1997, p. 1). Iser details a threefold fictionalizing process: (1) selection, (2) combination, and (3) self-disclosure.

Selection is the process of taking “identifiable items” from social reality, importing them into the fictional world, and transforming them “into a sign for something other than themselves” (ibid., 2). Through the process of selection, we “overstep” the empirical world we aim to reference. Selection happens in conjunction with combination.

Combination is the process of bringing the different empirical elements or details together. The bits of data, empirical elements, or details we select may come from traditional research processes (such as interviews or field research), or they may come to us more abstractly through the accumulation of research, teaching, and personal experiences. Tom Barone and Eliot Eisner (2012) explain that empirical details may arise out of any social research methods, and they further suggest that “empirical elements may also arise out of careful reflections on the previous experiences of the researcher with social phenomena. The research may occur within a preproduction phase, prior to the fashioning of a text; more often it will occur within the process of composition” (Barone & Eisner, 2012, p. 104). Therefore, descriptions and details written in a work of fiction-based research can be considered “data.” The use of details from the real world brings readers into the work of fiction while allowing writers to reimagine what “real worlds” are. Barone and Eisner (2012, p. 106) write:

Familiar elements of experience do help to lure the reader into the text and enable her to vicariously inhabit the world recreated therein... [T]he imported ‘realities’... must nevertheless remain identifiable and familiar, seen as believable, credible, lest readers no longer be able to relate the recreated world to their life experiences outside of the text.

The final act of fictionalizing is *disclosure*, meaning that a text reveals its fictional status.

Fiction-based research has the chance to reach broad audiences, making the products of social research accessible, engaging, and pleasurable. In addition to reaching a diverse public, the three primary goals of social research for which fiction-based research is well suited are (1) portraying the complexity of lived experience or illuminating human experience (linking the particular and the universal,

or micro and macro levels), (2) promoting empathy and self-reflection (as a part of a compassionate, engaged, or social justice approach to research), and (3) disrupting dominant ideologies or stereotypes (including building critical consciousness and raising awareness) (Leavy, 2013).

To achieve these goals, literary tools are used. *Low-Fat Love* is underscored with a commentary about female identity-building and self-acceptance and how, too often, women become trapped in limited visions of themselves. Women’s media is used as a signpost throughout the book to make visible the context in which women come to think of themselves, as well as of the men and women in their lives. In this respect, *Low-Fat Love* offers a critical commentary about popular culture and the social construction of femininity. Ultimately, the book explores women’s identity struggles in relation to the men in their lives and how women often develop myopic images of themselves as a part of “face-saving” strategies employed to cover up shame and as a learned devaluation of self. *Low-Fat Love* suggests that women seek new ways to see that are not dependent on male approval so that they will value themselves and reject degrading relationships. Moreover, as the main characters in the book learn, the most toxic relationship a woman may participate in is often with herself. So, too, the men in *Low-Fat Love* learn that one must find one’s voice or suffer the consequences.

The main literary tools used by Leavy to build empathetic engagement include rich characterization (relatable characters), representation of interiority (through internal dialogue), and the presence of resonant details. Readers, particularly the target audience of college students, relate to the characters’ struggles and the themes in the book. The fiction-based approach therefore facilitates research objectives such as promoting conversation, reflection, and the development of critical consciousness.

Performative Genres

Performative ways of engaging in art-as-research have grown within the context of a diverse performance paradigm (a full discussion of which remains outside the scope of this chapter) in which aesthetic, critical, and participatory modes of knowing are embodied through performance, which is “always already partial, moral, and political” (Denzin, 2011, p. 654). In enactment of data, the physical body of the performer functions as a method of inquiry and a way of knowing (Pelias, 2008). Transdisciplinary work in the social sciences and beyond can be (en)acted, (en)cultured, and (em)bodied through

performative inquiry, performance ethnography, autobiographical performance, literature in performance, and so on; the doing of research through live performance, dance, installation, film, ritual, and new media technologies. In the following section, we focus in depth on three areas within the performative genre: playbuilding, ethnodrama/ethnotheater, and film. With these specific disciplines within the wider range of performative research, we hope to illustrate this form of ABR practice to those not familiar with the genre.

PLAYBUILDING

Playbuilding is the practice of producing evocative texts that are performed (Barone, 1990; Norris, 2009). Playbuilding is a topic-, issue-, or problem-centered research strategy that involves assembling a group of people to brainstorm about a topic of mutual interest (Norris, 2009). The group draws on autoethnographic observations and often on data from other sources as well, including literature reviews, newspapers, and/or fiction (Norris, 2009). Norris refers to this group of people as A/R/Tors, denoting actors-researchers-teachers. All participants are stakeholders in the research process—collaborators, partners, co-creators, and co-authors (Norris, 2009).

The playbuilding process typically is as follows:

First, is data collection (generation), followed by data analysis (interpretation), and concluded with dissemination (performance). Such is the case with ethnodrama, where data is traditionally collected, analyzed, and then disseminated through an “alternative” form of representation. With Playbuilding, data is generated and interpreted in a different manner, and, at times, these three phases are simultaneous. (Norris, 2009, p. 22).

Norris has created a record keeping system that he likens to “coding” (Norris, 2000). Conceptualizing “record keeping” or “coding” as an “emergent process,” he advocates using a series of files in which cast or team members place note cards containing their thoughts, ideas, impressions, and so forth throughout the process. Some of the files he uses are “To Be Filed,” “Themes/Issues,” “Metaphors,” “Scene Ideas,” “Rehearsed Scenes,” “Quickies” (short scenes and phrases), “Keepers,” “Props/Costumes/Music Needs,” “External Research Data,” and “Potential Titles” (p. 47). Ultimately, as the performance approaches, Norris notes that a shift occurs from collection to compilation—a process guided by the question: “What do we want

this play to be about?” (2000, p. 47). Playbuilding has to draw on the tenets of the dramatic arts in a meaningful way as it represents the data. Norris explains this part of the process as he reviews going from *data to drama*: “The A/R/Tors take the generated data and judiciously take artistic license, using metaphor, composites, and theatrical styles to create a verisimilitude of lived-experiences to create texts (theatrical vignettes) that evoke conversation (2009, p. 35).”

The outcome of playbuilding is a live performance that may, in turn, generate new data or interpretations of the data as audience members are brought into the process (Norris, 2009). For example, post-performance discussions or focus groups can be used to generate new data for the next phase in a mixed or multimethod project.

Norris has been involved in the development of more than 200 performance pieces through his group, “Mirror Theatre.” The group has tackled numerous transdisciplinary topics including bullying. The program “*What’s the Fine Line?*” presented a workshop and performance on bullying (Norris, 2009). To create this program, the team used existing literature and drew on a cross-national study conducted by Smith, Morota, Junger-Tas, Olweus, Catalano, and Slee (1999) (Norris, 2009). The study included data about girl-on-girl bullying, which often occurs in changing rooms (Norris, 2009). Female cast members (team members) shared their personal experiences with the group as well (Norris, 2009). From these data, the scene “*The Girls’ Locker Room*” was created in which issues of relational aggression were addressed.

ETHNODRAMA AND ETHNOTHEATER

An *ethnodrama* is dramatic literary writing, such as a play or film script, consisting of research participants’ stories and/or researchers’ interpretations of data (Saldaña, 1999; 2011). In this form of ABR, interview transcripts, journal entries, personal memories, historical documents, and other data are dramatized into a theatrical script. If such scripts are performed, the result is termed *ethnotheater*, according to leading expert, Johnny Saldaña (2011). However, like ABR in general, numerous terms such as docudrama, performance ethnography, autoethnographic monologues, theatrical journalism, historical reenactment, reality theater, and theatrical nonfiction exist that refer to the wide variety within theater-as-research practice (Saldaña, 2011). Despite the plethora of terms and techniques, taking “research from page to stage” is

generally done to communicate, present, and document “real” life stories in moving ways (Saldaña, 2011, p. 3). Paradoxically, this reality is then heightened, embodied, and voiced through theatrical performance to more clearly ring true.

An example of ethnographic theater was produced in Vancouver, at an alternative school for adolescents who had been labeled as at-risk. There, inspired by the aforementioned ethnodramatic play *Street Rat* (Finley, Finley, & Saldaña, 2005), a theater company of young people wrote, designed, and performed their own ethnodrama. The 2009 work, *All I Ask: A Look Into the Hardships Modern Teenagers Face* (Not At-Risk Theater Company, At Home At School, 2009, as cited in Finley, 2011) was created by young people influenced by life experiences of poverty, homelessness, Deafness, and interactions with the foster care system. This work dramatized these adolescents’ lived experiences of everyday insults to personal dignity and communicated their rejection of the “at-risk” label. The communal process of writing, rewriting, staging, performing, and conducting post-performance audience conversations demonstrated the ability of these young people to actively engage in ethnographic theater as critical arts-based inquiry, with meaningful results in the local community and beyond (Finley, 2011).

FILM

Film is another performative genre in which some ABR is occurring. There is a long history of using documentary filmmaking in anthropology. Film has always been a contested genre within anthropological research practice (Ruby, 2009). Some label filmmaking in anthropology (and closely related fields) ethnofiction and ethnographic film (Sjöberg, 2008). There has been a sharp rise in these practices, as evidenced, for example, by the Ethnographic Film Festival of Athens 2012. The growth in ABR across the disciplines, coupled with the advent of the Internet and digital technologies, has resulted in new approaches to filmmaking as research across the disciplines. Kip Jones (2012) asserts that film is a genre of “performative social science,” which is a new way of thinking about and conducting research with the aim of reaching broader audiences.

The short film (30 minutes) *Rufus Stone* was created and executive produced by Kip Jones (Reader at the Media School and HSC at Bournemouth University in the United Kingdom), directed and scripted by Josh Appiganesi (*The Infidel; Ex Memoria*), and produced by Parkville Pictures, London. *Rufus Stone* is based on a three-year

research project titled “Gay and Pleasant Land? An Interdisciplinary Exploration of the Connectivity of Older People in Rural Civic Society,” which was funded by Research Councils UK. As Jones notes, the film is an innovative way to disseminate the research findings that had been garnered through traditional research methods.

The film tells the story of a young gay man from a rural area in the United Kingdom. Rufus and his love interest, Flip, are outed by a young woman in the village, Abigail, who had made unreciprocated advances at Rufus. The outing scene, which takes place at a lake where the two young men are discovering their feelings for each other, only to be taunted by Abigail and others, is quite powerful. Everyone’s reaction to the blossoming attraction is harsh and swift. As a result, Rufus flees the town and moves to London. This is all before the opening credits! The bulk of the film then takes place fifty years later, when Rufus returns to his village in order to sell his deceased parents’ home. He is forced to confront the village people, his lost love, and his own bitter memories. I don’t want to give away any further plot points here, but there are moments of intense beauty, humor, and deep tragedy. The film is not only a glaring look at how homophobia and intolerance can shape people’s experiences, but it is also a film very much about looking at who we are, how we became who we are, and how we allow our lives to unfold. In this respect, it is a film about identity, time, and the importance of introspection.

Rufus Stone represents the best of public scholarship. The film fosters empathetic engagement and compassion and unsettles stereotypes. This illustrates how film can be used to distribute research findings to very broad audiences, illuminating a range of social science concerns and educating the public about traditional research projects. For more information on *Rufus Stone*, please visit <http://microsites.bournemouth.ac.uk/rufus-stone/>

Visual Genres

Although visual elements are, of course, present in films and theatrical performances, for clarity, the use of visual art practice as research is conceptualized here as a separate genre. Fine and folk art practices such as photography, painting, drawing, collage, sculpture, instillations, quilt making, ceramics, and so forth have provided unique aesthetic knowledge for artist-researchers skilled in these areas (M. Franklin, 2012; Knowles & Cole, 2008; Knowles et al., 2008; McNiff, 1998; G. Sullivan, 2010). Shaun McNiff (1998) and Graeme

Sullivan (2010) have both produced important texts describing basic principles and theoretical background of visual art inquiry, and the reader is directed to these sources for in-depth discussions. To elucidate the breadth of work in many visual art forms, several robust examples are outlined here to illustrate the use of photography, visual art journaling, and multiple art forms.

PHOTOVOICE/PHOTOGRAPHY AND PHOTOVOICE AS RESEARCH

Photovoice (Wang & Burris, 1997) is a specific research practice increasingly used in community-based participatory research wherein participants use photography to jump-start discussion and action.

Photovoice is a process in which people

- (1) photograph their everyday health and work realities, (2) participate in group discussions about their photographs, thereby highlighting personal and community issues of greatest concern, and (3) reach policy makers, health planners, community leaders, and other people who can be mobilized to make change. (Wang & Pies, 2004, p. 96)

With roots in participatory documentary photography, this form of arts-based action research empowers community people to photograph daily realities, thereby providing key information for needs assessment, program planning, and grassroots activism (Wang & Pies, 2004). A specific protocol is followed to ensure local voices are heard. An example is the participatory advocacy project, *Witnesses to Hunger*, in which low-income mothers in Philadelphia, a large urban city in the Mid-Atlantic United States, took photos and recorded their stories about poverty and hunger to impact social welfare policy (Chilton, Rabinowich, Council, & Breaux, 2009) (see <http://www.witnessestohunger.org/>).

Others have used participant photography as a means to increase self-expression, explore identity, and form deepening relationships between photographer and viewer as co-researchers. Janet Newbury and Marie Hoskins (2010) were interested in experiences of adolescent girls who use methamphetamines and designed a research project in which the girls took photographs while reflecting on their lives. This practice led to conversations about the photographs with the adult researchers, in this relationally and artistically based study. The photographic images elicited narratives, which arose from the relationships between the adults and girls as co-researchers. The imagery evoked metaphors

that increased the complexity of the discourse and led to a deepening understanding of social and contextual aspects of the participants' lives (Newbury & Hoskins, 2010).

ART JOURNALING

Art journaling is another arts-based practice, used, for example, in work by Lisa La Jevic and Stephanie Springgay (2008), art educators practicing *a/r/tography*. Such research occurs through the creation of visual journals by research participants, where mixed text, images, magazine collage, and drawings become sites for artistic inquiry and active collaborative meaning-making. Student teachers focused on the artistic process of *a/r/tography* in their art journals to explore and critically reflect on their life experience: "visual journaling establishes an opening of inquiry for students to document their reflections, questions, and beliefs" (La Jevic & Springgay, 2008, p. 82). An ethics of embodiment was used in this study because issues of representation, authority, and voice required a "relational understanding of meaning making" (La Jevic & Springgay, 2008, p. 76) informed by *a/r/tography's* focus on the in-between spaces between art, teaching, and research. The *a/r/tography* research process, like ABR in general, does not identify set answers and instead remains open to further questions and complicating and troubling conclusions, while at the same time allowing for multiple co-created forms of knowing, otherwise unattainable.

MIXED MEDIA AND MULTIPLE ART FORMS

Researchers have also designed studies in which more than one art form is used. These mixed media studies (not to be confused with mixed methods studies, although the two are by no means incompatible) use a variety of different art media and forms to create a powerful impact. For example, in her dissertation on art education and art therapy in art classes at two alternative high schools in the Midwestern United States, Lisa Kay (2009) created small works of visual art as field notes (Figure 20.1). These artworks served to generate thematic analyses of school visits. In addition, Kay (2009) found poetry, a larger mixed media collage, and a written script of a play all useful to represent her research findings. In discussions of her work, Kay described the process of creating the visual field notes—small mixed-media collages created from paper, pastels, and found materials—as a way of capturing personal and artistic reflections of her observations (personal communication, Lisa

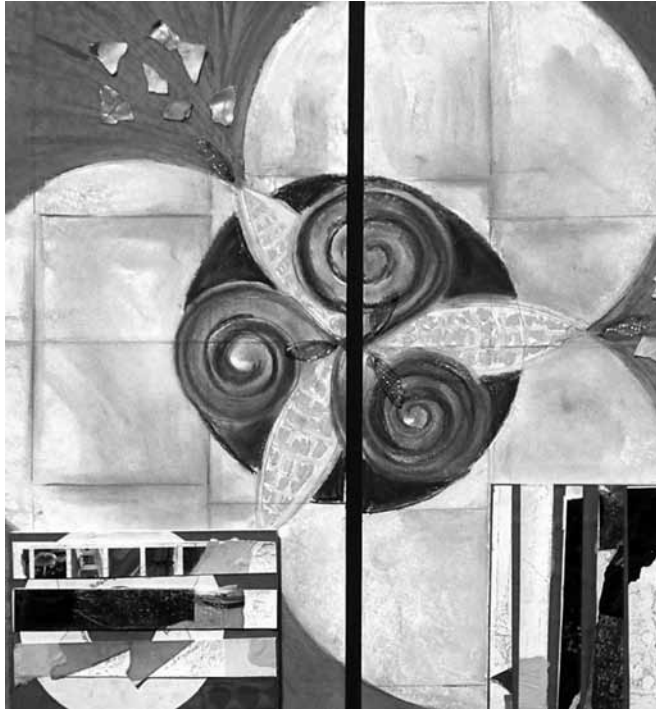


Figure 20.1 Detail of mixed media assemblage/collage which includes two visual field notes from Kay's 2008 dissertation, *Art Education Pedagogy and Practice with Adolescent Students at-risk in Alternative High Schools*.

Kay, November 16, 2010). This research artwork was shown in a gallery, while subsequent performances of the play at professional conferences provided another way to communicate different aspects of the research findings (Kay, 2013). In another example, an interdisciplinary research team led a project that resulted in a substantial display of poetry, narratives, drawings, and photography that examined the experience of hospital patients in Canada (Lapum, Ruttonsha, Church, Yau, & Matthews, 2011). Titled, *The 7,024th Patient Project*, the study resulted in a large installation of poetry and photography; by walking through a winding, labyrinth-like path, viewers learned emotionally and artistically of patients' journey through open heart surgery (see <http://www.youtube.com/watch?v=pYcSmsRW21g>). The researchers—whose expertise spanned the disciplines of nursing, interior design, cardiovascular surgery, and fashion design—conclude by asking,

How could the arts be used in other analytical approaches and other mediums for dissemination? How could we expand epistemological foundations through collaborations between artists, researchers, and practitioners? How could the arts be employed to further cultivate an ontological and epistemological space where imagination, aesthetics, and emotions

are integral to intellectual movements in research and health care? (Lapum et al., 2011, p. 112)

Works like these provide models for others interested in broadening the epistemological and disciplinary space and using multiple artistic forms to best disseminate research on complex social science topics.

Assessment and Evaluation: Development of Contested Criteria

Along with debates about what to call ABR came disagreements about how to evaluate it, if criteria should exist at all, and where to locate it within the academic landscape. For years, a heated discussion on whether novels could be accepted as doctoral dissertations was conducted, raising both practical and philosophical questions by Eisner, Howard Gardner, and others (Saks, 1996) even as some universities had already begun the practice (Sellitto, 1991). In Europe, a “similar debate about artistic research has been going on in parallel with—and very often without noticing—the North-American discussion about arts-based research” (Räsänen, 2008, p. 101). Standard academic forms of discourse, such as traditional style guidelines for dissertations and journal articles, required adaptation to accommodate experimental new forms (Finley, 2011). As

ABR became more common, researchers struggled to identify criteria to judge such work (Bochner & Ellis, 2003; Finley, 2003) while some questioned the very idea of criteria, validity, or uniform standards as inexorably bound to positivism (Bradbury & Reason, 2008). In a current scoping review of arts-based health research ($n = 71$ studies, 2000–2010), researchers felt there was a continuing need to identify quality criteria (Boydell et al., 2012). Although the topic continues to be “unresolved and even contentious” (Lafrenière & Cox, 2012), we will attempt to locate, organize, and describe areas of consensus and disagreement here.

Question/Method Fit

A key point that emerged early on was that, as in all research, the methodology should match the question (Saks, 1996). The appropriateness of the fit between research aims and methodology is a primary and uncontested criteria for all varieties of research (Creswell, 2007; Patton, 2002). Additionally, the standards by which the outcomes or results are evaluated must also be in accordance with the goals and methods (Leavy, 2009). At times, specious arguments were made due to confusion on this point, along the lines of, “Yes, but is it really research?” (Chenail, 2008). The aims of art-based researchers included powerful and emergent outcomes that could expand meaning, increase awareness and enlarge understanding, but these aims continue to be contested by some as scientific (St. Pierre & Roulston, 2006). In ABR, as in qualitative research in general, establishing legitimacy requires a philosophical belief in the value of such inquiry and an open and expanded view of the purpose and possibilities of research (Knowles & Cole, 2008; Patton, 2002).

The ability to “examine ourselves, investigate and express the worlds of others, transgress stifling conventions and boundaries, resist oppressions, grieve and heal, produce intersubjective knowledge, review the hidden meaning of memory work, and come to terms with multiple and contradictory identities” forms knowledge that is uniquely produced through ABR practices (Bochner & Ellis, 2003, p. 510). A systematic review of arts-based methods in the topic area of health research ($N = 30$) found that the arts were useful for uncovering or producing such knowledge, as well as for knowledge translation (dissemination), but the urgent call was to clarify the exact arts-based processes used in these studies and clearly explain the rationale and justification for their use (Fraser & al Sayah, 2011).

Likewise, systematic justifications of the need for arts-based methods to further specific goals continue to be of major concern for doctoral dissertation advisors and students today and “the need for clear rationale and clear explanation of the methodologies will likely continue” (Atkins, 2012, p. 63). Although the need for an appropriate fit between research goals and methods remains clear, what also seems apparent is that this link has not always been well articulated (Boydell et al., 2012; Fraser & al Sayah, 2011). As ABR emerges as a viable research practice, the continuing development of clear criteria by which this kind of research should be judged is needed (Boydell et al., 2012).

Aesthetic Power

A factor unique to ABR is its aesthetic power (Barone & Eisner, 2012; Faulkner, 2009; Leavy, 2009; Patton, 2002). Outcomes that involve the shaping of aesthetic form require skilled balance in “paying attention to the craft of the artistic practices used with usefulness in mind and creating evocative, provocative, illuminating and sensory representations of findings” (Leavy, 2011, p. 121). To be considered credible, ABR must carry, through aesthetic power, such interpretive vitality that provokes, stimulates, or connects with its audience, providing evocation (Barone & Eisner, 2012). This aesthetic power is constructed through the incisiveness, concision, and coherence of the final creative form (Barone & Eisner, 2012). The ability to skillfully use aesthetic elements in research to communicate emotional experience, inspire the imagination, and evoke empathy is important for such research outcomes, although the specific goals of course depend on the aims of the project. “The quality of the artistic elements of an arts-informed research project is defined by how well the artistic process and form serve research goals” (Cole & Knowles, 2008, p. 66). Again, the methods and goals must fit because aesthetic evocative power is only useful if the goal of the research is to enable research consumers to take an active role in the intersubjective construction of knowledge, potentially developing “deep insight” (Lafrenière & Cox, 2012, p. 2) by vicariously experiencing events from a different point of view.

Critical arts-based researcher Susan Finley (2003; 2008; 2011) has repeatedly raised concern about the use of such finely honed aesthetic criteria. Does this not necessarily require a level of artistic prowess, she asks, skills most often developed as a product of social privilege and years of education? If only certain

skilled individuals who can navigate both research and art domains are empowered to meet these criteria, the participatory and critical possibilities of ABR are halted, she argues. The requirement of aesthetic power limits the movement of “arts and social inquiry out of the elitist institutions of academe and art museums, to relocate inquiry within the realm of local, personal, everyday places and events” where oppression could be unveiled and social transformation begun (Finley, 2008, p. 72). “If we embrace rigor, if we engage in hegemonic control of the beautiful in research, then we run the risk of missing the opportunity” of the arts to inform and transform our world (Finley, 2003, p. 292).

However, if the production of the research does not “work” as a piece of art, that is, if it does not do the work of art—successfully create expressive form to communicate some new nondiscursive evocation of life experience (Langer, 1957)—then the research fails to perform the very function for which researchers had selected it. Artist-researchers such as Mary Beth Cancienne identified the risk of trivializing the art form of dance when hastily translating research text into a performance, doing neither the research nor the art justice (Bagley & Cancienne, 2001). Likewise, after reading poorly constructed poetry-as-research, researchers such as Faulkner (2009) and Piirto (2002) called for education and experience in literary endeavors before attempting to (re)present research through poetry. However, Lahman and colleagues encouraged inexpert poetry writing, suggesting, “poking around at research through lousy poetry may lead to *good-enough research poetry* as the researcher grows and develops” (Lahman et al., 2010, p. 47, emphasis in original; Lahman et al., 2011). Giving “attention to aesthetic quality” (Cole & Knowles, 2008, p. 66) or “paying attention to the craft” (Leavy, 2011, p. 121) implies that researchers should have or learn the art skills required to some extent. Arts-based researchers are often advised to circumvent this problem by working in teams with skilled artists (Lafrenière & Cox, 2012a; Leavy, 2011). Perhaps not all participants on the research team need to have the same level of artistic expertise. And this seems to be the solution Finley (2011) endorses, as she used as an example of critical arts-based inquiry young people conducting art-as-research dramatic projects with the guidance of their theater teacher.

Usefulness

A primary factor to consider in ABR is the usefulness of the resulting representation; in other words, the contribution of the work, which may include

educating broad audiences, contributing to a body of literature, or garnering attention for an issue of import. Most arts-based researchers would concur that usefulness is important (Leavy, 2011; 2013).

The issue for some is the extent to which usefulness and aesthetics are each important and how to balance the two. Although, as noted earlier, some researchers suggest that art works created in social research must meet aesthetic and artistic criteria developed in the arts (e.g., see Faulkner 2009) because ABR is conducted with research intentions beyond creating an artistic representation, usefulness is also a critical achievement. Because ABR is often conducted by researchers based in disciplines outside of the arts, sometimes usefulness compensates for aesthetic shortcomings. Although, ideally, research-driven artistic works are attentive to the craft they are adapting, they need not be “great” works of art per se in order to be useful (Leavy, 2009; 2011). When evaluating ABR, shy away from questions like: “Is it a good piece of art?” and rather ask: “What is this piece of art good for?” (Leavy, 2009; 2011). The relationship between aesthetics and usefulness is indeed complex because the artistic nature of the work may in fact contribute to its usefulness. For example, if someone enjoys reading a piece of fiction-based research because it is a well-written text, they may be more engaged, more reflective, and ultimately may learn more. In this regard, Maura McInyre (2001) suggests that we ask: “Can its artfulness increase its usefulness?” (p. 220, drawing on Finley & Knowles, 1995).

Participatory and Transformative

Currently, some of the most exciting work in the arts-in-research is in global community change and health promotion, featuring participatory and action-oriented methods (Kapitan et al., 2011; Kenny, Faries, Fiske, & Voyageur, 2004; McLean & Kelly, 2010; 2011). This work draws on the “oppositional, subversive, transformational, and otherwise resistive capacities of the arts” to address social inequalities, particularly in the area of the production and dissemination of knowledge (Leavy, 2011, p. 106). These artful and action-oriented processes enable silenced voices to be heard in new ways, becoming a “people’s pedagogy” with no less a goal than the “emancipation from colonizing human research that objectifies its participants (casting them as subjects)” through a “democratization” of research (Finley, 2011, p. 444). Arts research practices are useful for this endeavor. As Ardra Cole writes, to prompt change, “I needed words plump

and dripping with life juice, compelling and evocative images, representations that drew readers and viewers in to experience the research ‘text’... to more fully portray the complexities of the human condition to broader audiences” (Cole, 2004, p. 16). The social significance of the work became a central criterion for those who saw ABR as a site for a locally useful, transformative praxis (Finley, 2011; Grace & Wells, 2007; La Jevic & Springgay, 2008; Newbury & Hoskins, 2010).

ARTFUL AUTHENTICITY

In the wider tradition of qualitative research, constructivist criteria have long included the principle of authenticity, which can be defined as evident through a deep reflexivity about our individual and collective selves that results in fair depictions (Patton, 2002). Such reflective consideration raises awareness of multiple perspectives involved in research and emphasizes balanced points of view by those affected by/conducting research (Denzin & Lincoln, 2011*b*). In ABR in particular, in order for the audience to experience the research as feeling “true,” the research story must ring true through *referential adequacy*, a sense of validity that is, like a work of art, “fundamentally unmeasurable” (Barone & Eisner, 2012, p. 163). Legitimacy thus rests on the capacity of the art and reflexive dialogue to evoke this deep sense of authenticity (Finlay, 2002; Hervey, 2004). Sometimes termed *face validity* (Patton, 2002), authenticity is apparent through reflexive consciousness and the artful presentation of distinct and expressive voices. Authenticity is strengthened by the explicit reflexivity and transparency inherent in the expressive form of the art products. Art forms such as vivid imagery can enhance transparency, as “hearing or seeing or feeling the details of a lived experience, its textures and shapes, helps make the representation trustworthy or believable” (Weber, 2008, p. 45). “The best art is the most honest, authentic art” wrote art therapist and arts-based researcher Michael Franklin (2012, p. 89). Citing Imus (2001), Hervey (2004) found that the authentic *is* aesthetic and notes that this may be a critical link between artistic inquiry and authenticity.

CANONICAL GENERALIZATION

Traditional forms of research, at times, make claims to be generalizable to others beyond those who took place in the research. Arts-based researchers instead may claim canonical generativity, providing a sensory distillation of experience that

performs a heuristic function beyond the single case, with significant implications (Kapitan, 2010, after Eisner). Although the knowledge claims produced in this kind of research practice are intended to be ambiguous enough to allow for multiple, multidimensional, complex, dynamic, intersubjective, and contextual interpretations, this is also their strength. Additionally, technological advances are now leading to innovations in the aggregation or culmination of qualitative knowledge, potentially permitting comparison of findings to inform policy (Lincoln & Denzin, 2011).

Conclusion and Future Directions

Arts-based research is an expanding, emergent, and exciting field. Given the breadth of this transdisciplinary field and the extent of ongoing negotiations over best practices and assessment, it is impossible to cover the terrain in one chapter. Interested readers should seek some of the sources noted in this chapter. We would like to conclude with some discussion of the future of the field, focusing on those areas where we see some of the greatest potential for growth.

As noted, ABR developed amid the push toward public scholarship, and we believe it offers enormous potential for aiding that movement. With increases in representational forms that have the potential to reach audiences outside of the academy, fueled by technological developments in electronic publishing, we see no limitations on how far this research can spread. One area in which we see the potential for ABR to lead to social change is in the area of public policy.

There are many possibilities for ABR to be used to affect public policy, for example, by involving the public in the policy development process. One of the persistently difficult challenges in policy research is developing effective strategies for engaging the public in policy development (Nisker, 2008). Arts-based researcher Jeffrey Nisker writes: “Theater can be such an instrument, as it is able to engage, cognitively and emotionally, large numbers of citizens of diverse perspectives, provide them relevant information... and provide a forum where citizens are able to air and debate their opinions for policy research purposes” (2008, p. 614). Performances carry great potential to involve relevant stakeholders and the public more generally in the policy development process.

Health policy researchers are actively drawing on the potential of the dramatic arts to engage and inform people in order to involve different

segments of the public in the development of health policy (Nisker, 2008). Nisker writes: “Theatrical productions, focusing on the persons at the center of a health care issue, can bring all who ought to be responsible for its policy development (e.g., patients, their family members, the general public, health professionals) to a better understanding of the new scientific possibilities, ethical issues, and most important, the persons immersed therein” (2008, p. 615). For example, beyond healthcare conditions per se, there are many contemporary ethical issues and questions emerging at the intersection of science and technology (McTeer, 2005), such as in vitro embryo testing, stem cell research, cloning, and many other examples. Researchers can use ABR to bring the public into these discussions.

There are considerable ethical issues of which to be mindful. Health theater is a form of public performance; therefore, the researchers bear responsibility for the impact the performance has with regard to audience well-being after the performance. The need to create ethical guidelines has arisen out of incidents in which audience members were put at risk as a result of witnessing an ethnodramatic performance (Mieczakowski, Smith, & Morgan, 2002).

Nisker suggests that various stakeholders should be given drafts of the script for feedback and “reality checks” and to uncover differing perspectives (2008, p. 619). This feedback piece, in some form, is necessary in transdisciplinary projects in order to actively engage with and negotiate multiple viewpoints. Mieczakowski and colleagues suggest having a preview performance with an audience of people who possess knowledge about the topic under investigation. They also note that “post-performance forum sessions” can be used to analyze audience responses to the performance, in order to assess the show’s impact (2002, p. 49).

Additionally, we envision the use of new technology to enable this type of participant feedback, for example, through smartphones, interactive member checking through online interaction, web-enabled collaborative creative processes, and other innovative research processes. New online developments such as crowd sourcing and open source production can engage the power of participatory democracy to maximize and diversify stakeholder input and harness collective intellect (Brabham, 2009). We see possibilities for new creative communities of practice (Wenger, 2011) as arts-based researchers use the global communication possibilities of the Internet to collaborate (see Scott-Hoy & Ellis, 2008) and

mobilize research projects that blend the practices of art and science. We expect rapid changes and innovation in this area to continue.

Notes

1. Gioia Chilton, MA, ATR-BC, is a PhD Candidate in Creative Arts Therapies at Drexel University, in Philadelphia, Pennsylvania. As a recipient of the prestigious Drexel University Provost Fellowship, she is currently conducting innovative arts-based research into the artistic expression of positive emotions. Gioia is adjunct faculty at the George Washington University’s Graduate Art Therapy Program and at Marymount University. A registered and board certified art therapist with twenty years of experience, Gioia has conducted art therapy with those experiencing chronic mental illness, adolescents in foster care, and with children experiencing emotional disorders, throughout the culturally diverse Washington, DC, area. In 2010, she co-founded Creative Wellbeing Workshops, LLC, a NBCC-approved continuing education provider of interactive art-based trainings on authentic happiness (www.creativewellbeingworkshops.com). She has authored numerous articles on art therapy practice and education, art therapy and positive psychology, and arts-based research, and has presented on these topics nationally. Gioia’s artistic practice involves mixed media on paper and collaborative projects such as altered books.

Patricia Leavy, PhD is an independent researcher and novelist, formerly associate professor of sociology and the founding director of gender studies (2004–2008) at Stonehill College in Massachusetts. Leavy has emerged as a leader in the qualitative and arts-based research communities. She is the author of the acclaimed and best-selling novels, *Low-Fat Love* (2011) and *American Circumstance* (2013). She is the author of numerous nonfiction works including *Fiction as Research Practice* (2013), *Transdisciplinary Research Practice: Issue and Problem-Centered Approaches to Research* (2011), *Oral History: Understanding Qualitative Research* (2011), *Method Meets Art: Arts-Based Research* (2009), and *Iconic Events: Media, Politics and Power in Retelling History* (2007). She is coauthor of *The Practice of Qualitative Research* (2005, 2011) and *Feminist Research Practice: A Primer* (2007). She is the co-editor of *Hybrid Identities: Theoretical and Empirical Examinations* (2008), *Handbook of Emergent Methods* (2008), *Emergent Methods in Social Research* (2006), and *Approaches to Qualitative Research* (2004). She is editor for four book series: *Social Fictions*, *Teaching Gender* and *Teaching Race & Ethnicity* with Sense Publishers and *Understanding Qualitative Research* with Oxford University Press. She is regularly called on by the national media for her expertise on popular culture, gender, and other sociological topics. The New England Sociological Association named Leavy the 2010 “Sociologist of the Year” and she has recently been nominated for a Lifetime Achievement Award by the International Congress of Qualitative Inquiry and a Special Achievement Award by the American Creativity Association. She offers book talks as well as keynotes and workshops on a variety of topics. Please visit www.patricialeavy.com for more information.

2. It is important to note that we do not intend to privilege some arts-based research practices over others. We have chosen to focus on those practices with which we have the most experience and that we believe are among the most widely used.

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Qualitative Approaches in Internet-Mediated Research: Opportunities, Issues, Possibilities

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Abstract

Internet-mediated research (IMR) has grown expansively over the past decade in scope, range of methodological possibilities, and breadth of penetration across disciplines and research domains. However, the use of IMR approaches to support qualitative research has lagged behind its application in supporting quantitative methods. This chapter discusses the possibilities of using IMR methods in qualitative research and considers the issues and debates that have led some qualitative researchers to be reluctant to consider IMR as a viable alternative to traditional offline methods. The chapter adopts an optimistic stance on the potential for qualitative IMR and outlines a range of possible methods and strategies, as well as examples of successful (and less successful) studies. Practical advice on tools, procedures, and guidelines for good design practice is offered. A comment on likely future scope, methods, emerging techniques, and developments in qualitative IMR is presented.

Key Words: Internet research methods, online methods, qualitative methods, internet-mediated research

A Brief History of Qualitative Internet-Mediated Research

In the early 1990s, pioneers started to conceptualize, design, and pilot internet-mediated research (IMR) methods, and the first published reports of IMR studies started to appear. These pioneers engaged with the internet as it existed then, considering how early technologies such as e-mail, usenet discussion groups, and the World Wide Web¹ could potentially support data-gathering procedures in social and behavioral research. The main qualitative IMR approaches that seemed viable at this time included interviews, focus groups, and observational studies that used linguistic data, and procedures for implementing these methods were devised and piloted (e.g. interviews: Chen & Hinton, 1999; Murray & Sixsmith, 1998; focus groups: Gaiser, 1997; Tse, 1999; Ward, 1999; linguistic observation: Bordia, 1996; Ward, 1999; Workman, 1992). These early examples highlighted

and sparked discussion on a number of issues and considerations that have since been reviewed and explicated in authoritative texts on the subject, probably the most comprehensive early account being that of Mann and Stewart (2000). Although still providing very useful insights into enduring issues and debates, these early texts are inevitably dated in such a rapidly developing field (for more recent discussions, see Evans, Elford, & Wiggins, 2008; Hewson, 2007; 2008; Ignacio, 2012; Meho, 2006; O'Connor, Madge, Shaw, & Wellens, 2008; Rodham & Gavin, 2006; Seale, 2010). A significant development over the past decade or so that has implications for the scope and practice of qualitative IMR has been the emergence of *Web 2.0* (O'Reilly, 2005). Essentially, Web 2.0 refers to the evolution of the World Wide Web from a relatively stable, static online space for publication and dissemination of information to a more fluid, organic, interactive, collaborative space where users can

collectively act, interact, and construct and share information and knowledge. Key technologies and services that serve to create Web 2.0 include social networking spaces (e.g. Facebook, MySpace), wikis (e.g. Wikipedia), multimedia sharing spaces (e.g. SoundCloud FlickrR, YouTube), blogs (interactive personal online diaries), and a range of other collaborative information-sharing and dissemination sites, such as OpenCycleMap (<http://www.opencyclemap.org/>) and Google Docs (docs.google.com). Furthermore, the emergence of mobile devices that make these structures and services accessible “on the move” has created a sense of Web 2.0 being potentially “ever-present.” The emergence of Web 2.0 has been argued to constitute a qualitative shift in the structure and form of the internet and World Wide Web, rather than just an expansion in terms of growth of existing structures and services. The implications of this transformation for the scope and nature of social and behavioral IMR are non-trivial. Thus, the Web as it exists today readily creates a mass of easily locatable, often content-searchable traces of online collaborative activity and social interaction, traces that create a potentially rich source of data for use in qualitative social science research. The range and diversity of these traces of human activity, along with their ready searchability, creates intriguing possibilities for social science data-gathering methods particularly observational approaches. Emerging Web 2.0 technologies have created conceivably novel and innovative ways to study human social and communicative activity, some of which have recently been put into practice in qualitative IMR. It is the goal of this chapter to review some of the key approaches, tools, technologies, and procedures that are possible in qualitative IMR, ranging from early, enduring technologies and techniques (such as e-mail interviews), to newer approaches facilitated by the changing face of the internet and World Wide Web (Web 2.0). The key methods of qualitative research considered are interviews, focus groups, observational research, and document analysis. Throughout, emphasis is placed on identifying procedures and strategies that can help maximize the quality of research data gathered and the integrity of conclusions reached while adhering to key ethics principles.

Qualitative Internet-Mediated Research: State of the Art

In this section, each of the key methods available in qualitative IMR is considered in terms of supporting tools and technologies, design choices

and good practice, sampling and recruitment strategies, and ethical issues. Advantages and disadvantages of each approach are discussed, both as compared with other IMR approaches and with traditional offline approaches. Because it is essentially at the data-gathering stage that IMR methods have the biggest impact, as opposed to other stages and processes in the research methodology (e.g., conceptualization, interpretation, etc; see Hewson, 2008), this is the stage of primary focus here.² Also, whereas IMR approaches have already been used by a vast and diverse body of researchers coming from a wide range of disciplinary backgrounds and research domains (Fielding, Lee, & Blank, 2008), emphasis here is on techniques most relevant to qualitative social science research. Thus, examples and illustrations are drawn primarily from disciplines such as sociology, psychology, anthropology, and the like. Other examples are used where they serve as illustrations of particular methods, issues, or arguments relevant to social and behavioral research. The general advantages and disadvantages of IMR methods compared with traditional offline approaches have been well-stated by now (e.g., see Hewson, Yule, Laurent, & Vogel, 2003; Mann & Stewart, 2000) and will not be recapitulated at length here. Advantages include cost and time efficiency, expanded geographical reach, and access to hard-to-reach populations; disadvantages include possible sample bias, reduced levels of researcher control, and potential unreliabilities due to technical issues (e.g., Hewson et al., 2003). Consideration of the advantages and disadvantages of the particular methods and techniques outlined here will be woven into the discussion, as will consideration of ethical issues that emerge in particular contexts. For useful general discussions of ethical issues in IMR see BPS (2013), Ess (2007), Kraut et al. (2004), and Rodham and Gavin (2006).

Interviews and Focus Groups

TOOLS, TECHNOLOGIES, PROCEDURES

The main internet technologies able to support online interviews are e-mail, discussion forums, and online instant chat. E-mail is one of the earliest internet technologies (in fact, predating the emergence of the internet, according to Zakon, 2000), and it has been used to support online asynchronous interviews (e.g., Baker & Fortune, 2008; Bowker & Tuffin, 2004; McDermott & Roen, 2012; Murray & Sixsmith, 1998). Most internet users are familiar with and use e-mail, often on a daily basis. Popular e-mail clients

include Apple's Mail (for Mac) and Microsoft's Outlook (for Windows). Web-based e-mail is also widely used, with many freely available clients on offer, such as Hotmail (hotmail.com) and Google's Gmail (mail.google.com). A typical IMR interview scenario using e-mail involves the researcher sending a series of questions to the interviewee(s) in the body of an e-mail message, to which the interviewee simply responds by e-mail. Follow-up questions and responses can then be sent, as required (e.g., see Bowker & Tuffin, 2004). E-mail technologies can also be used to support focus group discussions because e-mails can also be sent to multiple users simultaneously, either by typing in several e-mail addresses to create a "group" of recipients and then requesting that everyone uses "reply all" to respond or by using mailing list software, whereby every time a group member replies, his or her response is circulated to a predefined group (a mailing list) of recipients. Several researchers have reported using this latter approach (e.g., Adler & Zarchin, 2002; Gaiser, 1997). LISTSERV software (the first mailing list service provider) supports the creation and management of mailing lists (see <http://www.lsoft.com/products/listserv.asp>), but it requires a paid set-up and yearly subscription fees to use and is thus one of the more expensive options. A searchable web interface database is available that allows a search of all existing LISTSERV mailing lists, returning information about the list, owner, and subscription instructions (see <http://www.lsoft.com/catalist.html>). For example, I searched using the term "cycling" and found a list named "women's cycling," to which I was able to send a subscription request; however, the request was denied because I was outside the service area for "womencycling" (the list being located in the United States, the author in the United Kingdom). As demonstrated by this example, lists may be set up with private or restricted access, and usually this information can be found prior to sending a subscription request. PhpList is another mailing list management solution (see <http://www.phplist.com/>) that offers either free open-source code for download or a hosted solution for a very reasonable fee (free for up to 300 messages per month, but requiring a small monthly fee for larger message quotas, priced on a sliding scale). Institutional IT support services (e.g., university IT departments) may also assist in setting up mailing lists for research purposes, particularly when larger scale, funded projects are involved. In general, mailing

list software offers a more reliable, robust solution than standard e-mail, since research participants may forget to hit the "reply all" button when using simple e-mail. Joining an existing list and asking members to engage in a focus group discussion for research purposes within that list would, in most conceivable contexts, be inappropriate and in violation of "netiquette" (online etiquette). Creating a new list for the purposes of the research, however, is plausible (as did Adler & Zarchin, 2002, using LISTSERV) and may often be appropriate. The ubiquity of e-mail, particularly the ease and familiarity with which most internet users engage with it as a communication medium, makes it a highly accessible and attractive option for both researcher and participant. No additional training or skills development is likely to be needed for participants to be able to take part in an e-mail-supported interview or focus group.

Discussion forums (or "newsgroups") provide an alternative solution for conducting online focus group interviews. These (e.g., Usenet groups; see <http://www.usenet.net>) are closely related to mailing lists in that they support online group discussions, but differ in the details of the underlying technology. Essentially, whereas mailing lists send all discussion items to each group member's own individual e-mail address, discussion groups locate all posts on a central server that group members access using a newsreader (e.g., see <http://www.newsreaders.info/recommended-newsreaders.htm> for a comprehensive list of options) in order to view and respond. In that sense, newsgroups are somewhat less invasive than mailing lists. Usenet groups can be accessed and managed at the web-based interface <http://www.groups.google.com>, which allows anyone to set up a group for free. Google groups, in fact, allows the user to set up either a mailing list or a web forum discussion group. Another option for managing online focus group discussions *virtual learning environments* (VLEs), such as Blackboard (<http://www.blackboard.com>) or Moodle (<http://moodle.org>). Many educational institutions will already use a VLE solution to support teaching and learning, and, typically, these incorporate group discussion functions to support communication between users. These solutions can also (depending on the particular software package) support one-to-one discussions and thus potentially individual interviews. Kenny (2005) has reported successfully using WebCT (now owned by Blackboard) to conduct online focus group interviews.

A variety of recently emerging technologies that function essentially as discussion forums and that could thus be used to support online focus groups are available, including www forums (a searchable database of existing www forums is available at <http://www.yuku.com>). As well as allowing users to search existing discussion forums, this resource also allows users to create their own multimedia, interactive “message boards,” which are in effect small-scale social networking sites. These could thus serve as flexible, cheap, and easy to set up spaces for conducting online focus groups. Facebook—a highly popular social networking website that, at the time of writing, claims to have more than 800 million users worldwide (<http://www.internetworldstats.com/facebook.htm>; accessed November 20, 2012)—also offers the option for registered users to set up “groups” that act essentially as smaller social networks in which select group members are given access by the group’s creator.

Clearly, the range and functionalities of software options readily available for supporting focus group studies online has expanded quite dramatically over the past decade or so. From early LISTSERV and usenet technologies (both of which continue to be well-used), a range of www forums and social networking sites have emerged. As additional examples of studies using these various emerging tools and technologies appear, the scope, opportunities, and relative merits that different solutions offer will become clearer. Possibilities for incorporating multimedia into discussions (e.g., posting pictures, audio, or video) emerge with the more recent technologies such as www forums, thus potentially expanding possibilities in ways that have not been readily supported using earlier technologies such as mailing lists or, indeed, when using traditional offline methods.

The technologies discussed so far—e-mail, mailing lists, and discussion groups and forums—are generally more suitable for conducting asynchronous interviews and focus groups (although in some cases they may also incorporate technology for supporting conversations in real-time; e.g., as in social networking sites such as Facebook, which now incorporates chat functions). Synchronous approaches, which arguably may be seen as more closely approximating offline face-to-face (FTF) methods and thus be more attractive to some researchers, are more readily supported by online chat software. Chat software essentially

supports the real-time direct exchange of text-based messages. Numerous free software options are available; for example, see mIRC (<http://www.mirc.com/>), Google Talk (<http://www.google.com/talk/>), and Messenger (now available for both Windows and Mac operating systems, see <http://www.microsoft.com/mac/messenger>). Increasingly, chat software incorporates options for supporting audio and video (e.g., Google Talk; Skype, <http://www.skype.com>) and/or mobile applications (e.g., ICQ, [icq.com](http://www.icq.com); Whatsapp, <http://www.whatsapp.com/>). The possibilities created by these real-time chat tools for synchronous interviewing approaches in IMR are thus also expansive and expanding, offering researchers a large selection of often free or low-cost options (assuming, of course, that potential participants have or can be given access to the necessary hardware). For focus group research, online conferencing options allow several group members to interact simultaneously; many academic institutions will have a conferencing solution already installed (e.g., Blackboard Collaborate, <http://www.blackboard.com/platforms/collaborate/overview.aspx> [previously Elluminate]). Many conferencing tools are available, including free solutions that generally are more limited in terms of how many users they can support and that are generally more demanding to set up (e.g., Php Free Chat, <http://www.phpfreechat.net/>) and those that charge a monthly fee and are generally easier to set up and more flexible (e.g., Adobe Connect, <http://www.adobe.com/uk/products/adobeconnect.html>). Researchers have used chat and conferencing software to support both synchronous interviews (e.g., Chen & Hinton, 1999; Davis, Bolding, Hart, Sherr, & Elford, 2004) and focus groups (e.g., Madge & O’Connor, 2002; Schneider, Kerwin, Frechtling, & Vivari, 2002) in IMR.

This discussion highlights the range of resources and options, many free and easily accessible, that have become available to support IMR interview and focus group methods, either using synchronous approaches that arguably more closely mimic offline traditional FTF methods or asynchronous approaches that present a novel interview setting in which participants are given greater flexibility and control over when to respond to interview questions. In the next section, issues involved in choosing between different tools, technologies, and design options, including whether to use synchronous or asynchronous approaches and when offline approaches may be preferable, are considered.

DESIGN ISSUES AND STRATEGIES

A key distinguishing feature of online interview approaches, as compared to traditional FTF approaches is, of course, the lack of physical proximity involved. Even with real-time approaches using audio and video (e.g., Skype), researcher and participant are not physically present to each other, and this may have an impact on the nature of the interaction compared with FTF approaches. Most IMR researchers to date have used solely text-based methods, thus removing the IMR interview context even further from a FTF interaction. The implications of this for qualitative interview research online have been widely discussed, tested in practice, and reflected on (e.g., Barratt, 2012; Madge & O'Connor, 2002). One central theme in these discussions concerns the extent to which the so-called impoverished communication medium of internet-based communications—that is, one essentially restricted to text-based exchanges—may impact on the nature and quality of data that can be obtained (this was especially of concern in the early days, when slower bandwidths, lower computing power, and less sophisticated software made audio and video applications highly implausible). Hence, the impact of the lack of normally available FTF cues—facial expressions, tone of voice, body language, and the like—has been a central theme, leading qualitative researchers to question whether IMR methods will ever offer a valid alternative to offline FTF methods. A number of sub-issues emerge around this theme, including the possible impact on levels of depth and reflexivity, accuracy and reliability, and establishing rapport (e.g., Davis et al., 2004). Additionally, the enhanced sense of anonymity that this feature may instill in an IMR interview (particularly text-based) context is of interest, with its potential implications for levels of social desirability and disclosure (e.g., Hewson, 2007). Here, each of these issues is discussed in turn to determine whether different online methods may or may not realistically provide potential alternatives to FTF interviews.

Depth and Reflexivity

The question of whether it is possible, in an IMR interview context, to achieve levels of depth and reflexivity equal or close to those achievable offline is debated, and researchers have reported different experiences. Reports of the successful generation of rich, reflective qualitative interview data online include those from Bowker and Tuffin (2004), Kenny (2005), McDermott and Roen (2012),

Murray and Sixsmith (1998), and O'Connor and Madge (2003). Less successful reports also exist (e.g., Bowker & Tuffin, 2004; Davis et al., 2004). One possible contributing factor here is whether synchronous or asynchronous approaches are used. As some authors have pointed out (e.g., Davis et al., 2004; Gaiser, 1997), online “chat” has developed a reputation for being rather playful and flippant; thus, it may encourage less reflective, sincere, or detailed responses. Indeed, most of the reported successes used asynchronous approaches, whereas less successful reports often have used synchronous methods. Having said this, O'Connor and Madge (2003) report being able to gather rich, reflective data using online synchronous focus groups. Barratt (2012) also reports successfully gathering data from young drug users through online chat interviews. Perhaps of relevance and noteworthy here is that, in both these studies, the researchers used carefully thought out rapport-building exercises. Also, Barratt (2012) notes that her interviewees were already experienced internet chat users; however, she also reports that the rapport-building strategies employed—which led to the generation of detailed, in-depth data—were not successful in all cases. Bowker and Tuffin (2004) directly compared asynchronous and synchronous approaches and confirmed that, for them, the latter approach was less able to generate rich, elaborate data. It certainly seems plausible that the extended timescale of asynchronous IMR interview approaches, offering potentially more time for thoughtful, reflective responses (compared with both synchronous and FTF approaches), might facilitate the elicitation of richer, more elaborate qualitative data. Furthermore, both researchers and participants are (typically, with most technological solutions, e.g., e-mail) able to look back over and reflect on the conversation in an asynchronous interview in a way that is much less easily done when using synchronous approaches and very difficult to do in real-time FTF interactions. This may serve to further enhance reflexivity and the depth and accuracy of interviewees' responses, as well as of researchers' questions (Hewson, 2007). In this sense, IMR approaches may even have a possible advantage over traditional FTF methods in being able to elicit rich, reflective, reflexive data. Arguably, this might compensate for the reduced information available from extralinguistic cues typical in an IMR interview. Synchronous approaches, conversely, do not allow time for reflection in the same way, yet still are subject to reduced cues from extralinguistic information compared

with FTF approaches. Taking into account these considerations, it is not surprising that researchers using asynchronous approaches have generally been more likely to report having obtained rich, detailed, reflective qualitative data; however, those examples that have generated high-quality, rich data using synchronous approaches show that this method can also be effective. Further research will help to uncover the factors (e.g., rapport-building techniques, interviewees' experience with online communication technologies, etc.) that might be related to successful outcomes in each case.

Asynchronous online interview approaches have also been noted as having their own particular potential disadvantages. Conversational "flow"³ might be reduced due to the lengthier timescale and lack of immediacy between questions, responses, follow-ups, and further responses (Gaiser, 2008; Hewson, 2007), and this may impact upon the coherence and reflexivity of a conversation (Gaiser, 2008). There is also less scope for the inclusion of substitutes for extralinguistic cues (body language, facial expressions, etc.) in online asynchronous approaches—or at least convention renders these less likely to be used in asynchronous than synchronous online contexts. Linguistic devices and cues often used in online chat, such as smilies (:-) and acronyms (ROTFL [rolling on the floor laughing]), may offer extra richness of information in synchronous approaches. Still, a smilie typed in a chat box is arguably rather impoverished when compared with naturalistic facial expressions used in FTF interaction.

One potentially appealing approach that may help overcome the absence of extralinguistic cues is the use of audio and video. As internet technologies develop, these possibilities become more viable (e.g., using Skype and similar technologies). Although direct physical proximity is still absent (e.g., it is not possible to smell the perfume the interviewer is wearing), this might constitute the closest possible distal approximation of actual FTF interaction. Presently, however, technological challenges still make this option less than fully attractive. Issues related to bandwidth, internet traffic, and availability and reliability of required software and hardware are problematic (e.g., Hewson, 2007). A handful of researchers have implemented multimedia approaches. For example, Hanna (2012) reports using Skype to conduct online interviews. Although she did experience some "technical glitches" (thus confirming that such problems are still an issue), she also argues that the approach is potentially useful

when a closer approximation of FTF interviews is desired while having the added advantage of offering a greater level of control to participants. This greater level of control could potentially encourage a more equal relationship between "researcher" and "researched" and reduce researcher intrusiveness into the interviewee's personal space (Hanna, 2012). Still, until the technologies supporting multimedia communication online improve in reliability and quality, many researchers may prefer to continue to use text-based methods. Choppy video and low-quality audio could seriously interfere with the flow and quality of an online interview.

Data Integrity and Quality

The issue of data integrity is somewhat related to considerations of depth and reflexivity and is underpinned by some of the same technological issues and constraints of the online communication medium. The lack of extralinguistic cues discussed earlier might potentially lead to ambiguities and misunderstandings in online text-based communications. Or, researchers may find it difficult in such contexts to properly get a sense of whether participants are fully engaged with the research process and offering sincere and genuine responses. Deception in an online communication medium certainly has more scope than in FTF contexts (e.g., fabricating information about one's demographic and biosocial attributes). This is linked to issues relating to the reduced levels in IMR of researcher control over—and knowledge of—participants and participants' behaviors. In an online interview context, the authenticity, accuracy, and reliability of the information obtained might be at risk should participants deliberately choose to deceive the researcher or should unintended misunderstandings or misinterpretations arise. Of course, such threats are not unique to the online communication environment, but arguably, in this context, they are compounded compared with traditional FTF approaches. The extralinguistic online devices mentioned earlier might help here (e.g., a smiley wink can help indicate that a comment was meant in jest or signify flirtation—assuming, of course, competency in and knowledge of the use of these devices by each conversational partner). In situations in which it is imperative that information about participants' personal (e.g., biosocial) characteristics is accurate, and this is not known in advance or cannot be verified offline, then audio and/or video options may be most suitable. However, overall, there is very little evidence that IMR participants deliberately set out to deceive researchers. Strategies for enhancing

confidence that participants are who they say they are (e.g., new mothers, as in the study by Madge & O'Connor, 2002) include rapport-building exercises because good rapport may encourage engagement, commitment, and honesty. Verification that participants are able to give full informed consent (e.g., are not under the required age) can become particularly important in some research contexts; see the further discussion of this ethics-related point in the section Sampling, Recruitment, Ethics.

When they occur, technological failings can also impact upon the integrity and quality of the data obtained in IMR. A good principle here is to strive to utilize robust, well-tested techniques, software solutions, and procedures. Piloting any technologies, tools, and procedures in advance of conducting an interview online is imperative. This may be done within the research team or by recruiting interviewees willing to try out the software technologies and procedures to be used. Technologies that are more likely to suffer reliability and quality issues, such as online multimedia chat (i.e., using audio and video) for example, are often better avoided, with a good rule of thumb being to use the lowest tech solution that will do the job (Hewson, 2003). Also, using technologies that are not only robust and reliable but also user-friendly and ideally familiar to both researcher(s) and participant(s) is advisable, other factors being equal. The accessibility of the tools and methods used—both to the researcher (to implement) and to potential participants (to participate)—also becomes relevant to sampling and recruitment issues.

On the whole, qualitative IMR interview researchers have reported positive experiences, with research participants found to be engaging with and taking the research seriously, as indicated in the examples cited earlier. There have been a few exceptions, however, where either engagement and participation was poor (Strickland et al., 2003) or ambiguities and misunderstandings arose (Davis et al., 2004). Regarding the choice between synchronous and asynchronous approaches, although the latter have been seen as potentially able to generate deeper, more reflective data, it has been suggested that the spontaneity of synchronous approaches may possibly lead to more honest (e.g., O'Connor et al., 2008) and less socially desirable (e.g., Gaiser, 2008) answers. Presumably, the idea here is that, with less time for reflection, thought, and “constructing” a suitably presented answer, candor may be enhanced. Equally, however, it seems that one could argue that a spontaneous gut response might be more likely to conform to

standard accepted normative views, whereas a more considered response might generate a more reliable, accurate, and honest answer. It would seem that future research is still needed to shed further light on such questions.⁴ Asynchronous approaches may have an advantage when it comes to obtaining reliable, accurate data because participants will have time to check “facts” against documentary evidence.

Levels of Rapport

Related to issues of both depth and reflexivity, and integrity and quality of data, is the issue of levels of rapport, particularly how best to establish good levels of rapport in an online communication context. Good rapport with participants may lead to richer, more honest, and higher quality qualitative data (e.g., Barratt, 2012).

Considering potential barriers to establishing rapport in IMR interviews, compared with traditional FTF approaches, the lack of physical proximity and reduced information from extra-linguistic cues which typify IMR contexts re-emerges as relevant (e.g. Jowett, Peel & Shaw, 2011). Adopting clear strategies for establishing rapport seems to have worked well for several researchers, and this seems a good principle to follow. For example, Madge and O'Connor (2002) report success in establishing good rapport with their synchronous focus group interviewees by initiating researcher self-disclosure prior to the interviews. Bowker and Tuffin (2004) adopted a similar approach, which was also largely successful, although they note that, in their experience, establishing “relational development” took longer than with FTF approaches. Gaiser (1997) reports a positive experience in establishing rapport in online focus groups through the use of introductory exercises. Other authors have also pointed out issues relating to establishing rapport online and emphasized the importance of adopting clear strategies and procedures to assist in achieving good rapport with participants (e.g., Barratt, 2012; Jowett et al., 2011). Authors who have reported poor levels of rapport have not always used such techniques. Strickland et al. (2003) report less positive experiences in establishing rapport in asynchronous focus group interviews, noting that it was difficult to get the discussion going; however, Strickland et al. did not employ any particular rapport-building strategies, unlike the aforementioned authors who reported more positive experiences. Such strategies are thus advisable and likely to be valuable. Jowett, Peel, and Shaw (2011) also note that, due to the reduced ease of establishing rapport with

participants prior to an online interview commencing, ongoing development of rapport throughout the research process becomes crucial. As mentioned earlier, establishing good rapport and “getting to know” participants may help maximize levels of confidence in the authenticity of the accounts they offer during the course of an interview.

Anonymity, Disclosure, Social Desirability

Establishing good rapport with participants has been found to work well for researchers who have devised carefully considered strategies for this, and rapport building is thus advised as good practice. However, this principle may sit somewhat uncomfortably alongside evidence that the heightened levels of anonymity typical in online interview contexts (compared to FTF approaches) can confer benefits. The IMR interview setting is interesting in the way that it supports fairly fluid and fluent levels of interaction—in the form of verbal exchanges either asynchronously or in real time—while at the same time offering a high degree of privacy due to the lack of visual cues and physical presence (in non-multimedia applications) (e.g., Hewson et al., 2003). This holds intrigue in terms of its implications and possible effects compared with traditional FTF interview contexts. There is some evidence that computer-mediated communication (CMC) contexts can lead to higher levels of self-disclosure than FTF contexts (e.g., Bargh, McKenna, & Fitzsimons, 2002; Joinson, 2001), as well as to reduced levels of socially desirable responding (e.g., Joinson, 1999). Such effects may conceivably transfer to the online interview setting. Evidence that this may be the case comes from reports of both online asynchronous (e.g., Murray & Sixsmith, 1998) and synchronous (e.g., Madge & O’Connor, 2002) interview contexts. There is also evidence that enhanced candor and self-disclosure effects may be constrained to visually anonymous CMC contexts (Joinson, 2001), thus suggesting possible benefits to using solely text-based interview contexts as opposed to multimedia applications using video. This may be a useful point to bear in mind when dealing with research contexts involving particularly sensitive or personal topics in which participants may naturally be less predisposed to disclose and offer highly personal information (but note the possible tension here with the enhanced importance of verifying personal characteristics, such as age, in highly sensitive research contexts). Whether adopting clear, focused rapport-building strategies may serve to reduce perceived levels of anonymity and thus

possibly reduce levels of candor and disclosure by participants requires further research, but the experiences of researchers such as O’Connor and Madge (2003), who established high levels of rapport and collected rich, detailed data, would appear to indicate that this is not an issue.

Overall, there is some compelling evidence that the online medium may lead to higher levels of self-disclosure of sensitive and personal information (see also Joinson & Paine, 2007), although the extent to which this is particularly linked to perceived higher levels of anonymity requires some further investigation.

Other advantages stemming from the arguably more “detached” or anonymous setting of the online interview include the possible balancing out of power relationships (both between researcher and participants and between participants; e.g., in a focus group), due in part perhaps to greater levels of anonymity and also to greater levels of participant control over when to respond (e.g., Madge & O’Connor, 2002). This may arguably lead to a “democratization” in online communication, due to participants often not being readily aware of each other’s sex, skin color, regional accent, and the like; it has been suggested that this may potentially balance out power relationships that can exist offline, where all these cues and sources of information are readily available (Illingworth, 2001). However, questions have been raised about the extent to which this idea is realistic (Mann & Stewart, 2000). For example, linguistic style—also evident in purely text-based communications—has been highlighted as potentially able to offer rich clues about a person’s attributes, identity, and characteristics, including gender (e.g., Herring, 1996), socioeconomic status, nationality, and so on. Still, in the context of the online interview, it is certainly quite plausible that heightened levels of privacy, anonymity, and flexibility in responding in one’s own timeframe and at a location of one’s choosing may well empower participants beyond attending a FTF on-site setting determined by the researcher (e.g., Illingworth, 2001). This may lead to enhanced motivation for participants and researchers to act as “co-researchers” and increase the scope for participants to become active in the interpretive process (Murray & Sixsmith, 1998). Ward (1999) has suggested that these features of the online interview environment might also encourage greater levels of researcher reflexivity, for example, by encouraging interviewees to direct more probing and bold questions to the researcher. Of course, participant

empowerment may emerge from the online mode of interaction by enabling participation by groups who may otherwise be largely excluded or find it particularly difficult to participate in offline FTF contexts (e.g., due to travel constraints, etc.). For some participants, opportunities to participate and have a “voice” may thus be enhanced by the online access medium; this could include, for example, the new mothers interviewed by Madge and O’Connor (2002) or people with physical disabilities (Bowker & Tuffin, 2004). On the flip side, individuals who do not have ready access to computer equipment and an internet connection, as well as those who may find the medium prohibitive due to literacy or typing dexterity issues, may be excluded by IMR. In asynchronous rather than synchronous online interview approaches, participants will have the highest levels of control over how, when, and where to participate. Even in synchronous approaches, however, participants are able to easily leave the online chat discussion at any stage, without consequences or embarrassment. One obvious and well-cited potential advantage of IMR approaches in general, and of online interviews in particular, is the scope for uniting participants (and researchers) located in different time zones and geographical locations around the world, particularly when asynchronous approaches are utilized (e.g., Hewson, 2007). This may serve to connect individuals who simply would not come into contact in an offline research location.

These key issues that emerge in an IMR interview/focus group context should be considered when making design choices; within any particular research context, selecting the procedures, tools, and techniques that are likely to be most helpful and successful will rely on keeping these various issues, options, and their implications in mind. The choice regarding whether an online or offline setting might be most beneficial, as well as decisions about which of the various online approaches might be best for any particular research context, relies on an awareness of these issues. Clearly, some potential tensions (or trade-offs) can emerge in IMR interview and focus group design: the absence of extralinguistic cues may incur both disadvantages (ambiguities, shallow levels of meaning, etc.) and benefits (enhanced candor and disclosure through increased anonymity levels). Techniques for establishing good levels of rapport, as described earlier, may help enhance depth, reflexivity, and engagement with the research process, but possibly reduce participants’ willingness to disclose very personal and sensitive information due to lower levels of anonymity. These speculations

all deserve further investigation. At present, there is good evidence that establishing rapport with individuals in online interviews is associated with obtaining high-quality data (e.g., Jowett et al., 2011; O’Connor & Madge, 2003). It seems that a range of factors will ultimately impact upon the nature of the data that are likely to be obtained in both synchronous and asynchronous online interview approaches, and further studies and researcher reflections will help elucidate what these are and the types of effects they may have. Inevitably, it seems there will be trade-offs to manage in making the choice between synchronous and asynchronous approaches and the various other design options available. To restate what is perhaps by now somewhat of a cliché, decisions need to be made depending on the particular research context, question, and goals at hand. The discussion so far has hopefully provided researchers with some indication of the issues and choices that emerge and the evidence regarding their respective benefits, drawbacks, and implications. Of course, one option is to use both synchronous and asynchronous interview approaches within the same study—indeed, some researchers have implemented and/or recommended this approach, thus reaping the benefits of each approach. Another option is to use both on- and offline methods (e.g., James & Busher, 2006; Orgad, 2006; Sanders, 2005), although some authors have criticized what they see as the often too prevalent supposition underlying this strategy that online methods cannot be trusted by themselves to provide high-quality data and therefore should only be used in tandem with traditional FTF methods (O’Connor et al., 2008).

One important point worth emphasizing, and one that is largely supported by the discussion so far, is that qualitative interview researchers may not necessarily be best advised to focus on trying to recreate the offline FTF interview context as closely as possible. Rather, there may be features and characteristics of online interview methods and strategies that are distinct from those typically used in offline FTF approaches, and these could usefully be adopted to take advantage of the benefits they may confer (such as the potential enhanced levels of disclosure in nonproximal contexts without visual cues). Thus, in some situations and for some purposes, online methods might create distinct, alternative contexts and interaction/communication modes that may have advantages over traditional offline approaches. Attempting to mimic as closely as possible traditional FTF methods is not necessarily going to prove the best strategy; recognizing that

different approaches may need to be taken in online environments—and that these may have their own particularities and potential benefits—is important (Jowett et al., 2011).

SAMPLING, RECRUITMENT, ETHICS

Here, consideration is given to the different sampling strategies that can and have been used in qualitative interview and focus group IMR and on deciding which of these may be the best choices for particular research contexts. Ethical issues that emerge are also highlighted and discussed throughout. Essentially, most of the technologies already described as being supportive of conducting online interviews can also be used to contact and recruit potential participants. Thus, e-mail and mailing lists, online discussion forums, social network sites, and other readily accessible “public” spaces supporting online communities and groups involved in social interaction (e.g., special interest webpages, blogs, etc.) provide potential points of initial contact. It is worth noting that the concerns about sampling and recruiting participants online that were prevalent in the early days of IMR—due to the perceived extreme levels of bias inherent in the demographics of the internet user population (IUP) (e.g., see Bordia, 1996; Schmidt, 1997)—are now rather less worrying. This is in large part due to the expanding ubiquity of the internet in peoples’ daily lives, thus rendering the IUP far less biased than it once was (Hewson & Laurent, 2008). Given that qualitative researchers typically do not require large-scale representative probability samples that enable generalization to a wider population, the issue was perhaps always of less concern in qualitative IMR. Nevertheless, qualitative researchers have also expressed concerns about sample representativeness (e.g., McDermott & Roen, 2012). Also, qualitative researchers often strive to obtain data from individuals who represent groups of particular interest and relevance to their research domain, and these target groups may be specialist and/or traditionally hard-to-access. Restrictions in the breadth and scope of the IUP thus become relevant. Arguably, the internet expands the potential for reaching such specialist groups, as well as for potentially being able to facilitate data collection from a larger group of participants than is often possible in offline qualitative research (primarily due to cost and time savings). Online sampling procedures allow access to hidden, select, specialist populations in a way that would be very difficult, if not impossible, to achieve offline (as evidenced by, e.g., Baker

& Fortune, 2008; Barratt, 2012; McDermott & Roen, 2012; Temple & Brown, 2011).

One option is to use e-mail to directly contact potential participants, assuming e-mail addresses can be obtained, such as in the form of mailing lists (e.g., see Parsons, Koken, & Bimbi, 2004; Strickland et al., 2003). However, care must be taken here regarding issues relating to privacy, netiquette, and spamming. Any such directly solicited e-mail requests should be carefully passed through any moderators or managers of relevant mailing lists, whose approval can help minimize the chance of the invitation being received negatively or treated with hostility. “Cold calling” e-mail requests may not be welcomed by some individuals. As a general rule, one ill-advised option is to purchase large lists of e-mail addresses from commercial providers because it is likely that many listed individuals have been included without full awareness (e.g., by not opting out during a purchase transaction or commercial website enquiry). Use of such lists may thus be considered ethically dubious, as well as potentially raising issues for data quality and integrity. Exceptions to this rule might occur when a list is obtained from a trusted, reliable, well-known source and where the sampling and recruitment procedures for adding contacts to the list are clear and deemed acceptable. Online panels may offer such a solution, although typically these are more appropriate for survey-based IMR, which is what panel members have often explicitly signed up for.

Online discussion group forums (in their various forms), as outlined earlier, also present a possible way of reaching potential participants (e.g., see Murray & Sixsmith, 1998). Posting participation requests to existing discussion groups is an option and may seem quite acceptable when these groups are fully open. However, some groups will have restricted access so that not everyone can join. Posting a message within a discussion group will typically require signing up to obtain an account. Careful consideration of netiquette and privacy is also required when using this approach. Although arguably less invasive than sending an e-mail directly to an individual, small support-network discussion groups (often concerned with sensitive topics and issues) may not welcome the intrusion of an out-of-the-blue request from someone outside the group looking for research participants. Conversely, some groups might actually welcome such requests and the opportunity to disseminate their views more widely (e.g., political activist groups). As always, care and sensitivity is required

when deciding when it is likely to be appropriate to approach online discussion groups in this way. Certainly, a suitable introduction should be constructed, naming the researcher's affiliation and clearly describing the nature of the research and participation request. As with mailing lists, contacting moderators of discussion groups or websites (where they exist) is also good practice before posting any requests. When a researcher is actively involved in an existing discussion forum as an established group member, requests might be received more warmly (although this scenario may be relatively uncommon). In a similar vein, websites and social networking sites may present a useful target location for such requests, bearing in mind all the same considerations regarding ethics, privacy, and netiquette. It is essential to consider whether it is appropriate for requests to propose conducting research interviews within an existing discussion space or outside it, in a dedicated research area. Often, the latter will be appropriate, so as not to disrupt existing spaces where members' primary objective is not to take part in research interviews. Also, researchers should be aware that ethical issues can arise surrounding the security, privacy, and anonymity of data derived through online discussions, particularly when these occur in open, publicly accessible spaces. Often, the archives of existing groups are readily available online for searching and locating specific topics, themes, and even quotes. Constructing a dedicated space in which to conduct a research interview or focus group allows for greater researcher control over the data generated and who is able to access it.

Of course, offline sampling approaches can also be used to recruit participants for IMR studies, who can then be directed to the relevant online participation space. This would be appropriate if the target group is unlikely to be accessible online (e.g., because of not having ready internet access; such as the homeless). Kenny (2005) has used this approach, purposively sampling to recruit nurses registered in a conversion course in an Australian rural university; the nurses then went on to take part in a focus group discussion using WebCT. Participants were given an offline training session on how to use WebCT initially, an important part of the recruitment process given the range of skills and experience within the group. Of course, if participants recruited offline do not already have internet access, then resources or appropriate strategies (e.g., using a public computer) will need to be offered to enable participation. Conceivable contexts in which this might offer an advantage over using offline FTF

approaches occur when, for example, participants of interest are widely geographically dispersed: third parties could be used to assist with recruitment (e.g., in centers offering services for the homeless) and then provide access to a computer to enable participation in the study. This could potentially facilitate research that would be much more costly and time consuming, or even impracticable, offline.

Deciding which of these options is most appropriate—assuming netiquette and privacy issues have already been taken into account—will be largely driven by the nature of the sample required. A particularly appealing aspect of sampling online is the ability to readily search and gain access to topic-specific interaction spaces, which, as noted earlier, may serve to help reach specialist and/or hard-to-access populations. A good number of authors have reported successfully using this strategy (e.g., Murray & Sixsmith, 1998; Parsons et al., 2004; McDermott & Roen, 2012), using a variety of methods. Parsons et al. (2004) searched and acquired e-mail addresses of male escorts who then were asked to take part in offline interviews. Murray and Sixsmith (1998) posted invitations to specialist discussion groups and mailing lists, and McDermott and Roen (2012) and Baker and Fortune (2008) made use of specialist topic-relevant websites to recruit participants from relevant groups of interest (see also Madge & O'Connor [2002], who recruited parents by placing links to the research study website on appropriately selected topic-relevant existing websites). These authors often point out the enhanced online opportunities for accessing specialist populations who are otherwise very difficult to reach offline (e.g., McDermott & Roen, 2012). Access to such specialist populations is thus a clearly demonstrated success of interview-based IMR approaches.

Accessibility issues were raised earlier in relation to using online technologies and procedures that can enable participation by intended groups: the same consideration emerges with respect to online sampling strategies. Thus, recruiting via online discussion groups, social networking sites, mailing lists, and the like might either restrict or facilitate participation by certain groups and users, depending on who uses these various online spaces. Considering who likely uses various online technologies and spaces is thus important in deciding which sampling procedures to employ. E-mail, for example, is likely to reach a broader and more diverse user group compared with online chat or social networking spaces where the user demographic is likely to show greater bias toward younger, more technologically

proficient and highly computer literate users. Intended geographical reach and the empowering of participants may also become relevant here, particularly in relation to the technologies chosen to implement study procedures; for example, synchronous approaches may prevent ready interaction between individuals in different time zones compared with asynchronous approaches that can more readily accommodate time zone discrepancies. Likewise, synchronous approaches may restrict users with limited typing fluency from taking part (and so will not be appropriate when such groups are the target population). Some of these points may seem obvious, but others could be more easily overlooked without giving due care and attention to the details when planning research sampling.

A set of ethical issues emerges around confidentiality, anonymity, and data security in IMR more generally and online interviewing in particular. E-mail is a relatively nonsecure transmission method because e-mails are typically stored on a number of different servers as they move from sender to recipient. The possibility of interception is therefore relatively high compared with other traditional (e.g., collecting and storing data in a locked filing cabinet in a locked office) and online (e.g., sending data directly to a secure university-managed server) methods. Maximizing the security of data storage and transmission methods online is imperative, particularly when data are of a highly sensitive and personal nature. Strategies for enhancing the security of e-mail exchanges include setting up research-dedicated accounts with all identifying information and real names removed and/or using encryption software solutions. These options are worth considering. However, it should be borne in mind that simply removing real names is often not sufficient for ensuring the anonymity of rich, elaborate, personal qualitative data. Related to these considerations is the ethical obligation of researchers to make participants aware of any security risks (it is very common for an informed consent information statement to assure participants that all data will remain confidential and/or anonymous). Many participants may not be aware of the possibilities of e-mail interception or of the searchable, readily accessible nature of many discussion forum archives, for example. Researchers should thus be aware of these potential risks to breaching participant confidentiality and consider the extent to which participants need to be made aware of these risks. Guaranteeing through informed consent protocols that all data will be stored anonymously

and confidentially may, arguably, be at best misleading.

Confidentiality and anonymity issues can also arise after participation has occurred—at the research dissemination stage—due to the already noted readily searchable and locatable nature of online archives (e.g., of discussion forums) in a way generally not possible in offline methods. For example, should a researcher conduct a focus group discussion within an existing online discussion space (with all members' prior consent) and then proceed to publish and disseminate the results of the research study, including carefully anonymized quotes, it is quite possible that these quotes could then be entered into a search engine, located in their original context, and attributed to the original author. Thus, the traceability of much online interaction creates additional ethical concerns in IMR. On this point, e-mail exchanges may have an advantage because they are not readily available to search in online archives in the same way. Setting up a private discussion group for research purposes and making sure that public accessibility to this is restricted is one viable solution (as already mentioned). In addition to researchers being aware of these security and confidentiality issues so that they can take steps to maximize the protection of participants and minimize risks, participants also should be made aware of any nontrivial threats to the confidentiality of their data so that they can properly assess the risks involved and make an informed decision on whether to participate. This includes threats arising from planned dissemination procedures. Synchronous online discussions, in general, typically do not automatically generate online archives as publicly available and searchable discussion logs (although it is possible to save real-time discussions, e.g., by cutting and pasting into a text document), so they may be less troubling in this respect than asynchronous approaches.

A further relevant ethical issue concerns restricting access to online research sites, in the sense of the extent to which the researcher is able to verify and maintain control over who takes part. Thus, in the context of research on sensitive topics, verifying participants' identities and restricting access where appropriate may be essential. Research with young people (under 16 years of age according to the British Psychological Society ethics guidelines [BPS, 2011]) requires informed consent from a parent or guardian. In an IMR context, verifying whether someone is under a certain age can be problematic, as can verifying whether someone is

able to give valid informed consent for other reasons (e.g., where special needs might be an issue). Researchers should not risk allowing participants who are unable to give proper, valid informed consent to take part in an online interview when sensitive and potentially upsetting themes are involved (in other situations, a certain level of risk is often considered inevitable because it is rarely possible to completely verify participant identity online). Obtaining parent/guardian consent in research involving young people should not necessarily be ruled out in IMR, but very carefully thought-out, rigorous, robust procedures should be ensured if this approach is used. Given the enhanced scope for deception in non-FTF settings, these procedures will need to be foolproof and will typically (if not always) involve offline contact. Arguably, research with young people or other vulnerable groups who are not able to give informed consent should not be attempted online, except in circumstances in which it is clear that robust and reliable procedures for gaining gateway (e.g., guardian) consent are possible. A good general ethics principle, which applies well to IMR, is that, as the potential risk to participants increases (e.g., as in research involving sensitive and potentially upsetting topics), the need for procedures in which fundamental ethics principles (e.g., securing reliable informed consent) cannot be violated also increases. Although further research is needed to discover how different sampling methods and recruitment sites will generate certain types of samples in IMR, this should be considered carefully at the design and planning stage of any IMR study, including when contemplating the use of interviews and focus groups. Some groups will not have access to even relatively widely used internet technologies such as e-mail (e.g., the homeless or illiterate) and thus will be excluded from approaches that use these sources as sampling and recruitment tools or for administering online procedures. Thus, sometimes, offline methods may still be necessary. At other times, online methods will allow access to groups that are largely inaccessible offline (e.g., McDermott & Roen, 2012). On the topic of ethics in online interview research, resources that provide informative further discussion of the issues raised here include Brownlow and O'Dell (2002) and Rodham and Gavin (2006).

Box 21.1 summarizes the key design choices and recommendations to keep in mind when planning IMR interviews and focus groups. For each principle listed, the corresponding core research design

criterion (reliability, validity, ethics, accessibility) to which it is particularly relevant is given in parentheses. Although it is recognized that the term "validity" may often be considered less appropriately applied to many qualitative research contexts, it is used here as shorthand to also include recognized qualitative equivalents such as integrity, authenticity, credibility, and so on.

Online Observation

The scope for conducting social and behavioral observational IMR seems potentially vast when one reflects on the extensive archives of easily searchable and accessible traces of human interaction available online, primarily in the form of text-based exchanges that are created every day. These archives can potentially provide rich sources of data for qualitative observational research. Before looking at the range of strategies and methods available in qualitative observational IMR, it is worth noting that the boundary between this and other methods can become somewhat blurred in an online context. For example, fully disclosed participant observation may share much in common with online focus group approaches. Observation and document analysis approaches also may not be always clearly distinguishable; for example, blogs, which started as relatively static but regularly updated online personal diaries, now often serve as dynamic, rapidly changing, interactive spaces where various people, as well as the blog author, can post comments and replies (Wakeford & Cohen, 2008). Blogs as published documents, as interactive, social communication spaces, or as something in between can thus potentially provide data for IMR approaches that may be seen as more or less closely approximating "observation" or "document analysis." Given such potential ambiguities, for present purposes, the suggestion by Hewson (2007) will be adopted, in which a working definition of online observational research is "that which uses logs of interactions (typically verbal exchanges) between participants, as opposed to document analysis which makes use of static records constructed specifically for the purpose of dissemination via the internet, and whose primary purpose is not to facilitate an ongoing dialogue-type communication between individuals" (p. 416). Clearly, this is just an approximation, with blogs being a good example of an online phenomenon that may arguably be treated as either a document or an ongoing social interaction (indeed, there are many forms of blogs, each of which may more or less closely fit either of these categories). Also, observation online may feasibly occur

Box 21.1 Principles for good practice in qualitative internet-mediated interview and focus group research

- Use robust, well-tested procedures that have been well-piloted (*reliability*).
- Use the simplest low-tech solutions and equipment available that will do the job (*reliability, accessibility*).
- Use appropriate procedures for verifying identity (e.g., offline, audio/video) where this is crucial (e.g., highly sensitive research) (*ethics, validity*).
- Adopting clear strategies for establishing rapport has been shown to work well and is advisable (*validity*).
- Remain mindful of potential trade-offs when deciding on procedures and making design choices; for example, asynchronous approaches may facilitate depth and reflexivity but reduce conversational “flow” (*validity*).
- Related to the above principle, remain aware of possibilities for combining methods—online and offline, asynchronous and synchronous, etc. (*validity*).
- Carefully consider security and confidentiality risks when making sampling and procedural design choices and the ethical responsibility to inform potential participants of any nontrivial risks they may be unaware of (*ethics, validity*).
- Adopt procedures for maximizing security and confidentiality where possible (e.g., setting up a dedicated online research site) (*ethics, validity*).
- Remain mindful of the possible threats to participant confidentiality and anonymity that can emerge from dissemination and publication procedures, and take careful measures to minimize these threats (*ethics*).
- Respect standards of privacy and netiquette, and pass participation requests through moderators where appropriate (e.g., sampling from mailing lists or newsgroups) (*ethics*).
- Make sure participation requests are well-constructed, containing information on researcher affiliations, contact details for further information, and value of the research (*ethics, validity*).
- Carefully consider how different sampling approaches and design procedures may facilitate or restrict access by different groups (*accessibility, ethics*).

in real time and not necessarily involve using “logs” of interactions. However, adopting this approximate definition, with the aforementioned provisos noted, this section considers the various opportunities and good design practice principles for qualitative observational approaches in IMR.

TOOLS, TECHNOLOGIES, PROCEDURES

The main tools to support observation approaches in IMR include many of those already mentioned to support online interviews. Thus e-mail, mailing lists, asynchronous discussion forums, and synchronous chat all create opportunities for observing primarily linguistic interactions. In addition, blogs, social networking sites, and other spaces supporting linguistic interaction create potential sources of data. Some of these online spaces (e.g., social networking websites) also incorporate multimedia exchanges, such as the sharing of photographs and music, and this opens possibilities for expanding observational research beyond the purely linguistic. Multimedia sharing websites such as YouTube (youtube.com) could also offer opportunities for gathering data for

qualitative analysis that moves beyond purely linguistic text-based exchanges. The extent to which such spaces provide examples of “interaction” can be ambiguous; for example, YouTube allows subscribers to post video clips that may be considered published “documents” or “records,” but that can be commented on by others, often generating an exchange of opinions and views on the video content itself. The more enduring online discussion spaces (e.g., Usenet groups) and the more recent emergence of Web 2.0 and its associated interactive technologies seem undoubtedly to create substantial and diverse possibilities for supporting observational research. The rich and detailed nature of many online social interactions, particularly those arising from asynchronous discussion forums, presents expansive possibilities for qualitative observational research in IMR. It is beyond the scope of this chapter to outline detailed procedures for locating and searching archives of mailing lists, discussion forums, and other online spaces, but many of the resources noted earlier offer helpful guides for doing this (e.g., for more about LISTSERV and how to

search mailing lists, see <http://www.lsoft.com/manuals/1.8d/user/user.html>; it is also possible to search Usenet archives via the easy-to-use Google groups web interface, <https://groups.google.com/forum/?fromgroups#!browse>).

A particularly intriguing possibility emerges from the presence of online virtual reality environments (VREs). These set out to mimic in online, graphic, multimedia spaces certain offline physical environments and contexts and thus create opportunities for real-time, multimedia observation within these spaces. A well-known example is SecondLife (secondlife.com, see also <http://secondlife.com/whatis/>), which consists of a very large number of three-dimensional (3D) graphical spaces (cities, bars, universities, museums, parks, etc.) that users' avatars can port to and navigate through, exploring environments and interacting with others. In 1997, SecondLife reported a total of 7 million users (Hewson, 2008); in 2011, it reports more than 1 million users logging on each month. The massively multiplayer online role-playing game World of Warcraft (WoW) is another example; recent reports indicate around 10 million subscribers in the latter half of 2012 (source: <http://www.statista.com/statistics/208146/number-of-subscribers-of-world-of-warcraft/>; accessed January 2012]). Visiting the home pages of these and similar VREs provides information on how to set up an account and enter and interact within the virtual space. Although examples are not yet abundant, some authors have reported attempts to conduct qualitative observational research in such spaces (e.g., Williams, 2007).

DESIGN ISSUES AND STRATEGIES

Several options and design choices arise in qualitative observational IMR. Some of these, particularly the choice between participant versus nonparticipant and disclosed versus undisclosed approaches, are guided by much the same principles as emerge offline. Thus, for example, researcher participation approaches (particularly when disclosed) can tend to lead to disruptions in naturalistic behaviors. These points will not be reiterated here because our focus is on issues and considerations that arise particularly in an IMR context. Choices regarding whether to observe asynchronous or synchronous exchanges or utilize stored archives versus live, real-time interactions also emerge. It is worth noting here that the choice of supporting technologies and data sources (e.g., mailing list archives, online

chat, etc.) can restrict or facilitate particular design choices, such as whether to use participant or nonparticipant approaches, overt or covert strategies, and so on.

Accessing Asynchronous Discussion Archives

Essentially, the ready availability of a mass of stored online archives of (for the most part) asynchronous communication and interaction enables nonparticipant approaches that locate, search, and extract relevant data from these archives. Usenet discussion group archives (discussed earlier) are an obvious example of such data sources. Given the impracticalities, or indeed the impossibility, of contacting all contributors to such discussions "after the event" to request consent to use their contributions as research data, this approach will invariably lead to nondisclosed observations (the ethical acceptability of nondisclosure in various contexts is addressed further later). An early example of a covert, nonparticipant observation has been reported by Bordia (1996), who was able to search for and locate instances of rumor transmission using Usenet, the internet, and Bitnet. Although Bordia's analysis approach was essentially quantitative, this study demonstrates how IMR methods can potentially facilitate access to rich, content-specific linguistic data in ways often not possible using offline methods; such data could also be valuable for qualitative research projects. Brady and Guerin (2010) present a more recent example of this approach in which they collected discussion board posts covering a two-week period from the archives of an online parenting support group; they then subjected these data to qualitative analysis.⁵ Searching mailing lists (e.g., LISTSERV lists) can be more involved than searching Usenet or website archives; see Bordia (1996) for an account of the type of procedure involved (note that some lists can also be searched via a web interface, thus making the process a little simpler). The advantages of using stored archives in this way include gathering data in a particularly convenient, time- and cost-effective manner compared with many offline and other online approaches and being able to readily locate content-specific examples in ways often not possible offline.⁶ Such techniques also can help facilitate access to potentially greater numbers of cases than is often possible offline (e.g., Bordia, 1996). Where nonparticipant, undisclosed approaches are methodologically and ethically appropriate and justified, this IMR strategy is an attractive option.

“Real-Time” Observation of Asynchronous Discussions

An alternative strategy for observing online asynchronous discussions is to follow them in real time. Thus, instead of accessing logs of discussions that have already taken place, a researcher may subscribe to a group and follow an ongoing discussion as it unfolds. Of course, the extent to which this approach can be truly considered “real-time” is a little unclear: unless a researcher is online 24/7, he or she will also be accessing stored posts that were sent in the past, thus blurring the distinction between real-time and archived sources in IMR. For nonparticipant observation approaches, it is not obvious why a real-time strategy would confer any advantage over accessing stored archives, given the extended timescale required and the generally greater effort involved in pulling all data pieces together into a composite dataset. Indeed, the time stamps available in archived logs mean that no extra information is actually gained from following an ongoing discussion in real time. A real-time strategy is beneficial, however, when a participant observation approach is desired. Clearly, using logs of past discussions does not allow any scope for researcher intervention, whereas following a discussion group during its ongoing conversation around a topic does. Fox, Ward, and O’Rourke (2005) carried out a participant observation in real time by following discussions on a pro-anorexia website. They participated in discussions on the website message boards (e.g., by posting questions) and also engaged in personal exchanges with individuals who chose to contact them directly (also leading to one-to-one interviews). Given the sensitive nature of the topic, these researchers felt that full informed consent was appropriate, both for accessing and collecting posts to the website and for publishing any comments or quotes from individuals. Brotsky and Giles (2007) also carried out a participant observation online in real time, but, unlike the previous study, this was done covertly: one researcher joined several pro-anorexia online community websites by assuming a plausible persona in an attempt to gain insights into the beliefs, perceptions, and behaviors of group members. The ethical implications of adopting either disclosed or undisclosed participant observation in this way are explored further later. Of course, the choice between disclosed and undisclosed approaches can also become relevant in nonparticipant approaches, where it can also be an option—at least in those cases where disclosure becomes practicable (via routes for reliably contacting all potential participants). Generally, disclosure

in nonparticipant observation becomes more viable when such research is carried out in real time rather than by accessing archives. Aho, Paavilainen, and Kaunonen (2012) adopted a disclosed, nonparticipant approach in their longitudinal qualitative study of mothers’ experiences after the death of a child; messages were collected unobtrusively from a private internet support group website over a period of five years, after first obtaining consent from participants. Participant observation approaches also may be preferable in ethnographic research contexts where immersion within a community is often seen as a crucial part of the research process. Often, ethnographic researchers will adopt mixed- or multimethods strategies (e.g., involving observation, interviews, surveys, document analysis). There has been a fair bit of interest in ethnographic approaches in IMR: for comprehensive early discussions of the approach, the reader is referred to Hine (2000) and Markham (1998). For a more recent overview, see Hine (2008), and for a recent example of the approach, see Tackett-Gibson (2008) who, after gaining permission from website administrators, carried out an ethnographic observation of online communities engaged in exchanging drug use information. Interestingly, whereas Tackett-Gibson had initially proposed to fully disclose the research and actively take part in online discussions, the website moderators felt this would not be appropriate and restricted involvement to observations via lurking and accessing website discussion archives. This illustrates the need for researchers to be aware that moderators may have perspectives and agendas that differ from their own and that these may potentially hinder (as well as sometimes facilitate) the initial proposed research process and goals. Although contacting moderators seems a necessary step if using disclosed approaches (moderators will likely see any announcements and intervene when these have not been authorized), arguably, contact may not always be necessary when using nondisclosed approaches, particularly when there are strong arguments that this may lead to an unjustified “blocking” of valuable research.

Observation of Synchronous Discussions

It is also possible to observe synchronous discussions, and these are most likely to involve live, real-time chat contexts (stored archives of synchronous discussions being less readily available; but see Al-Sa’Di & Haman, 2005). Examples of the successful use of streamed online chat in observational IMR are available (e.g., Al-Sa’Di & Haman, 2005;

Rodino, 1997). Following live synchronous discussions using undisclosed, nonparticipant observation approaches may at first impression seem untenable because this would require lurking within a live discussion group setting—a behavior unlikely to go unnoticed or be welcomed. In larger synchronous discussion group settings, however, such lurking may be less noticeable and more feasible (although still rather more noticeable than in asynchronous contexts). Rodino (1997) lurked in internet relay chat (IRC) sessions in order to conduct a qualitative analysis of gendered language use, and she does not report any particular problems with the approach. Likewise, Al-Sa’Di and Haman (2005) report no problems with lurking to collect data in this way in a quantitative study of “e-English.” Hudson and Bruckman (2004), conversely, do report less positive experiences; they carried out a (primarily) quantitative study to look at responses of chat room participants to disclosures of observation intent (recording the discussion) by researchers, as well as reactions to undisclosed entry and lurking. In both cases, they experienced hostile responses and were often kicked out, although this happened less often when merely entering and lurking. However, these researchers made no attempt to engage with participants in any way that might resemble strategies to establish good levels of rapport (e.g., as discussed by O’Connor & Madge, 2003). It appears that a number of factors may influence the reactions of potential participants to researchers who attempt to carry out observational research in synchronous discussion settings, either with or without disclosure. Further research examples will help to identify which procedures and practices are likely to work best in this context.

On the whole, reports of attempts at observation in synchronous discussion contexts have been rather less common than in asynchronous contexts. Still, both nonparticipant and participant approaches, disclosed or undisclosed, seem a possibility. Reasons for choosing to observe synchronous rather than asynchronous discussions involve many of the same considerations that were raised in relation to online interview approaches; for example, asynchronous approaches may lead to more reflective, detailed, well-considered data, but perhaps lack conversational “flow.”

Observations Moving Beyond Linguistic Interaction

So far, focus has been on possibilities and methods involving essentially linguistic observation. With the emergence of Web 2.0 and its associated technologies (social network sites, interactive

blogs, multimedia sharing spaces, etc.) observation opportunities in IMR have extended beyond purely text-based interactions. Thus, the social network site Facebook creates a space where mutual friends in a social network group can post comments, pictures, sound clips, videos, and web links on each others’ “walls” and thus create rich, multimedia patterns of communication. Although access to personal walls is often restricted (e.g., to family, friends, friends of friends, etc.), Facebook also supports open, publicly available discussion groups that can be searched by topic and their posted content accessed freely. To revisit an earlier theme of some of the studies cited, I carried out a search using the term “anorexia” and found a list of hits, the first of which was an open, freely accessible group containing postings of images, comments, requests, web links, adverts, and video clips. Many of these posts were in languages other than English, thus demonstrating the possibilities of IMR for broad geographical reach. Blogs may offer similarly rich multimedia sources that could support qualitative online observational research. Google Blog Search (<http://www.google.co.uk/blogsearch>) allows blogs to be searched and accessed by topic. As noted earlier, such resources can blur the boundary between online observational and document analysis approaches (in the next section, examples of what may reasonably be considered the latter approach are considered).

A particularly intriguing and exciting opportunity for online observation emerges from VREs. To date, relatively few qualitative studies have utilized these online spaces to conduct social research, with most existing examples coming from computer science and educational applications and/or utilizing an experimental paradigm (the approach offering good scope for experimental manipulation of variables; see Schroeder & Bailenson, 2008, for an informative overview). However, the potential for conducting both qualitative interviews and observational ethnographic research in such environments has been noted (Bainbridge, 2007). Williams (2007) has discussed the scope for implementing participant observation approaches in VREs while also noting the lack of attention to this approach to date. Williams (2007) describes an online ethnography study in which he gathered detailed field notes in the graphical environment Cyberworlds, supplementing observations with online focus group interviews. He considers some of the challenges associated with using VREs for conducting participant observation compared with solely text-based approaches online or with offline

approaches. One noted advantage of IMR methods concerns the enhanced scope for recording detailed field notes covertly; for example, screen recorders can be used to capture detailed, accurate recordings in IMR (Williams, 2007). A range of future possibilities are conceivable in qualitative research using VREs, and a range of design options seems tenable. Thus, using existing virtual environments such as SecondLife may be appropriate in some contexts, or, alternatively, researcher-constructed environments might be more suitable when specific controlled settings are required. SecondLife can support both approaches because it allows registered users to set up their own “island” that people can visit. The extent to which such approaches will become fruitful avenues for qualitative social science research remains largely anticipated as more examples of studies adopting this approach are forthcoming.

SAMPLING, RECRUITMENT, ETHICS

Because in many cases IMR (and offline) observation will be covert (e.g., by accessing existing discussion archives), direct recruitment of participants is often not required. Exceptions may occur when a researcher chooses to set up a dedicated research environment (e.g., VRE or online chat space) and “bring participants to the research setting”; in this case, options and issues emerge that are similar to those discussed in relation to recruiting interview participants. Sampling of data sources from online interaction and communication spaces for observation is often simply a case of using tools that allow a search for topic-relevant sources, as discussed previously. Synchronous approaches that gather data in real time from existing online spaces may involve directly recruiting participants (e.g., by posting requests to existing discussion forums), but many of the same principles and techniques for searching and locating topic-relevant themes and discussions also apply here. Much of the focus of this section is thus on the ethical issues that arise, several of which are contentious and require further debate, clarification, and resolution. These debates center particularly on the issue of the distinction between public and private spaces online, including considerations regarding the ethical appropriateness of covert observation strategies that do not seek permission or informed consent from participants for data usage.

Disclosed observation (in which contributors are made aware of the research) will involve recruitment, at least in the minimal sense that using data from any participant who has expressly declined

giving permission is precluded (using such data would be very difficult to justify ethically). Thus, by not declining, it may be considered that participants have given permission by not opting out. More rigorous procedures for gaining informed consent, similar to those employed in (offline and online) reactive methods (e.g., interviews, surveys, etc.), may be considered appropriate by many researchers using disclosed approaches. This might involve obtaining signed consent forms (or their online equivalents) in which participants indicate that they have read and understood a participant information sheet. Several researchers have advocated and implemented disclosure in observational IMR (e.g., Aho et al., 2012; Brownlow & O’Dell, 2002; Ferri, 2000). Obtaining consent for the publication of any quotes derived from online observational research has also been considered important ethical good practice (e.g., Elgesem, 1996; Sharf, 1999). However, other researchers have taken a rather different stance, arguing that in cases where potential data can be considered to be “in the public domain,” undisclosed observation is ethically justified and acceptable. Public discussion group spaces and archives are perhaps the most likely sources to be considered in the public domain and have indeed been used as data sources for observational IMR without disclosure (e.g., Brady & Guerin, 2010; Bordia, 1996; Denzin, 1999). Some authors have argued that such undisclosed observation of publicly accessible resources is ethically justified as long as careful measures are taken to protect the anonymity and confidentiality of contributors (e.g., King, 1996), whereas others have considered it appropriate to gain consent from discussion group moderators but not (necessarily) from discussion group members (e.g., Brady & Guerin, 2010).

Clearly, there is a range of opinions on the topic of disclosure in observational IMR, and the issue remains unresolved, with no clear consensus. At the heart of these debates is the question of how to treat information sources that are readily available in the (online) public domain, as well as what constitutes being “in the public domain” in an online context. There is a far greater consensus in endorsing the key principle that private communications (e.g., personal e-mails) should not be used without consent (e.g., Sharf, 1999). This follows established offline research ethics practice (e.g., BPS, 2011). Without a clear set of currently agreed-upon guidelines to follow, it is important to consider a number of factors that may help direct researchers in making appropriate and ethically sound decisions regarding

whether and when disclosure is necessary or non-disclosure is justifiable in observational IMR. The decisions made will no doubt interact with the particular methodological strategies, research contexts, and practicalities involved. Regarding the latter, as noted earlier, it may often be impracticable if not impossible to reliably contact all participants who took part in an archived discussion session, thus rendering it implausible to disclose research intentions and/or gain informed consent. Indeed, it may often also be difficult to reliably contact all members of an ongoing asynchronous discussion session due to the fluid nature of online spaces and individuals' presence within them. This issue becomes particularly relevant when considering discussion spaces that support very large (and often varying) numbers of users. When the scientific value of the research is high and the risk of harm low, such practical constraints may provide good arguments for utilizing undisclosed observation approaches. However, when levels of risk of harm to participants are higher (e.g., in research on particularly sensitive or personal topics) and/or when data are likely to be traceable to individuals, then nondisclosure may be less easy to justify. The level of risk of harm, then, emerges as a key factor to be taken into account when making design choices related to disclosure and consent, along with practical and scientific value considerations. One argument against disclosure is that it may have a negative impact on the integrity and validity (and hence scientific value) of the data obtained. Thus, Reid (1996) has reported that disclosing the research process to participants in a multiuser dungeon (MUD) altered their behavior, leading to the manufacture of "quotable" verbalizations for the attention of the researcher. Such possible effects should be weighed up alongside those factors and principles already noted.

Another factor to be considered, and one which is arguably fundamental, is the extent to which the individuals who use online spaces (e.g., discussion forums, blogs, etc.) would consider them to be "in the public domain," including whether they would be likely to agree to the information they have contributed being used for research purposes. Just because a technologically proficient and experienced researcher is able to gain access to online spaces and archives, this does not necessarily mean that participants themselves are fully aware of this level of public access or that they are happy for their contributions to be acquired, analyzed, and published. Knowing what the *expectations* of users of online interactive spaces are is perhaps one of the central

issues in the ongoing debate over the public–private distinction online (e.g., Ferri, 2000). Arguably, if it is not reasonable to assume that participants would expect members of the public, including researchers, to view their contributions and possibly use these as research data, then researchers should not do so without first obtaining consent. However, determining what potential participants' likely expectations are, and what should thus reasonably be considered "public" and "private" in IMR, is not as straightforward as looking at whether the site is password protected or freely accessible. The issue clearly merits further research and discussion.

Returning to the point that certain settings may restrict the design options available, observation of synchronous real-time discussions may often preclude nonparticipant, nondisclosed observation approaches. Unless entering and lurking within such a discussion space is likely to go unnoticed (perhaps possible in groups with a large number of participants), a researcher may well be confronted by participants and interrogated or even kicked out, as reported by Hudson and Bruckman (2004). This situation could prevent a planned research project from proceeding. Furthermore, attempts to engage in covert observation by lurking can also have ethical implications. The principle of *social responsibility* (e.g., BPS, 2011; BPS, 2013) requires researchers to take care not to damage existing social structures. Lurking within a discussion group has the potential to disrupt and possibly damage the group, at best by temporarily interrupting the harmony, focus, and intended purpose of the group, and at worst by causing longer term damage, such as fostering mistrust, suspicion, and disrupting group cohesion. This could be particularly damaging for an online support group. Such ethical considerations may strengthen the case for disclosing any intentions to observe—as a nonparticipant—synchronous online groups. Participant, nondisclosed observation of a synchronous group might be more viable and less likely to cause disruption or damage, as long as the researcher is accepted as a genuine member. Of course, such deception will need to be carefully considered and justified, taking into account many of the factors raised earlier; as is often the case, trade-offs may become apparent, such as the risk of causing harm to the group through nondisclosure weighed up against the possible negative impact of disclosure on data integrity and quality. Different contexts may demand different resolutions.

Ethical issues also arise in relation to the analysis and dissemination of research data and findings;

in IMR, the enhanced scope for tracing published findings and data samples (e.g., verbatim quotes) back to their original sources must be considered. Particularly in observational research, where data may often originate from and remain available in publicly accessible archives (e.g., discussion group logs), researchers need to consider the potential for any published data to be traced and viewed in their original contexts, including along with identifying information about the author. As Reips and Buffardi (2012) point out, the structure and form of the internet creates possibilities for identifying individuals by piecing together various sources of information in ways typically not possible offline. The risk of revealing both the identities of and additional information about individuals who have contributed data becomes especially troubling when research has not been disclosed and no informed consent has been gained; with informed consent, at least participants can be made aware of the risks and will have agreed to take part nevertheless. At the very least, researchers should strive to maximize the robustness of the identity-protection procedures in place in an IMR setting (King, 1996), including protecting online (e.g., pseudonyms, usernames, avatar names, etc.) as well as offline identities (e.g., Frankel & Siang, 1999). One strategy to help overcome problems associated with the enhanced traceability of data online is to construct a dedicated research space that is protected and not open to public access, although this option is less useful when unobtrusive observation of naturalistic interactions is required.

In summary, there presently is no accepted set of standards for ethics in online observational research, with a key ongoing debated and controversial issue being when it is necessary to disclose research intentions and obtain informed consent and when it is acceptable to deem material freely available for use as data due to being readily accessible in the public domain. For further consideration of this and related issues, see the useful discussions in Brownlow and O'Dell (2002) and Ess (2007). Box 21.2 summarizes some useful guiding principles.

Online Document Analysis

There are relatively few examples of attempts at qualitative document analysis in IMR, compared with quantitative approaches (Hewson, 2008). Here, some existing examples are considered, and the scope of the approach is discussed. Some key issues to keep in mind when selecting between different methods and design choices are highlighted.

TOOLS, TECHNOLOGIES, PROCEDURES

All sorts of online documents exist that could be harvested as raw data for qualitative analysis: webpages (e.g., personal homepages), blogs (although many interactive blogs straddle the boundary between “documents” and logs of interaction), news articles, scientific articles, and online repositories of photos (FlickrR), videos (YouTube), and musical compositions (SoundCloud). Some (e.g., YouTube videos) arguably blur the boundary between “documents” and “interactions.” Often, accessing topic-specific examples of interest is a simple matter of conducting a search using some of the techniques already discussed in this chapter. A general search of World Wide Web content can easily be conducted using Google (e.g., google.co.uk, google.com). Google also allows a more restricted search that returns results within a particular category (e.g., images, blogs, maps, etc.). Searches within specific online services such as Facebook, YouTube, FlickrR, and the like can also be conducted to locate content-specific information of a particular type or format. The extent to which these various online resources may be useful as portals to online documents for use in qualitative research will depend on the research aims and topic, as well as on the nature of the resource and the data it offers. Twitter (twitter.com)—an online space where people can post small bursts of information called “tweets”—offers a fairly flexible advanced search mechanism (see twitter.com/search-advanced) that enables a search by words, phrases, locations, whether the tweet includes a smiley (taken to indicate a positive comment), and other options. I tested this search engine by submitting the input “David Cameron” under “words” and selecting “Positive :)” under the “other” category. A number of tweets were returned, ordered by the most relevant, the first of which stated “Dear Libya, please keep our Prime Minister David Cameron. We don't want him anymore, thanks! :)” Clearly not a positive comment in favor of Cameron, but it was included in the results because it contained a smiley symbol, as requested. This use of Twitter may perhaps be less appropriate to qualitative researchers due to the short-burst nature of the posts (tweets are limited to 140 characters maximum), which may not provide adequately rich, elaborate data for many qualitative research goals. Webpages, blogs, and other sources might be more useful.

Analysis of newspaper articles certainly seems to hold promise; the *Guardian* website (guardian.co.uk), for example, includes a search engine

that can locate topic-specific content—a search of this website using the same search term (“David Cameron :)”) returned several hits, the first of which was a blog titled “PMQs and gay marriage vote reaction: Politics live blog.” Many other online journalistic publications include similar search engine functions, as do most official websites. Future examples of qualitative document analysis in IMR using resources such as those outlined here, should help to clarify the value of the approach, as well as highlight caveats.

DESIGN ISSUES AND STRATEGIES

One design choice to consider in relation to online document analysis is whether to use existing, locatable sources, as such as those listed earlier, or solicit documents online. Both approaches have been used, with the former generally being most common and generally rather easier and less time-consuming to carry out. Basic online search techniques and also some of the search techniques considered earlier in discussing observational research (e.g., to locate blogs) can offer appropriate and effective ways to access existing potential data sources. Several researchers have used this approach; for example, Thoreau (2006) analyzed representations of disability by disabled people using articles (text and images) from *Ouch!*, a web-based magazine produced largely by and for disabled people. Thoreau reports being able to gather data that differed from that often available via traditional offline media sources (radio, press, television) in that it conveyed a strong sense of community and offered articles mostly involving personal narratives and accounts of personal experience. Heinz et al. (2002) also report a qualitative document analysis study in which they carried out a rhetorical-critical examination of texts and images on gay, lesbian, bisexual, and transgender (GLBT) websites. They argue that the World Wide Web provides a particularly important source of information on transformations in the cultural construction of GLBT identities, emphasizing the transnational nature of these online spaces, which enabled them to compare “shifting cultural identities” in a way that would have been much more difficult and time-consuming offline. This illustrates the potential benefit of IMR mentioned earlier—its ability to collapse geographical boundaries and enable cross-cultural research. Blogs may also serve as data sources for qualitative document analysis; Clarke and van Amerom (2008) present an example in which they analyzed the online blogs of men and women who self-identified

as depressed. Marcus et al. (2012) carried out a qualitative analysis of blogs of young adults with mental health concerns, arguing that IMR methods are useful for reaching this “underserved and under-treated” population. These examples demonstrate the scope for using text and images that are readily accessible online as sources of data for qualitative document analysis and also illustrate some of the potential advantages of the approach.

Soliciting documents directly from participants has been a less common strategy but may be useful in certain contexts, such as when target participant groups and/or topics are not so readily accessible via online archives. Hessler et al. (2003) used this approach to study risk behavior in adolescents by asking participants to submit daily diary entries via e-mail. These researchers report successfully generating rich narratives using this method and outline a number of potential benefits of the online methodology—including encouraging open and candid responses, overcoming difficulties in establishing rapport, and reducing costs compared with traditional FTF methods. A noteworthy caveat, however, concerns the lack of e-mail security, leading to potential threats to participant confidentiality and anonymity. Hessler et al. (2003) addressed this issue by using fake names and gaining signed offline consent from a responsible adult gatekeeper. Thus, given careful attention to appropriate ethics procedures, document solicitation in IMR may prove a viable approach with potential advantages over offline methods, particularly in contexts involving sensitive research with young people. As Hessler et al. note, e-mail can offer a “comforting air of informality” (p. 113), and the enhanced anonymity of the online setting may help to bridge the age gap that can often be a barrier in traditional FTF methods involving research with young people. Possibilities for incorporating multimedia into such approaches also emerge more readily online; for example, a researcher could set up a blog-type space for posting diary entries where audio, video, and images could be included. Setting security features so that the blog is private and only accessible to authorized, named individuals will almost certainly be necessary in such scenarios (and this is possible, using e.g., blogger.com). Inevitably, document solicitation approaches will be generally less time- and cost-efficient than searching for and locating existing archives on research-relevant topics (where these exist), but, as Hessler et al. (2003) have shown, they can still prove to be cost-effective when compared with traditional offline methods.

SAMPLING, RECRUITMENT, ETHICS

For approaches that involve searching and locating documents that already exist online, directly recruiting participants is not necessary. Search engines and dedicated searching tools for exploring the archives of repositories will prove most useful here. Schütz and Machilek (2003) offer a useful discussion of sampling for homepages, although their primary focus is on obtaining representative samples for quantitative research. They mention directories such as the Yahoo! directory of English language personal homepages (http://dir.yahoo.com/Society_and_Culture/People/), which can be used to conduct searches for topic-specific content (although when I carried out searches using this resource, most results returned were for commercial websites, not personal homepages). Here again, a Google search may offer a handy and effective solution, for example, when publicly available pages on a particular topic are sought. Essentially, sampling and recruitment options for document analysis online are much the same as those discussed earlier for observational research. Where documents are solicited directly, then approaches similar to those already outlined (e.g., recruiting via specialist newsgroups) will prove useful; similar issues to those already raised will thus require consideration, and similar principles of good practice will apply.

Regarding ethics issues, much the same considerations re-emerge as with observational research online: privacy, gaining (or waiving) informed consent, and protecting identities. Privacy is perhaps less of a concern here, however, as generally the types of documents discussed here are intentionally placed on the web in public spaces with the expectation that they will be readily accessible to and viewed by third parties. Indeed, often this is the purpose of creating a blog, webpage, and so on. This is rather different from the situation in which online discussion group spaces—whose primary function is to facilitate interaction and communication between individual group members—become also accessible to others (e.g., in stored discussion archives). Thus, there is arguably a clear distinction between accessing and analyzing (disseminating, publishing, etc.) material gathered from group discussion archives and accessing material in published online documents for use as research data. Clearly, sensitivity on the part of the researcher and respect for individuals and good ethics practice is required when assessing different data sources and research contexts and in making appropriate ethical choices.

An important further consideration in document analysis approaches concerns copyright: as well as assessing privacy expectations, the legality of using published material without attributing this to the author must be considered. Copyright of a personal webpage generally lies with the author, whereas content on a social network website is generally copyrighted by the site provider. The legal requirement to attribute copyrighted material to sources can create a tension with ethics principles, however, particularly those relating to confidentiality, anonymity, and protecting participants/individuals. Bowker and Tuffin (2004) and Reips and Buffardi (2012) have both highlighted this potential conflict, noting the tension between protecting authors' identity and adhering to copyright requirements to cite authors of works in online document analysis. Also relevant to this issue is the previously noted point that individuals/authors may not always be fully informed about and aware of the risks and potential harms involved in disclosure of their data and identities; to reiterate, wherever possible, a researcher is arguably obliged to raise such awareness where it may reasonably be thought lacking. Finally, reminding ourselves of the often ready traceability of online sources, including information about document authorship, and considering this alongside the enhanced opportunities for obtaining consent in document analysis approaches compared with observational contexts (e.g., where large discussion forum archives are used), it may be argued that informed consent should be sought and obtained in IMR document analysis. Grounds for not doing so when this is easy to achieve would, arguably, need to be justified. As always, such considerations, choices, and final decisions need to be weighed up against assessed levels of risk of harm (see Box 21.2).

Conclusion

This chapter has provided an overview of the scope for gathering qualitative research data online, outlined supporting tools and technologies, and reviewed key design choices and the issues and caveats that emerge. Interview and focus group approaches have probably been the most widely used to date, with observational and document analysis approaches being less common. Examples of successful attempts that have led to high-quality data and insightful, valuable conclusions have been provided. Less successful examples were also noted, and some of the factors that might contribute to these varying outcomes (e.g., successful

Box 21.2 Principles for good practice in qualitative internet-mediated observation and document analysis research.

- Keep in mind that different observation sites/sources may restrict or facilitate the design options available (e.g., using archived logs precludes participant approaches and makes disclosure/consent often implausible; observing real-time chat makes undisclosed, nonparticipant observation often untenable) (*ethics, validity*).
- Keep in mind the different types of dialogue and interaction that may be encouraged by synchronous (e.g., playful) and asynchronous (e.g., reflective) technologies when selecting which is most appropriate (*validity*).
- Carefully consider whether undisclosed approaches are ethically justifiable, keeping in mind the following key factors: *privacy expectations, sensitivity of data, levels of risk of harm, legal and copyright regulations, scientific value* (*ethics*).
- Keep in mind that trade-offs will often emerge, especially in relation to ethics procedures; for example, disclosure may increase the risk of reducing data authenticity and validity, but also reduce the risk of harming a group (*ethics, validity*).
- Keep in mind that it is often good practice to consult moderators before carrying out observation (e.g., of online discussion groups), particularly where disclosed approaches are proposed; however, moderators may also have agendas and opinions that could be prohibitive to the research and, in some cases, not making contact is arguably justified (*ethics, validity*).
- Remain mindful of the increased traceability of data sources online and the potential associated threats to anonymity/confidentiality, particularly in devising dissemination/publication strategies (*ethics*).
- Take steps to maximize data security, especially when utilizing less secure technologies such as e-mail (e.g., in soliciting documents) (*ethics*).

rapport-building strategies, use of synchronous vs. asynchronous technologies, etc.) were highlighted. Some ongoing controversies—particularly surrounding ethics and public–private spaces online—were also highlighted, and tentative approaches for thinking about and resolving associated dilemmas in order to make appropriate, ethically sound design choices were suggested. For example, considering individuals’ likely privacy expectations was proposed as an important factor in deciding whether gaining permission to use traces as observational data is necessary. Assessing the levels of potential risk of harm to individuals resulting from the research was also proposed as fundamentally important (just as in offline research), and a key tenet emphasizes that researchers should strive to implement robust procedures to minimize possible violations of key ethics principles (e.g., underage participants gaining access) as levels of risk increase. A key aim of the chapter was to offer researchers a set of guidelines and principles that can help inform design choices and help obtain high-quality, valuable data in an ethical manner: a summary of these principles is provided in Boxes 21.1 and 21.2. The examples presented throughout the chapter show a positive outlook for qualitative IMR; success stories are available across a range of approaches, methods,

and disciplines. A number of very appealing advantages of IMR approaches were identified, with supporting evidence from existing research examples to date: these include enhanced access to remote and specialist populations; enhanced scope for obtaining candid, rich data in certain contexts; facilitated access to topic-specific content and to larger sample sizes; and time and cost savings. Although ongoing debates and controversies are apparent, further examples of studies implementing the methods discussed here, as well as research on enduring issues (e.g., privacy expectations in various online contexts), should help inform these debates and hopefully lead toward resolution of some of these issues in the near future. In the meantime, the internet and both its technologies and social/cultural impact continue to undergo rapid development. Consequently, IMR methods continue to evolve and mutate at an impressive rate. Some reflections on the shape that future developments in qualitative IMR may take are offered next.

Future Directions

When considering future directions in qualitative IMR, three key questions can be posed: What might be the impact of emerging and new internet technologies? What might be the impact of changing

patterns of internet usage? Which methods are most likely to emerge as established approaches in social and behavioral research? These questions are intricately linked. Here, some speculations on likely future developments and answers to these questions are offered.

Emerging Internet Technologies

There is no doubt that internet technologies are developing at an impressive rate, with mobile applications, multimedia options, and integration and interaction with everyday objects all undergoing development. The “internet of things” is an intriguing development (e.g., see <http://www.independent.co.uk/news/business/feet-of-genius-show-were-all-connected-to-the-internet-of-things-7537450.html>, retrieved January 2013; Swan, 2012), referring to the connection of everyday objects (e.g., refrigerators, running shoes, pills) to the internet by microchips to enable the transfer of data and information. For example, running shoes that track performance and automatically send data to a computing device and can post a tweet reporting performance to a social networking site already exist. This may be considered a relatively low-tech example compared with developments such as “smart pills” containing a chip that, when ingested, can monitor data such as heart rate, body temperature, respiration, and so on and then send this information to an internet-connected device. Such examples (and other more mundane applications; e.g., thermostats to control central heating systems, automotive fuel consumption analyzers) illustrate the ways in which the internet appears set to become increasingly a part of everyday lives. Although it may be difficult to imagine at present how some of these innovations might hold scope for facilitating developments in social and behavioral qualitative IMR, internet technologies primarily concerned with social interaction and communication do seem set to develop in ways that will further expand the opportunities currently available. Internet-based audio/video communication technologies—such as Skype—were noted earlier as currently suffering from reliability and quality issues. However, there is little doubt that continuing increases in computing power and internet connection speeds (e.g., many providers are now offering fiber optic broadband) will improve the performance of these services dramatically, leading to online audio/visual interactions that may well come very close to mimicking offline FTF interactions. A number of authors have offered similarly

optimistic forecasts for internet-based multimedia communication technologies and their impact on social and behavioral IMR (Fielding & Macintyre, 2006; Gaiser, 2008; O’Connor et al., 2008). The benefits of such developments for online interview methods is clear and offer the potential to address issues related to the “impoverished” nature of online communications and lack of extralinguistic cues in research contexts where these seem especially important. The integration of such internet technologies with “internet-ready” televisions may expand these opportunities even further.

Another noteworthy development concerns the emergence and impact of mobile devices and technologies. Smartphones are now commonplace, allowing users to remain in contact with internet services “on the fly.” Keeping track of e-mails, instant messages, status updates, tweets, and so on is now readily achievable from practically any location where internet connectivity is available. Geolocation information can be tracked and posted (as when “checking in” on Facebook). The ubiquity of mobile internet devices is apparent while just sitting on a crowded train and observing how many people are engaged in interactions with their smartphones. It’s easy to imagine various ways to take advantage of this internet-connected presence of a large number of people in social and behavioral IMR. Regarding the types of qualitative methods and approaches discussed throughout this chapter, some possibilities include incorporating geolocation and multimedia information into document solicitation approaches (e.g., daily diaries) to enable ongoing recordings to take place continuously or at regular intervals; incorporating contextual information into observation and interview approaches, as by asking participants to record information at a range of locations and in a diversity of settings; or facilitating the collection of data that convey complex patterns by merging geolocation information, online interaction behaviors (posting tweets, updating a status, etc.), web-browsing activities (visiting topic-specific pages), and so on. The extent to which these possibilities may prove useful for qualitative IMR remains largely to be seen. The advantages of such approaches may include being able to incorporate rich, authentic context effects into the data and, through obtaining measurements “in situ,” needing to rely less on memory for past events, feelings, behaviors, and so on. The number of “apps” available for mobile smart devices is extensive (see <https://itunes.apple.com/gb/genre/ios/id36?mt=8>), and these offer great scope for developing interactive

data collection tools. Technical challenges aside (expertise or technical support will be needed to build an app), a daily diary app is conceivable, for example, that prompts users to enter information, perhaps interacting with geolocation information collected while the app is running and utilizing audio and video functionalities. Existing data collection strategies may be potentially adapted to and/or extended in an app format, such as the “emotion maps” discussed by Gabb (2009), which are used essentially to chart the patterning of affective behavior around the home.⁷ The advantages of apps over existing offline materials might include enhanced ease and convenience of use, greater flexibility (e.g., in adding spoken notes, etc.), and the ability to send data directly back to the researcher (or an online server) as it is submitted. Drawbacks might include reduced accessibility due to hardware and internet connectivity requirements. Some researchers have, in fact, already developed apps to collect data, although in a quantitative research context (e.g., Dufau et al., 2011). The scope of the approach for supporting qualitative research is potentially exciting but remains to be explored. Ethnographic research is perhaps a prime candidate for this type of approach: it is plausible that a researcher could become “immersed” (albeit nonproximally) in the daily activities of an individual or group by following them via a mobile device such as a smartphone. A range of possible benefits of such an approach are imaginable.

Shifting Patterns of Internet Usage

As the internet and its technologies become more a part of people’s everyday lives, so will people’s competencies, expectations, and patterns of usage evolve and shift. Such shifts will be very important in influencing the scope and potential of social and behavioral IMR methods, including qualitative approaches. For example, as online audio/video communication technologies (such as Skype) become more reliable and able to more closely approximate actual FTF interactions, their usage is likely to catch up with, if not exceed, traditional telephone services, which are generally rather more expensive (particularly for international calls) and do not offer the appeal of incorporating rich visual cues. Such developments in usage patterns will impact positively on previously mentioned issues related to accessibility in qualitative IMR methods, particularly in online interview contexts. Skype is already used by many to keep in touch with overseas friends and family. The impact on qualitative

interview research of such technologies becoming more ubiquitous may well be that online interviews will be as well used as, or even more widely used than, telephone interviews or even become the “gold standard” (the latter suggestion is highly tentative!). The extent to which such speculations may be borne out in practice will depend on a number of factors, but certainly such possibilities are far from inconceivable.

Aside from these developments in the use of multimedia applications and their obvious potential implications for interview-based IMR, the expanding presence of the internet in people’s daily lives more generally has further implications for qualitative IMR approaches. Returning to the point that the best strategy may not always be to mimic offline methods and settings as closely as possible (e.g., FTF interview methods), as people become more proficient and engaged with other forms of online communication, such as text-only based interactions, the value, appeal, and appropriateness of these technologies for supporting qualitative IMR may very well expand. Thus, the proliferation of online discussion spaces (newsgroups, web forums, mailing lists, etc.) over recent years has been revolutionary; discussion spaces and online social network groups covering a vast array of functions and topics now exist. As these technologies and services continue to permeate and proliferate, cultural shifts in the way they and the social structures they support are perceived (including their relationship with offline forms of interaction) are likely to ensue. It is not difficult to imagine that such online forms of communication may eventually become widely accepted as routine and natural ways of establishing, developing, and maintaining social relationships with both individuals and groups (in addition to existing offline channels). Such developments would have implications for the range of qualitative IMR methods discussed in this chapter by making these online spaces even more likely to provide a suitable means of access to groups of interest who are proficient, experienced, and confident in using the communication channels they support. Indeed, the recent emergence of Web 2.0 and the widespread popularity and uptake of the services and technologies it offers (particularly for social networking activities) is a reflection that this shift has already begun. Although a common presumption is that social networking spaces and activities are (presently) largely the domain of young, tech-savvy users who interact on Facebook, MySpace, and Friendster, a perusal of the range of online groups and networks that exist indicates

that social network users span a broad demographic spectrum. “Gransnet,” for example (an offshoot of the larger “Mumsnet” group), is an online discussion forum that is highly active, well-established, and can often involve intense, highly emotionally charged exchanges between members; indeed, the compelling nature of immersion within this online group has been evidenced by individual concerns about being addicted to the site (personal communication with active forum member).

In summary, as a growing proportion of the population become increasingly adept at using these online forms of interaction, researchers may need to reassess claims that the online medium is less natural, rich, and fluent than offline FTF interactions.

Enduring Qualitative IMR Methods

Throughout this chapter, successful examples of a range of qualitative IMR studies have been presented, showing that each of the key methods discussed—interviews, focus groups, observation, and document analysis—holds promise. Some approaches have seemed on the whole less successful than others, such as synchronous online interview approaches (compared with asynchronous approaches). However, future anticipated technological developments may well help to overcome many of the issues that have emerged, offering the potential for ongoing developments and improvements in the techniques and procedures that have been possible to date. It is suggested here that online interview methods and opportunities are likely to expand and diversify, taking into account the considerations discussed earlier; in particular, the scope for gaining access to participants who would otherwise be unreachable (offline) is likely to remain highly attractive to researchers. Observation and document analysis approaches show every indication of following suit, with the examples available to date indicating a range of benefits and demonstrated successful outcomes. The prospect of carrying out observational research in VREs has only just started to be explored in qualitative IMR, but may well expand further. Using VREs as a space in which to conduct virtual FTF interviews or focus groups has also been noted as a possibility (Gaiser, 2008; Vehovar & Manfreda, 2008). Indeed, Stewart and Williams (2005) report on a study that piloted this approach by conducting a focus group within a VRE, commenting that “Some of the concerns that have plagued internet research, such as the lack of proxemical (use of space) and kinesical

(body movement) features that aid in interpretation and analysis in the offline setting, are now being nullified by these new and emerging ‘physical’ online environments” (p. 407). Combining such approaches (observations, interviews, etc.) may further support ethnographic research in virtual worlds: for an existing discussion of the scope of the approach, see Boellstorff (2010). Further implementations across an array of research domains will be informative in elucidating the potential of IMR approaches using VREs.

One possible barrier to online observational IMR approaches, in particular, concerns the ongoing debates surrounding the online public–private domain distinction and when it is or isn’t appropriate to engage in observational research without disclosure and/or consent. The issue has been contentious, and it remains to be seen how discussions will pan out and whether any clearer resolution and agreed set of standards and guidelines will emerge. Further research into participants’ expectations, as well as possible and actual consequences of conducting undisclosed observations, will help inform these debates. Other possible barriers to IMR include the reduced scope for engaging in highly sensitive research with young populations (under age 16), which remains problematic at present but may find future resolutions in emerging technologies and evolving strategies. On the positive side, a number of very appealing benefits are apparent—to recap, these center around enhanced access to a diverse range of potential participants, including hard-to-access groups; cost and time savings; and possible benefits due to reduced biosocial cues and enhanced levels of anonymity. All in all, the conclusion here is that the future for qualitative IMR seems very promising.

Notes

1. First launched in 1991 by its inventor Tim Berners-Lee (Zakon, 2000).
2. Although it is recognized that in qualitative research, more so than with quantitative approaches, the various research stages are more likely to interact and deviate from a rigidly structured sequence.
3. As noted by Hewson (2007), the meaning of this term can vary depending on context; here, it is used to mean roughly the coherence and meaningful links between themes and topics in a dialogue.
4. I am unaware of any research attempting to assess the validity of each of these hypotheses.
5. It is not entirely clear whether the discussion board posts used were retrieved from stored archives or collected as they appeared on the boards during the discussion period. However, analysis seems to have occurred once all relevant

posts making up the dataset had been collated and saved by the researchers.

6. Perhaps the closest offline equivalents may involve using large-scale corpora, such as the British National Corpus (BNC) (see <http://www.natcorp.ox.ac.uk/>), a large, searchable database of written documents and spoken language sources including some formal and informal conversations. Such traditional corpora derived from offline sources are unlikely to offer the breadth, diversity, and scope of online traces of human interactions, however.
7. These emotion maps essentially ask participants to place stickers of facial expressions on to a floor plan representing the layout of their home; this is a technique that has been used to supplement other qualitative data-gathering methods, such as interviews, observation, and solicitation of diaries.

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Multi-Method, Mixed-
Method and Participatory
Designs



Case Study Research: In-Depth Understanding in Context

Helen Simons

Abstract

This chapter explores case study as a major approach to research and evaluation. After first noting various contexts in which case studies are commonly used, the chapter focuses on case study research directly. Strengths and potential problematic issues are outlined and then key phases of the process. The chapter emphasizes how important it is to design the case, to collect and interpret data in ways that highlight the qualitative, to have an ethical practice that values multiple perspectives and political interests, and to report creatively to facilitate use in policy making and practice. Finally, it explores how to generalize from the single case. Concluding questions center on the need to think more imaginatively about design and the range of methods and forms of reporting required to persuade audiences to value qualitative ways of knowing in case study research.

Key Words: case study, singular, contexts, interpretation, qualitative, generalization, particularization, artistic, creative methods

Introduction

This chapter explores case study as a major approach to research and evaluation using primarily qualitative methods, as well as documentary sources, contemporaneous or historical. However, this is not the only way in which case study can be conceived. No one has a monopoly on the term. While sharing a focus on the singular in a particular context, case study has a wide variety of uses, not all associated with research. A case study, in common parlance, documents a particular situation or event in detail in a specific sociopolitical context. The particular can be a person, a classroom, an institution, a program, or a policy. Below I identify different ways in which case study is used before focusing on qualitative case study research in particular. However, first I wish to indicate how I came to advocate and practice this form of research. Origins, context, and opportunity often shape the research processes we

endorse. It is helpful for the reader, I think, to know how I came to the perspective I hold.

The Beginnings

I first came to appreciate and enjoy the virtues of case study research when I entered the field of curriculum evaluation and research in the 1970s. The dominant research paradigm for educational research at that time was experimental or quasi-experimental, cost-benefit, or systems analysis, and the dominant curriculum model was aims and objectives (House, 1993). The field was dominated, in effect, by a psychometric view of research in which quantitative methods were preeminent. But the innovative projects we were asked to evaluate (predominantly, but not exclusively, in the humanities) were not amenable to such methodologies. The projects were challenging to the status quo of institutions, involved people interpreting the

policy and programs, were implemented differently in different contexts and regions, and had many unexpected effects.

We had no choice but to seek other ways to evaluate these complex programs, and case study was the methodology we found ourselves exploring, in order to understand how the projects were being implemented, why they had positive effects in some regions of the country and not others, and what the outcomes meant in different sociopolitical and cultural contexts. What better way to do this than to talk with people to see how they interpreted the “new” curriculum; to watch how teachers and students put it into practice; to document transactions, outcomes, and unexpected consequences; and to interpret all in the specific context of the case (Simons, 1971, 1987, pp. 55–89). From this point on and in further studies, case study in educational research and evaluation came to be a major methodology for understanding complex educational and social programs. It also extended to other practice professions, such as nursing, health, and social care (Zucker, 2001; Greenhalgh & Worrall, 1997; Shaw & Gould, 2001). For further details of the evolution of the case study approach and qualitative methodologies in evaluation, see House, 1993, pp. 2–3; Greene, 2000; Simons, 2009, pp. 14–18; Simons & McCormack, 2007, pp. 292–311).

This was not exactly the beginning of case study, of course. It has a long history in many disciplines (Simons, 1980; Ragin, 1992; Gomm, Hammersley, & Foster, 2004; Platt, 2007), many aspects of which form part of case study practice to this day. But its evolution in the context just described was a major move in the contemporary evolution of the logic of evaluative inquiry (House, 1980). It also coincided with movement toward the qualitative in other disciplines, such as sociology and psychology. This was all part of what Denzin & Lincoln (1994) termed “a quiet methodological revolution” (p. ix) in qualitative inquiry that had been evolving over the course of forty years.

There is a further reason why I continue to advocate and practice case study research and evaluation to this day and that is my personal predilection for trying to understand and represent complexity, for puzzling through the ambiguities that exist in many contexts and programs and for presenting and negotiating different values and interests in fair and just ways.

Put more simply, I like interacting with people, listening to their stories, trials and

tribulations—giving them a voice in understanding the contexts and projects with which they are involved, and finding ways to share these with a range of audiences. In other words, the move toward case study methodology described here suited my preference for how I learn.

Concepts and Purposes of Case Study

Before exploring case study as it has come to be established in educational research and evaluation over the past forty years, I wish to acknowledge other uses of case study. More often than not, these relate to purpose, and appropriately so in their different contexts, but many do not have a research intention. For a study to count as research, it would need to be a systematic investigation generating evidence that leads to “new” knowledge that is made public and open to scrutiny. There are many ways to conduct research stemming from different traditions and disciplines, but they all, in different ways, involve these characteristics.

Everyday Usage: Stories We Tell

The most common of these uses of case study is the everyday reference to a person, an anecdote or story illustrative of a particular incident, event, or experience of that person. It is often a short, reported account commonly seen in journalism but also in books exploring a phenomenon, such as recovery from serious accidents or tragedies, where the author chooses to illustrate the story or argument with a “lived” example. This is sometimes written by the author and sometimes by the person whose tale it is. “Let me share with you a story,” is a phrase frequently heard

The spirit behind this common usage and its power to connect can be seen in a report by Tim Adams of the London Olympics opening ceremony’s dramatization by Danny Boyle.

It was the point when we suddenly collectively wised up to the idea that what we are about to receive over the next two weeks was not only about “legacy collateral” and “targeted deliverables,” not about G4S failings and traffic lanes and branding opportunities, but about the second-by-second possibilities of human endeavour and spirit and communality, enacted in multiple places and all at the same time. Stories in other words. (Adams, 2012)

This was a collective story, of course, not an individual one, but it does convey some of the major characteristics of case study—that richness

of detail, time, place, multiple happenings and experiences—that are also manifest in case study research, although carefully evidenced in the latter instance. We can see from this common usage how people have come to associate case study with story. I return to this thread in the reporting section.

Professions Individual Cases

In professional settings, in health and social care, case studies, often called *case histories*, are used to accurately record a person's health or social care history and his or her current symptoms, experience, and treatment. These case histories include facts but also judgments and observations about the person's reaction to situations or medication. Usually these are confidential. Not dissimilar is the detailed documentation of a case in law, often termed a *case precedent* when referred to in a court case to support an argument being made. However in law there is a difference in that such case precedents are publicly documented.

Case Studies in Teaching

Exemplars of practice

In education, but also in health and social care training contexts, case studies have long been used as exemplars of practice. These are brief descriptions with some detail of a person or project's experience in an area of practice. Though frequently reported accounts, they are based on a person's experience and sometimes on previous research.

Case scenarios

Management studies are a further context in which case studies are often used. Here, the case is more like a scenario outlining a particular problem situation for the management student to resolve. These scenarios may be based on research but frequently are hypothetical situations used to raise issues for discussion and resolution. What distinguishes these case scenarios and the case exemplars in education from case study research is the intention to use them for teaching purposes.

Country Case Studies

Then there are case studies of programs, projects, and even countries, as in international development, where a whole-country study might be termed a case study or, in the context of the Organization for Economic Co-operation and Development (OECD), where an exploration is conducted of the state of the art of a subject, such as education or environmental science in one or

several countries. This may be a contemporaneous study and/or what transpired in a program over a period of time. Such studies often do have a research base but frequently are reported accounts that do not detail the design, methodology, and analysis of the case, as a research case study would do, or report in ways that give readers a vicarious experience of what it was like to be there. Such case studies tend to be more knowledge and information-focused than experiential.

Case Study as History

Closer to a research context is case study as history—what transpired at a certain time in a certain place. This is likely to be supported by documentary evidence but not primary data gathering unless it is an oral history. In education, in the late 1970s, Stenhouse (1978) experimented with a case study archive. Using contemporaneous data gathering, primarily through interviewing, he envisaged this database, which he termed a “case record,” forming an archive from which different individuals, at some later date, could write a “case study.” This approach uses case study as a documentary source to begin to generate a history of education, as the subtitle of Stenhouse's 1978 paper indicates “Towards a contemporary history of education.”

Case Study Research

From here on, my focus is on case study research per se, adopting for this purpose the following definition:

Case study is an in-depth exploration from multiple perspectives of the complexity and uniqueness of a particular project, policy, institution or system in a “real-life” context. It is research based, inclusive of different methods and is evidence-led. (Simons, 2009, p. 21).

For further related definitions of case study, see Stake (1995), Merriam (1998), and Chadderton & Torrance (2011). And for definitions from a slightly different perspective, see Yin (2004) and Thomas (2011a).

Not Defined by Method or Perspective

The inclusion of different methods in the definition quoted above definition signals that case study research is not defined by methodology or method. What defines case study is its singularity and the concept and boundary of the case. It is theoretically possible to conduct a case study using primarily quantitative data if this is the best way of providing evidence to inform the issues the case is exploring.

It is equally possible to conduct case study that is mainly qualitative, to engage people with the experience of the case or to provide a rich portrayal of an event, project, or program.

Or one can design the case using mixed methods. This increases the options for learning from different ways of knowing and is sometimes preferred by stakeholders who believe it provides a firmer basis for informing policy. This is not necessarily the case but is beyond the scope of this chapter to explore. For further discussion of the complexities of mixing methods and the virtue of using qualitative methods and case study in a mixed method design, see Greene (2007).

Case study research may also be conducted from different standpoints—realist, interpretivist, or constructivist, for example. My perspective falls within a constructivist, interpretivist framework. What interests me is how I and those in the case perceive and interpret what we find and how we construct or co-construct understandings of the case. This not only suits my predilection for how I see the world, but also my preferred phenomenological approach to interviewing and curiosity about people and how they act in social and professional life.

Qualitative Case Study Research

Qualitative case study research shares many characteristics with other forms of qualitative research, such as narrative, oral history, life history, ethnography, in-depth interview, and observational studies that utilize qualitative methods. However, its focus, purpose, and origins, in educational research at least, are a little different.

The focus is clearly the study of the singular. The purpose is to portray an in-depth view of the quality and complexity of social/educational programs or policies as they are implemented in specific socio-political contexts. What makes it qualitative is its emphasis on subjective ways of knowing, particularly the experiential, practical, and presentational rather than the propositional (Heron, 1992, 1999) to comprehend and communicate what transpired in the case.

Characteristic Features and Advantages

Case study research is not method dependent, as noted earlier, nor is it constrained by resources or time. Although it can be conducted over several years, which provides an opportunity to explore the process of change and explain how and why things happened, it can equally be carried out

contemporaneously in a few days, weeks, or months. This flexibility is extremely useful in many contexts, particularly when a change in policy or unforeseen issues in the field require modifying the design.

Flexibility extends to reporting. The case can be written up in different lengths and forms to meet different audience needs and to maximize use (see the section on Reporting). Using the natural language of participants and familiar methods (like interview, observation, oral history) also enables participants to engage in the research process, thereby contributing significantly to the generation of knowledge of the case. As I have indicated elsewhere (Simons, 2009), “This is both a political and epistemological point. It signals a potential shift in the power base of who controls knowledge and recognizes the importance of co-constructing perceived reality through the relationships and joint understandings we create in the field” (p. 23).

Possible Disadvantages

If one is an advocate, identifying advantages of a research approach is easier than pointing out its disadvantages, something detractors are quite keen to do anyway! But no approach is perfect, and here are some of the issues that often trouble people about case study research. The “sample of one” is an obvious issue that worries those convinced that only large samples can constitute valid research and especially if this is to inform policy. Understanding complexity in depth may not be a sufficient counterargument, and I suspect there is little point in trying to persuade otherwise. For frequently, this perception is one of epistemological and methodological, if not ideological, preference.

However, there are some genuine concerns that many case researchers face: the difficulty of processing a mass of data; of “telling the truth” in contexts where people may be identifiable; personal involvement, when the researcher is the main instrument of data gathering; and writing reports that are data-based, yet readable in style and length. But one issue that concerns advocates and nonadvocates alike is how inferences are drawn from the single case.

Answers to some of these issues are covered in the sections that follow. Whether they convince may again be a question of preference. However, it is worth noting here that I do not think we should seek to justify these concerns in terms identified by other methodologies. Many of them are intrinsic to the nature and *strength* of qualitative case study research.

Subjectivity, for instance, both of participants and researcher is inevitable, as it is in many other

qualitative methodologies. This is often the basis on which we act. Rather than see this as bias or something to counter, it is an intelligence that is essential to understanding and interpreting the experience of participants and stakeholders. Such subjectivity needs to be disciplined, of course, through procedures that examine both the validity of individuals' representations of "their truth", and demonstrate how the researcher took a reflexive approach to monitoring how his or her own values and predilections may have unduly influenced the data.

Types of Case Study

There are numerous types of case study, too many to categorize, I think, as there are overlaps between them. However, attempts have been made to do this and, for those who value typologies, I refer them to Bassey (1999) and, for a more extended typology, to Thomas (2011b). A slightly different approach is taken by Gomm, Hammersley, and Foster (2004) in annotating the different emphases in major texts on case study. What I prefer to do here is to highlight a few familiar types to focus the discussion that follows on the practice of case study research.

Stake (1995) offers a threefold distinction that is helpful when it comes to practice, he says, because it influences the methods we choose to gather data (p. 4). He distinguishes between an *intrinsic case study*, one that is studied to learn about the particular case itself and an *instrumental case study*, in which we choose a case to gain insight into a particular issue (i.e., the case is instrumental to understanding something else; p. 3). The *collective case study* is what its name suggests: an extension of the instrumental to several cases.

Theory-led or *theory-generated case study* is similarly self-explanatory, the first starting from a specific theory that is tested through the case; the second constructing a theory through interpretation of data generated in the case. In other words, one ends rather than begins with a theory. In qualitative case study research, this is the more familiar route. The theory of the case becomes the argument or story you will tell.

Evaluation case study requires a slightly longer description as this is my context of practice, one which has influenced the way I conduct case study and what I choose to emphasize in this chapter. An evaluation case study has three essential features: to determine the *value* of the case, to include and balance different interests and values, and to report findings to a range of stakeholders in ways that they can use. The reasons for this may be found in the

interlude that follows, which offers a brief characterization of the social and ethical practice of evaluation and why qualitative methods are so important in this practice.

Interlude: Social and Ethical Practice of Evaluation

Evaluation is a social practice that documents, portrays, and seeks to understand the *value* of a particular project, program, or policy. This can be determined by different evaluation methodologies, of course. But the value of qualitative case study is that it is possible to discern this value without decontextualizing the data. While the focus of the case is usually a project, program, policy, or some unit within, studies of key individuals, what I term *case profiles*, may be embedded within the overall case. In some instances, these profiles, or even shorter cameos of individuals, may be quite prominent. For it is through the perceptions, interpretations, and interactions of people that we learn how policies and programs are enacted (Kushner, 2000, p. 12). The program is still the main focus of analysis, but, in exploring how individuals play out their different roles in the program, we get closer to the actual experience and meaning of the program in practice.

Case study evaluation is often commissioned from an external source (government department or other agency) keen to know the worth of publicly funded programs and policies to inform future decision making. It needs to be responsive to issues or questions identified by stakeholders, who often have different values and interests in the expected outcomes and appreciate different perspectives of the program in action. The context also is often highly politicized, and interests can conflict. The task of the evaluator in such situations becomes one of including and balancing all interests and values in the program fairly and justly.

This is an inherently political process and requires an ethical practice that offers participants some protection over the personal data they give as part of the research and agreed audiences access to the findings, presented in ways they can understand. Negotiating what information becomes public can be quite difficult in singular settings where people are identifiable and intricate or problematic transactions have been documented. The consequences that ensue from making knowledge public that hitherto was private may be considerable for those in the case. It may also be difficult to portray some of the contextual detail that would enhance understanding for readers.

The ethical stance that underpins the case study research and evaluation I conduct stems from a theory of ethics that emphasizes the centrality of relationships in the specific context and the consequences for individuals, while remaining aware of the research imperative to publicly report. It is essentially an independent democratic process based on the concepts of fairness and justice, in which confidentiality, negotiation, and accessibility are key principles (MacDonald, 1976; Simons, 2009, pp. 96–111; and Simons 2010). The principles are translated into specific procedures to guide the collection, validation, and dissemination of data in the field. These include:

- engaging participants and stakeholders in identifying issues to explore and sometimes also in interpreting the data;
- documenting how different people interpret and value the program;
- negotiating what data becomes public respecting both the individual's "right to privacy" and the public's "right to know";
- offering participants opportunities to check how their data are used in the context of reporting;
- reporting in language and forms accessible to a wide range of audiences;
- disseminating to audiences within and beyond the case.

For further discussion of the ethics of democratic case study evaluation and examples of their use in practice, see Simons (2000, 2006, 2009, chapter 6, 2010).

Designing Case Study Research

Design issues in case study sometimes take second place to those of data gathering, the more exciting task perhaps in starting research. However, it is critical to consider the design at the outset, even if changes are required in practice due to the reality of what is encountered in the field. In this sense, the design of case study is emergent, rather than pre-ordinate, shaped and reshaped as understanding of the significance of foreshadowed issues emerges and more are discovered.

Before entering the field, there are a myriad of planning issues to think about related to stakeholders, participants, and audiences. These include whose values matter, whether to engage them in data gathering and interpretation, the style of reporting appropriate for each, and the ethical guidelines that will underpin data collection and reporting. However, here I emphasize only three: the broad

focus of the study, what the case is a case of, and framing questions/issues. These are steps often ignored in an enthusiasm to gather data, resulting in a case study that claims to be research but lacks the basic principles required for generation of valid, public knowledge.

Conceptualize the Topic

First, it is important that the topic of the research is conceptualized in a way that it can be researched (i.e., it is not too wide). This seems an obvious point to make, but failure to think through precisely what it is about your research topic you wish to investigate will have a knock-on effect on the framing of the case, data gathering, and interpretation and may lead, in some instances, to not gathering or analyzing data that actually informs the topic. Further conceptualization or reconceptualization may be necessary as the study proceeds, but it is critical to have a clear focus at the outset.

What Constitutes the Case

Second, I think it is important to decide what would constitute the case (i.e., what it is a case of) and where the boundaries of this lie. This often proves more difficult than first appears. And sometimes, partly because of the semifluid nature of the way the case evolves, it is only possible to finally establish what the case is a case of at the end. Nevertheless, it is useful to identify what the case and its boundaries are at the outset to help focus data collection while maintaining an awareness that these may shift. This is emergent design in action.

In deciding the boundary of the case, there are several factors to bear in mind. Is it bounded by an institution or a unit within an institution, by people within an institution, by region, or by project, program or policy? If we take a school as an example, the case could be comprised of the principal, teachers, and students, or the boundary could be extended to the cleaners, the caretaker, the receptionist, people who often know a great deal about the subnorms and culture of the institution.

If the case is a policy or particular parameter of a policy, the considerations may be slightly different. People will still be paramount—those who generated the policy and those who implemented it—but there is likely also to be a political culture surrounding the policy that had an influence on the way the policy evolved. Would this be part of the case?

Whatever boundary is chosen, this may change in the course of conducting the study when issues arise that can only be understood by going to another level. What transpires in a classroom, for example, if this is the case, is often partly dependent on the support of the school leadership and culture of the institution and this, in turn, to some extent is dependent on what resources are allocated from the local education administration. Much like a series of Russian dolls, one context inside the other.

Unit of analysis

Thinking about what would constitute the unit of analysis—a classroom, an institution, a program, a region—may help in setting the boundaries of the case, and it will certainly help when it comes to analysis. But this is a slightly different issue from deciding what the case is a case of. Taking a health example, the case may be palliative care support, but the unit of analysis the palliative care ward or wards. If you took the palliative care ward as the unit of analysis this would be as much about how palliative care was exercised in this or that ward than issues about palliative care support in general. In other words, you would need to have specific information and context about how this ward was structured and managed to understand how palliative care was conducted in this *particular* ward. Here, as in the school example above, you would need to consider which of the many people who populate the ward form part of the case—nurses, interns, or doctors only, or does it extend to patients, cleaners, nurse aides, and medical students?

Framing Questions and Issues

The third most important consideration is how to frame the study, and you are likely to do this once you have selected the site or sites for study. There are at least four approaches. You could start with precise questions, foreshadowed issues (Smith & Pohland, 1974), theories, or a program logic. To some extent, your choice will be dictated by the type of case you have chosen, but also by your personal preference for how to conduct it—in either a structured or open way.

Initial questions give structure; foreshadowed issues more freedom to explore. In qualitative case study, foreshadowed issues are more common, allowing scope for issues to change as the study evolves, guided by participants' perspectives and events in the field. With this perspective, it is more likely that you will generate a theory of the case toward the end, through your interpretation and analysis.

If you are conducting an *instrumental case study*, staying close to the questions or foreshadowed issues is necessary to be sure you gain data that will illuminate the central focus of the study. This is critical if you are exploring issues across several cases, although it is possible to do a cross-case analysis from cases that have each followed a different route to discovering significant issues.

Opting to start with a theoretical framework provides a basis for formulating questions and issues, but it can also constrain the study to only those questions/issues that fit the framework. The same is true with using program logic to frame the case. This is an approach frequently adopted in evaluation case study where the evaluator, individually or with stakeholders, examines how the aims and objectives of the program relate to the activities designed to promote it and the outcomes and impacts expected. It provides direction, although it can lead to simply confirming what was anticipated, rather than documenting what transpired in the case.

Whichever approach you choose to frame the case, it is useful to think about the rationale or theory for each question and what methods would best enable you to gain an understanding of them. This will not only start a reflexive process of examining your choices—an important aspect of the process of data gathering and interpretation—it will also aid analysis and interpretation further down the track.

Methodology and Methods

Qualitative case study research, as already noted, appeals to subjective ways of knowing and to a primarily qualitative methodology, that captures experiential understanding (Stake, 2010, pp. 56–70). It follows that the main methods of data gathering to access this way of knowing will be qualitative. Interviewing, observation, and document analysis are the primary three, often supported by critical incidents, focus groups, cameos, vignettes, diaries/journals, and photographs. Before gathering any primary data, however, it is useful to search relevant existing sources (written or visual) to learn about the antecedents and context of a project, program, or policy as a backdrop to the case. This can sharpen framing questions, avoid unnecessary data gathering, and shorten the time needed in the field.

Given that there are excellent texts on qualitative methods (see, for example, Denzin & Lincoln, 1994; Seale, 1999; Silverman, 2000, 2004), I will not discuss all potential relevant methods here, but simply focus on the *qualities* of the primary methods that are particularly appropriate for case study research.

Primary Qualitative Data Gathering Methods

Interviewing

The most effective style of interviewing in qualitative case study research to gain in-depth data, document multiple perspectives and experiences and explore contested issues is the unstructured interview, active listening and open questioning are paramount, whatever prequestions or foreshadowed issues have been identified. This can include photographs—a useful starting point with certain cultural groups and the less articulate, to encourage them to tell their story through connecting or identifying with something in the image.

The flexibility of unstructured interviewing has three further advantages for understanding participants' experiences. First, through questioning, probing, listening, and, above all, paying attention to the silences and what they mean, you can get closer to the meaning of participants' experiences. It is not always what they say.

Second, unstructured interviewing is useful for engaging participants in the process of research. Instead of starting with questions and issues, invite participants to tell their stories or reflect on specific issues, to conduct their own self-evaluative interview, in fact. Not only will they contribute their particular perspective to the case, they will also learn about themselves, thereby making the process of research educative for them as well as for the audiences of the research.

Third, the open-endedness of this style of interviewing has the potential for creating a dialogue between participants and the researcher and between the researcher and the public, if enough of the dialogue is retained in the publication (Bellah, Madsen, Sullivan, Swidler, & Tipton, 1985).

Observations

Observations in case study research are likely to be close-up descriptions of events, activities, and incidents that detail what happens in a particular context. They will record time, place, specific incidents, transactions, and dialogue, and note characteristics of the setting and of people in it without preconceived categories or judgment. No description is devoid of some judgment in selection, of course, but, on the whole, the intent is to describe the scene or event “as it is,” providing a rich, textured description to give readers a sense of what it was like to be there or provide a basis for later interpretation.

Take the following excerpt from a study of the West Bromwich Operatic Society. It is the first night of a new production, *The Producers*, by this

amateur operatic society. This brief excerpt is from a much longer observation of the overture to the first evening's performance, detailing exactly what the production is, where it is, and why there is such a tremendous sense of atmosphere and expectation surrounding the event. Space prevents including the whole observation, but I hope you can get a glimmer of the passion and excitement that precedes the performance:

Birmingham, late November, 2011, early evening... Bars and restaurants spruce up for the evening's trade. There is a chill in the air but the party season is just starting...

A few hundred yards away, past streaming traffic on Suffolk Street, Queensway, an audience is gathering at the New Alexandra Theatre. The foyer windows shine in the orange sodium night. Above each one is the rubric: WORLD CLASS THEATRE.

Inside the preparatory rituals are being observed; sweets chosen, interval drinks ordered and programmes bought. People swap news and titbits about the production...

The bubble of anticipation grows as the 5-minute warning sounds. People make their way to the auditorium.

There have been so many nights like this in the past 110 years since a man named William Coutts invested £10,000 to build this palace of dreams... So many fantasies have been played under this arch: melodramas and pantomimes, musicals and variety... So many audiences, settling down in their tip-up seats, wanting to be transported away from work, from ordinariness and private troubles... The dimming lights act like a mother's hush. You could touch the silence. *Boinnng!* A spongy thump on a bass drum, and the horns pipe up that catchy, irrepressible, tasteless tune and already you're singing under your breath, 'Springtime for Hitler and Germany...'

The orchestra is out of sight in the pit. There's just the velvet curtain to watch as your fingers tap along. What's waiting behind?

Then it starts it to move.

Opening night...

It's opening night! (Matarasso, 2012, pp. 1–2)

For another and different example—a narrative observation of an everyday but unique incident that details date, time, place, and experience—see Simons (2009, p. 60).

Such naturalistic observations are also useful in contexts where we cannot understand what is going

on through interviewing alone—in cultures with which we are less familiar or where key actors may not share our language or have difficulty expressing it. Careful description in these situations can help identify key issues, discover the norms and values that exist in the culture, and, if sufficiently detailed, allow others to cross corroborate what significance we draw from these observations. This last point is very important to avoid the danger in observation of ascribing motivations to people and meanings to transactions.

Finally, naturalistic observations are very important in highly politicized environments, often the case in commissioned evaluation case study, where individuals in interview may try to elude the “truth” or press on you that their view is the “right” view of the situation. In these contexts, naturalistic observations not only enable you to document interactions as you perceive them, but they also provide a cross-check on the veracity of information obtained in interviews.

Document analysis

Analysis of documents, as already intimated, is useful for establishing what historical antecedents might exist to provide a springboard for contemporaneous data gathering. In most cases, existing documents are also extremely pertinent for understanding the policy context.

In a national policy case study I conducted on a major curriculum change, the importance of preexisting documentation was brought home to me sharply when certain documentation initially proved elusive to obtain. It was difficult to believe that it did not exist, as the evolution of the innovation involved several parties who had not worked together before. There was bound, I thought, to be minuted meetings sharing progress and documentation of the “new” curriculum. In the absence of some crucial documents, I began to piece together the story through interviewing. Only there were gaps, and certain issues did not make sense.

It was only when I presented two versions of what I discerned had transpired in the development of this initiative in an interim report eighteen months into the study that things started to change. Subsequent to the meeting at which the report was presented, the “missing” documents started to appear. Suddenly found. What lay behind the “missing documents,” something I suspected from what certain individuals did and did not say in interview, was a major difference of view about how the innovation evolved, who was key in the process,

and whose voice was more important in the context. Political differences, in other words, that some stakeholders were trying to keep from me. The emergence of the documents enabled me to finally produce an accurate and fair account.

This is an example of the importance of having access to all relevant documents relating to a program or policy in order to study it fairly. The other major way in which document analysis is useful in case study is for understanding the values, explicit and hidden, in policy and program documents and in the organization where the program or policy is implemented. Not to be ignored as documents are photographs, and these, too, can form the basis of a cultural and value analysis of an organization (Prosser, 2000).

Creative artistic approaches

Increasingly, some case study researchers are employing creative approaches associated with the arts as a means of data gathering and analysis. Artistic approaches have often been used in representing findings, but less frequently in data gathering and interpretation (Simons & McCormack, 2007). A major exception is the work of Richardson (1994), who sees the very process of writing as an interpretative act, and of Cancienne and Snowber (2003), who argue for movement as method.

The most familiar of these creative and artistic forms are written—narratives and short stories (Clandinin & Connelly, 2000; Richardson, 1994; Sparkes, 2002), poems or poetic form (Butler-Kisber, 2010; Duke, 2007; Richardson, 1997; Sparkes & Douglas, 2007), cameos of people, or vignettes of situations. These can be written by participants or by the researcher or developed in partnership. They can also be shared with participants to further interpret the data. But photographs also have a long history in qualitative research for presenting and constructing understanding (Butler-Kisber, 2010; Collier, 1967; Prosser, 2000; Rugang, 2006; Walker, 1993).

Less common are other visual forms of gathering data, such as “draw and write” (Sewell, 2011), artefacts, drawings, sketches, paintings, and collages, although all forms are now on the increase. For examples of the use of collage in data gathering, see Duke (2007) and Butler-Kisber (2010), and for charcoal drawing, Elliott (2008).

In qualitative inquiry broadly, these creative approaches are now quite common. And in the context of arts and health in particular (see, for example, Frank, 1997; Liamputtong & Rumbold, 2008; Spouse, 2000), we can see how artistic approaches illuminate in-depth understanding. However, in

case study research to date, I think narrative forms have tended to be most prominent.

Finally, for capturing the quality and essence of peoples' experience, nothing could be more revealing than a recording of their voices. Video diaries—self-evaluative portrayals by individuals of their perspectives, feelings, or experience of an event or situation—are a most potent way both of gaining understanding and communicating that to others. It is rather more difficult to gain access for observational videos, but they are useful for documentation and have the potential to engage participants and stakeholders in the interpretation.

Getting It All Together

Case study is so often associated with story or with a report of some event or program that it is easy to forget that much analysis and interpretation has gone on before we reach this point. In many case study reports, this process is hidden, leaving the reader with little evidence on which to assess the validity of the findings and having to trust the one who wrote the tale.

This section briefly outlines possibilities, first, for analyzing and interpreting data, and second, for how to communicate the findings to others. However it is useful to think of these together and indeed, at the start, because decisions about how you report may influence how you choose to make sense of the data. Your choice may also vary according to the context of the study—what is expected or acceptable—and your personal predilections, whether you prefer a more rational than intuitive mode of analysis, for example, or a formal or informal style of writing up that includes images, metaphor, narratives, or poetic forms.

Analyzing and Interpreting Data

When it comes to making sense of data, I make a distinction between analysis—a formal inductive process that seeks to explain—and interpretation, a more intuitive process that gains understanding and insight from a holistic grasp of data, although these may interact and overlap at different stages.

The process, whichever emphasis you choose, is one of reducing or transforming a large amount of data to themes that can encapsulate the overarching meaning in the data. This involves sorting, refining, and refocusing data until they make sense. It starts at the beginning with preliminary hunches, sometimes called “interpretative asides” or “working hypotheses,” later moving to themes, analytic propositions, or a theory of the case.

There are many ways to conduct this process. Two strategies often employed are *concept mapping*—a means of representing data visually to explore links between related concepts—and *progressive focusing* (Parlett & Hamilton, 1976), the gradual reframing of initially identified issues into themes that are then further interpreted to generate findings. Each of these strategies tends to have three stages: initial sense making, identification of themes, and examination of patterns and relationships between them.

If taking a formal analytic approach to the task, the data would likely be broken down into segments or datasets (coded and categorized) and then reordered and explored for themes, patterns, and possible propositions. If adopting a more intuitive process, you might focus on identifying insights through metaphors and images, lateral thinking, or puzzling over paradoxes and ambiguities in the data, after first immersing yourself in the total dataset, reading and re-reading interview scripts, observations and field notes to get a sense of the whole. Trying out different forms of making sense through poetry, vignettes, cameos, narratives, collages, and drawing are further creative ways to interpret data, as are photographs taken in the case arranged to explain or tell the story of the case.

Reporting Case Study Research

Narrative structure and story

As indicated in the introduction, telling a story is often associated with case study and some think this is what a case study is. In one sense, it is and, given that story is the natural way in which we learn (Okri, 1997), it is a useful framework both for gathering data and for communicating case study findings. Not any story will do however. To count as research, it must be authentic, grounded in data, interpreted and analyzed to convey the meaning of the case.

There are several senses in which story is appropriate in qualitative case study: in capturing stories participants tell, in generating a narrative structure that makes sense of the case (i.e., the story *you* will tell), and in deciding how you *communicate* this narrative (i.e., in story form). If you choose a written story form (and advice here can be sought from Harrington (2003) and Caulley (2008)), it needs to be clearly structured, well written, and contain only the detail that is necessary to give readers the vicarious experience of what it was like in the case. If the story is to be communicated in other ways, through, for example, audio or videotape, or computer or personal interaction, the same applies, substituting visual and interpersonal skill for written.

Matching forms of reporting to audience

The art of reporting is strongly connected to usability, so forms of reporting need to connect to the audiences we hope to inform: how they learn, what kind of evidence they value, and what kind of reporting maximizes the chances they will use the findings to promote policies and programs in the interests of beneficiaries. As Okri (1997) further reminds us, the writer only does half the work; the reader does the other (p. 41).

There may be other considerations as well: how open are commissioners to receiving stories of difficulties, as well as success stories? What might they need to hear beyond what is sought in the technical brief? And through what style of reporting would you try and persuade them? If conducting noncommissioned case study research, the scope for different forms of reporting is wider. In academia, for instance, many institutions these days accept creative and artistic forms of reporting when supported by supervisors and appreciated by examiners.

STYLES OF REPORTING

The most obvious form of reporting is linear, often starting with a short executive summary and a brief description of focus and context, followed by methodology, the case study or thematic analysis, findings, and conclusions or implications. Conclusion-led reporting is similar in terms of its formality, but simply starts the other way around. From the conclusions drawn from the analyzed data, it works backward to tell the story through narrative, verbatim, and observational data of how these conclusions were reached. Both have a strong story line. The intent is analytic and explanatory.

Quite a different approach is to engage the reader in the experience and veracity of the case. Rather like constructing a portrait or editing a documentary film, this involves the sifting, constructing, re-ordering of frames, events and episodes to tell a coherent story primarily through interview excerpts, observations, vignettes, and critical incidents that depict what transpired in the case. Interpretation is indirect through the weaving of the data. The story can start at any point provided the underlying narrative structure is maintained to establish coherence (House, 1980, p. 116).

Different again, and from the other end of a continuum, is a highly interpretative account that may use similar ways of presenting data but weaves a story from the outset that is highly interpretative. Engaging metaphor, images, short stories, contradictions, paradoxes, and puzzles, it is invariably

interesting to read and can be most persuasive. However, the evidence is less visible and therefore less open to alternative interpretations.

Even more persuasive is a case study that uses artistic forms to communicate the story of the case. Paintings, poetic form, drawings, photography, collage, and movement can all be adopted to report findings, whether the data was acquired using these forms or by other means. The arts-based inquiry movement (Mullen & Finley, 2003) has contributed hugely to the validation and legitimation of artistic and creative ways of representing qualitative research findings. The journal *Qualitative Inquiry* contains many good examples, but see also Liamputtong & Rumbold (2008). Such artistic forms of representation may not be for everyone or appropriate in some contexts, but they do have the power to engage an audience and the potential to facilitate use.

Generalization in Case Study Research

One of the potential limitations of case study often proposed is that it is impossible to generalize. This is not so. However, the way in which one generalizes from a case is different from that adopted in traditional forms of social science research that utilize large samples (randomly selected) and statistical procedures and which assume regularities in the social world that allow cause and effect to be determined. In this form of research inferences from data are stated as formal propositions that apply to all in the target population. See Donmoyer (1990) for an argument on the restricted nature of this form of generalization when considering single-case studies.

Making inferences from cases with a qualitative data set arises more from a process of interpretation in context, appealing to tacit and situated understanding for acceptance of their validity. Such inferences are possible where the context and experience of the case is richly described so the reader can recognize and connect with the events and experiences portrayed. There are two ways to examine how to reach these generalized understandings. One is to generalize from the case to other cases of a similar or dissimilar nature. The other is to see what we learn in-depth from the uniqueness of the single case itself.

Generalizing from the Single Case

A common approach to generalization and one most akin to a propositional form is cross-case

generalization. In a collective or multi-site case study, each case is explored to see if issues that arise in one case also exist in other cases and what interconnecting themes there are between them. This kind of generalization has a degree of abstraction and potential for theorizing and is often welcomed by commissioners of research concerned that findings from the single case do not provide an adequate or “safe” basis for policy determination.

However, there are four additional ways to generalize from the single case, all of which draw more on tacit knowledge and recognition of context, although in different ways. In *naturalistic generalization*, first proposed by Stake (1978), generalization is reached on the basis of recognition of similarities and differences to cases with which we are familiar. To enable such recognition, the case needs to feature rich description; people’s voices; and enough detail of time, place, and context to provide a vicarious experience to help readers discern what is similar and dissimilar to their own context (Stake, 1978).

Situated generalization (Simons, Kushner, Jones, & James, 2003) is close to the concept of naturalistic generalization in relying for its generality on retaining a connectedness with the context in which it first evolved. However, it has an extra dimension in a practice context. This notion of generalization was identified in an evaluation of a research project that engaged teachers in and with research. Here, in addition to the usual validity criteria to establish the warrant for the findings, the generalization was seen as dependable if trust existed between those who conducted the research (teachers, in this example) and those thinking about using it (other teachers). In other words, beyond the technical validity of the research, teachers considered using the findings in their own practice because they had confidence in those who generated them. This is a useful way to think about generalization if we wish research findings to improve professional practice.

The next two concepts of generalization—*concept* and *process generalization*—relate more to what you discover in making sense of the case. As you interpret and analyze, you begin to generate a theory of the case that makes sense of the whole. Concepts may be identified that make sense in the one case but have equal significance in other cases of a similar kind, even if the contexts are different.

It is the concept that generalizes, not the specific content or context. This may be similar to the process Donmoyer (2008) identifies of “intellectual generalization” (quoted by Butler-Kisber, 2010, p. 15) to indicate the cognitive understanding one

can gain from qualitative accounts even if settings are quite different.

The same is true for generalization of a process. It is possible to identify a significant process in one case (or several cases) that is transferable to other contexts, irrespective of the precise content and contexts of those other cases. An example here is the collaborative model for sustainable school self-evaluation I identified in researching school self-evaluation in a number of schools and countries (Simons, 2002). Schools that successfully sustained school self-evaluation had an infrastructure that was collaborative at all stages of the evaluation process from design to conduct of the study, to analyzing the results and to reporting the findings. This ensured that the whole school was involved and that results were discussed and built into the ongoing development of school policies and practice. In other cases, different processes may be discovered that have applicability in a range of contexts. As with concept generalization, it is the process that generalizes not the substantive content or specific context.

Particularization

The forms of generalization discussed above are useful when we have to justify case study in a research or policy context. But the overarching justification for how we learn from case study is *particularization*—a rich portrayal of insights and understandings interpreted in the particular context. Several authors have made this point (Stake, 1995; Flyvberg, 2006; Simons 2009). Stake puts it most sharply when he observes that “The real business of case study is particularization, not generalization” (p. 8), referring here to the main reason for studying the singular, which is to understand the uniqueness of the case itself.

My perspective (explored further in Simons, 1996; Simons, 2009, p. 239; Simons & McCormack, 2007) is similar in that I believe the “real” strength of case study lies in the insights we gain from in-depth study of the particular. But I also argue for the universality of such insights—if we get it “right.” By which I mean that if we are able to capture and report the uniqueness, the essence, of the case in all its particularity and present this in a way we can all recognize, we will discover something of universal significance. This is something of a paradox. The more you learn in depth about the particularity of one person, situation, or context, the more likely you are to discover something universal. This process of reaching

understanding has support both from the way in which many discoveries are made in science and in how we learn from artists, poets, and novelists, who reach us by communicating a recognizable truth about individuals, human relationships, and/or social contexts.

This concept of particularization is far from new, as the quotation from a preface to a book written in 1908 attests. Stephen Reynolds, the author of *A Poor Man's House*, notes that the substance of the book was first recorded in a journal, kept for purposes of fiction, and in letters to one of his friends, but fiction proved an inappropriate medium. He felt that the life and the people were so much better than anything he could invent. The book therefore consists of the journal and letters drawn together to present a picture of a typical poor man's house and life, much as we might draw together a range of data to present a case study. It is not the substance of the book that concerns us here but the methodological relevance to case study research. Reynolds notes that the conclusions expressed are tentative and possibly go beyond *this* man's life, so he thought some explanation of the way he arrived at them was needed:

Educated people usually deal with the poor man's life deductively; they reason from the general to the particular; and, starting with a theory, religious, philanthropic, political, or what not, they seek, and too easily find, among the millions of poor, specimens—very frequently abnormal—to illustrate their theories. With anything but human beings, that is an excellent method. Human beings, unfortunately, have individualities. They do what, theoretically, they ought not to do, and leave undone those things they ought to do. They are even said to possess souls—untrustworthy things beyond the reach of sociologists. The inductive method—reasoning from the particular to the general... should at least help to counterbalance the psychological superficiality of the deductive method. (Reynolds, 1908: preface)¹

Slightly overstated perhaps, but the point is well made. In our search for general laws, we not only lose sight of the uniqueness and humanity of individuals, but reduce them in the process, failing to present their experience in any “real” sense. What is astonishing about the quotation is that it was written over a century ago and yet many still argue today that you cannot generalize from the particular.

Going even further back, in 1798, Blake proclaimed that “To Generalize is to be an Idiot. To Particularize is the Alone Distinction of Merit.” In

research, we may not wish to make such a strong distinction: these processes both have their uses in different kinds of research. But there is a major point here for the study of the particular that Wilson (2008) notes in commenting on Blake's perception when he says: “Favouring the abstract over the concrete, one ‘sees all things only thro’ the narrow chinks of his cavern’” (referring here to Blake's *The Marriage of Heaven and Hell* [1793]; in Wilson, 2008, p. 62). The danger Wilson is pointing to here is that abstraction relies heavily on what we know from our past understanding of things, and this may prevent us experiencing a concrete event directly or “apprehend[ing] a particular moment” (Wilson, 2008, p. 63).

Blake had a different mission, of course, than case researchers, and he was not himself free from abstractions, as Wilson points out, although he fought hard “to break through mental barriers to something unique and living” (Wilson, 2008, p. 65). It is this search for the “unique and living” and experiencing the “isness” of the particular that we should take from the Blake example to remind ourselves of the possibility of discovering something “new,” beyond our current understanding of the way things are.

Focusing on particularization does not diminish the usefulness of case study research for policy makers or practitioners. Grounded in recognizable experience, the potential is there to reach a range of audiences and to facilitate use of the findings. It may be more difficult for those who seek formal generalizations that seem to offer a safe basis for policy making to accept case study reports. However, particular stories often hold the key to why policies have or have not worked well in the past. It is not necessary to present long cases—a criticism frequently levelled—to demonstrate the story of the case. Such case stories can be most insightful for policy makers who, like many of us in everyday life, often draw inferences from a single instance or case, whatever the formal evidence presented. “I am reminded of the story of...”

The case for studying the particular to inform practice in professional contexts needs less persuasion because practitioners can recognize the content and context quite readily and make the inference to their own particular context (Simons et al., 2003). In both sets of circumstances—policy and practice—it is more a question of whether the readers of our case research accept the validity of findings determined in this way, how they choose to learn, and our skill in telling the case study story.

Conclusion and Future Directions

In this chapter, I have presented an argument for case study research, making the case, in particular, for using qualitative methods to highlight what it is that qualitative case study research can bring to the study of social and educational programs. I outlined the various ways in which case study is commonly used before focusing directly on case study as a major mode of research inquiry, noting characteristics it shares with other qualitative methodologies, as well as its difference and the difficulties it is sometimes perceived to have. The chapter emphasizes the importance of thinking through what the case is, to be sure that the issues explored and the data generated do illuminate this case and not any other.

But there is still more to be done. In particular, I think we need to be more adventurous in how we craft and report the case. I suspect we may have been too cautious in the past in how we justified case study research, borrowing concepts from other disciplines and forms of educational research. More than 40 years on, it is time to take a greater risk—in demonstrating the intrinsic nature of case study and what it can offer to our understanding of human and social situations.

I have already drawn attention to the need to design the case, although this could be developed further to accentuate the uniqueness of the particular case. One way to do this is to feature individuals more in the design itself, not only to explore programs and policies through perspectives of key actors or groups and transactions between them, which to some extent happens already, but also to get *them* to characterize what makes the context unique. This is the reversal of many a design framework that starts with the logic of a program and takes forward the argument for personal evaluation (Kushner, 2000), noted in the interlude on evaluation. Apart from this attention to design, there are three other issues I think we need to explore further: the warrant for creative methods in case study, more imaginative reporting; and how we learn from a study of the singular.

Warrant for More Creative Methods in Case Study Research

The promise that creative methods have for eliciting in-depth understanding and capturing the unusual, the idiosyncratic, the uniqueness of the case, was mentioned in the methods section. Yet, in case study research, particularly in program and policy contexts, we have few good examples of the use of artistic approaches for eliciting and interpreting

data, although more, as acknowledged later, for presenting it. This may be because case study research is often conducted in academic or policy environments, where propositional ways of knowing are more valued.

Using creative and artistic forms in generating and interpreting case study data offers a form of evidence that acknowledges experiential understanding in illuminating the uniqueness of the case. The question is how to establish the warrant for this way of knowing and persuade others of its virtue. The answer is simple. By demonstrating the use of these methods in action, by arguing for a different form of validity that matches the intrinsic nature of the method, and, above all, by good examples.

Representing Findings to Engage Audiences in Learning

In evaluative and research policy contexts, where case study is often the main mode of inquiry or part of a broader study, case study reports often take a formal structure or sometimes, where the context is receptive, a portrayal or interpretative form. But, too often, the qualitative is an add-on to a story told by other means or reduced to issues in which the people who gave rise to the data are no longer seen. However, there are many ways to put them center stage.

Tell good stories and tell them well. Or, let key actors tell their own stories. Explore the different ways technology can help. Make video clips that demonstrate events in context, illustrate interactions between people, give voice to participants—show the reality of the program, in other words. Use graphics to summarize key issues and interactive, cartoon technology, as seen on some TED presentations, to summarize and visually show the complexity of the case. Video diaries were mentioned in the methods section: seeing individuals tell their tales directly is a powerful way of communicating, unhindered by “our” sense making. Tell photo stories. Let the photos convey the narrative, but make sure the structure of the narrative is evident to ensure coherence. These are just the beginnings. Those skilled in information technology could no doubt stretch our imagination further.

One problem and a further question concerns our audiences. Will they accept these modes of communication? Maybe not, in some contexts. However, there are three points I wish to leave you with. First, do not presume that they won't. If people are fully present in the story and the complexity is not diminished, those reading, watching, or hearing about the

case will get the message. If you are worried about how commissioners might respond, remember that they are no different from any other stakeholder or participant when it comes to how they learn from human experience. Witness the reference to Okri (1997) earlier about how we learn.

Second, when you detect that the context requires a more formal presentation of findings, respond according to expectation but also include elements of other forms of presentation. Nudge a little in the direction of creativity. Third, simply take a chance, that risk I spoke about earlier. Challenge the status quo. Find situations and contexts where you can fully represent the qualitative nature of the experience in the cases you study with creative forms of interpretation and representation. And let the audience decide.

Learning from a Study of the Singular

Finally, to return to the issue of “generalization” in case study that worries some audiences. I pointed out in the generalization section several ways in which it is possible to generalize from case study research, not in a formal propositional sense or from a case to a population, but by retaining a connection with the context in which the generalization first arose—that is, to realize in-depth understanding in context in different circumstances and situations. However, I also emphasized that, in many instances, it is particularization from which we learn. That is the point of the singular case study, and it is an art to perceive and craft the case in ways that we can.

Acknowledgments

Parts of this chapter build on ideas first explored in Simons, 2009.

Note

1. I am grateful to Bob Williams for pointing out the relevance of this quotation from Reynolds to remind us that “there is nothing new under the sun” and that we sometimes continue to engage endlessly in debates that have been well rehearsed before.

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Program Evaluation

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Abstract

The profession of educational and social program evaluation has expanded exponentially around the globe since the mid-1960s and continues to receive the considerable attention of theorists, methodologists, and practitioners. The literature on it is wide and deep, reflecting an array of definitions and conceptions of purpose and social role. This chapter discusses these topics and several others, including opinions about the choice of methods, some of which are used primarily by evaluators; the aspects of programs that evaluators typically address; the concept of value; the differences between evaluation and social science research; research on evaluation topics; and the major evaluation issues and concerns that have dominated discussion in the literature over the years.

Key Words: educational program evaluation, social program evaluation, evaluation purposes, evaluation roles, evaluation methods, evaluation models and approaches, qualitative methods in evaluation, research on evaluation, evaluation profession

Evaluation is a ubiquitous activity conducted by people of all ages. Humans have practiced evaluative activities in everyday life since the beginning of recorded history and could not have evolved socially without practicing evaluative activities long before. As a discipline and formal organizational activity, it has been conducted with vigor and considerable professional attention in Western nations for about half a century (Madaus & Stufflebeam, 2000). Our purpose in this chapter is to provide an overview of what has emerged from the work of evaluation theorists, methodologists, and practitioners about the evaluation of social, educational, health, and other programs during this period. We begin by defining programs and evaluation, discussing program evaluation purposes and the social roles that evaluation addresses in its various guises, presenting a brief overview of how evaluations are conducted, and identifying the major evaluation approaches and models. We continue with a discussion of the major features of qualitative methods in program

evaluation, a description of the aspects of programs that are addressed in evaluations, an overview of the concept of value in evaluation, and a discussion of the differences between evaluation and social science research. We conclude by discussing research on evaluation, discussing recurring fundamental issues in the evaluation literature, and providing final remarks about the state of the profession. Our intent is to provide a snapshot of the breadth and complexities of the profession and discipline of evaluation. Necessarily, our treatment is wide and thin; the reference list provides ample suggestions for deeper explorations of the topic. We do not discuss the breadth of evaluation methods because most are familiar and drawn from the compendium of social science research methods. We pay somewhat more attention to qualitative evaluation than to quantitative evaluation but refer readers to other chapters in this volume for explanations about how qualitative methods should be conducted. Table 23.1 lists common evaluation terms and their definitions.

Table 23.1 Major Terms Used in Program Evaluation

Term	Brief definition
Audience	The consumers or recipients of an evaluation.
Context	The social circumstances that surround and potentially influence the evaluand or evaluation.
Criteria	Indicators of success or merit that are linked to success or merit by empirical research.
Cultural competence	“Cultural competence in evaluation is a stance taken toward culture, not a discrete status or simple mastery of particular knowledge and skills. There are four key concepts associated with cultural competence: 1) culture is central to economic, political, and social systems as well as individual identity; 2) culture is fluid; 3) culture requires that evaluators maintain a high degree of self-awareness and self-examination; and 4) culture has implications for all phases of evaluation—including staffing, development, and implementation of evaluation efforts as well as communicating and using evaluation results.” (American Evaluation Association, 2011b)
Ethics	Ethics in evaluation refers to the rules of professional conduct recognized by the American Evaluation Association (e.g., integrity/honesty: Evaluators ensure the honesty and integrity of the entire evaluation process) and the Joint Committee on Standards for Educational Evaluation (e.g., propriety: The rights of individuals affected by evaluations should be protected.).
Evaluand	That which is being evaluated—person, performance, program, proposal, product, etc.
Evaluation practice	The professional application of or engagement in evaluation, as opposed to theories about evaluation application or use.
Evaluation profession	The occupation or vocation in evaluation requiring a complex set of knowledge and skills acquired through formal education and/or practical experience. The evaluation profession in the United States is served by the American Evaluation Association.
Evaluation questions	Key evaluation questions frame an evaluation: they indicate the scope of the evaluation and communicate the focus of the evaluation.
Evaluation research	Work that goes beyond the routine application of long-validated principles or techniques. It is similar to other research (e.g., mathematics research), although it usually identifies explicit evaluative conclusions.
Evaluation theory	Evaluation theory addresses why specific evaluation practices can lead to specific kinds of results across evaluation situations. There is no single theory of evaluation, and, given the continually evolving nature of evaluation, it is not likely that there will be one.
Evaluation use	Evaluation use or utilization occurs when evaluation findings or practice influence the actions or thoughts of stakeholders. Two main types of evaluation use are instrumental use (direct actions) and conceptual use (changes in thinking).
Evaluator roles	Evaluator roles vary widely. They are usually defined by the knowledge and skills they need, the functions they perform, and how they interact with stakeholders, the organization or program, and the profession.
External evaluation	An external evaluation is a process that uses people external to the program or institution to evaluate quality or standards. External evaluators are independent of the organization they are evaluating.
Feasibility	Feasibility refers to the degree to which an evaluation is realistic. Feasibility can refer to an evaluation’s design and procedures, the evaluator’s knowledge and ability, the evaluation’s cost effectiveness and time line, and the evaluation’s political context. It is one of the Program Evaluation Standards.

Formative evaluation	Evaluation conducted during the development or improvement of a program with the intent to improve.
Guiding Principles for Evaluators	The Guiding Principles are intended to guide the behavior of evaluators proactively and to inform clients, stakeholders, and the public about what to expect from professional evaluation. Developed by the American Evaluation Association, these principles include <i>systematic inquiry, competence, integrity and honesty, respect for people, and responsibilities for general and public welfare.</i>
Implementation	The extent to which a program or treatment has been executed or carried out in a particular situation.
Intended users	Intended users of an evaluation are those who have the responsibility to apply findings and implement recommendations.
Intended uses	“The concept of intended uses moves from the general idea of doing a useful evaluation to a specific and concrete focus on the priorities of intended uses for a particular evaluation’s primary intended users.” (Patton, 2005, p. 206)
Internal evaluation	An internal evaluation is a process that uses people internal (e.g., in-house staff) to the organization to evaluate the organization’s own programs. Internal evaluators are directly accountable to the organization they are evaluating.
Key evaluation checklist	A general checklist, developed by Michael Scriven, for many kinds of evaluations, particularly program evaluations. The checklist includes fourteen points: <i>description, background and context, consumer, resources, values, process, outcomes, costs, comparisons, generalizability, significance, recommendations, report, and meta-evaluation.</i>
Logic of evaluation	The logic of evaluation refers to how evaluation is possible, the nature of evaluation, and the logical structure of its inferences.
Logic model	A model articulated diagrammatically or in narrative form that demonstrates how a program or project is understood or intended to contribute to its specified outcomes. A typical logic model identifies inputs (resources), activities (what is done with the inputs), outputs (direct products), and outcomes (benefits).
Merit	The intrinsic value of an evaluand, as opposed to its extrinsic or system-related value.
Misuse of evaluation	Misuse of evaluation occurs when the evaluation is manipulated in ways that distort the findings or the inquiry.
Naturalistic evaluation	An approach that places less emphasis on the scientific method and more of an emphasis on contextual factors, unstructured interviewing, and observation. Generally, naturalistic evaluation tends to employ qualitative methods and is essentially a constructivist and interpretivist approach.
Needs assessment	A process or a systematic set of procedures undertaken for the purpose of setting priorities and making decisions about program or organizational improvement or allocation of resources.
Outcome evaluation	An evaluation that focus on outcomes rather than processes or inputs.
Performance evaluation	The evaluation of a particular achievement, in the form of output or process—for example, a student’s test performance or an athlete’s performance in a sporting event.
Personnel evaluation	Personnel evaluation refers to the systematic assessment of a person’s qualifications or performance in relation to an organizational role and purpose.
Portfolio assessment	An assessment technique often used in educational contexts to document student achievement. A portfolio is a collection of a student’s work that can demonstrate mastery, serve as a summary, or document a range of achievement or progress.

(continued)

Table 23.1 (Continued)

Term	Brief definition
Process evaluation	Process evaluation refers to an evaluation that focuses on the activities and events that occur as a program is delivered.
Product evaluation	Product evaluation usually refers to the evaluation of physical objects produced by a manufacturing process. It encompasses a product's life cycle, stakeholders and frames of references for the product, logic models and relationships among measures, and sources of data.
Program evaluation	The systematic investigation of planned interventions, designed to address social, educational, or commercial problems or needs, which results in judgments worth or significance.
Proposal evaluation	The evaluation of plans (i.e., public policy, defense, business) and more commonly, of proposals submitted for funding, usually to a foundation or government agency.
Program evaluation standards	A guide for evaluating educational activities for evaluators and evaluation users. The standards are organized into four categories: <i>utility</i> , <i>feasibility</i> , <i>propriety</i> , and <i>accuracy</i> .
Program logic	Program logic is the reasoning underlying an articulated causal model of how a program is understood or intended to contribute to its specified outcomes.
Program theory	Program theory refers to how a program brings about its effects or brings about improved effects. It can address prescriptive assumptions (what actions are required to solve a social problem) and descriptive assumptions (why the problem will respond to these actions) underlying a program.
Propriety	Propriety, one of the Program Evaluation Standards, refers to the extent to which an evaluation is conducted legally, ethically, and with due regard for the welfare of those involved in the evaluation, as well as of those affected by its results.
Pseudo-evaluation	Refers to an evaluation that lacks substance, or “the cover without the content.” Pseudo-evaluations fail to produce and report valid assessments of merit and worth to audiences who have the right to know. They are often motivated by political objectives.
Stakeholder	Stakeholders are people who have a stake or a vested interest in the evaluand (program, policy or product being evaluated) and therefore also have a vested interest in the evaluation. Stakeholders are usually those with decision authority over or responsibility for the evaluand or are people who are the intended beneficiaries or who may be disadvantaged by the program or evaluand (Greene, 2005).
Standards	Standards are the levels of performance, typically expressed as a rating or grade, on a given criterion.
Summative evaluation	A summative evaluation is one that is conducted at the completion of a program, typically for the benefit of some external audience or decision maker. It is a report <i>on</i> the program, not necessarily <i>to</i> the program.
Theory-driven evaluation	Theory-driven evaluation is a contextual or holistic assessment of a program based on the conceptual framework of program theory. The purpose of theory-driven evaluation is to provide information on the performance or merit of the program and on how and why the program achieves such a result.
Values	The values of people who are involved with programs are not definitive of the merit of a program but often form crucial input to the needs assessment, and often affect it indirectly. Four important issues in determining value address (a) defining what “having value” means, (b) whether the practice of evaluation ought to be concerned with making value judgments, (c) whether value judgments are an objective or subjective matter, or (d) determining who has responsibility for making value judgments in evaluation (Schwandt, 2005).
Worth	Worth is an outcome of an evaluation and refers to the extrinsic value of the evaluand in a particular context, as opposed to the evaluand's intrinsic value or merit.

The Evaluand: The Object of Evaluation

Before defining evaluation, it is helpful to define *evaluand*—the object of evaluation, or that which is being evaluated. Professional evaluators commonly discuss six categories of evaluands: programs, policies, performances, products, personnel, and proposals. These broad categories address the foci of most formal evaluative activity. Performances, for example, occur in the arts, sport, and education (as in assessments of writing skills), and products can include all that is evaluated in publications such as *Consumer Reports*, software applications such as statistical packages or mobile applications, instructional materials, and so forth. Performances and products can also address the foci of everyday informal evaluation: people regularly examine movie reviews before choosing an evening's entertainment and decide which brands and versions of products to buy based on friends' recommendations. Our focus in this chapter, however, is on formal evaluations of *programs*, defined as planned interventions of some duration designed to address social, educational, or commercial problems or needs. (Sometimes, program evaluation is called *evaluation research*, but this term has largely disappeared from the literature.) The logic of evaluation and the various evaluation topics that we discuss also apply in varying degrees to other evaluands, but our elaboration and examples are about programs.

Definition, Purposes, and Social Roles of Evaluation

Definition

The simplest definition of evaluation is *the judgment of merit or worth*. The French origin of the meaning has to do with assigning value to an evaluand. As Fournier (2005, pp. 139–140) succinctly stated,

Conclusions made in evaluations encompass both an empirical aspect (that something is the case) and a normative aspect (judgment about the value of something). It is the value feature that distinguishes evaluation from other types of inquiry, such as basic science research, clinical epidemiology, investigative journalism, or public polling.

Merit has to do with the intrinsic value of an evaluand—that is, whether it performs its intended function, independent of context or costs. Worth has to do with the degree to which an evaluand has extrinsic value—that is, the extent to which it meets needs in light of its context and costs. It also can be

about the value of the evaluand to society. A program might function well and achieve its outcomes, thus exhibiting merit, but if it does not meet a need or provide a service of value to its beneficiaries (e.g., program clients or students served in a program) or the broader society, it is not said to have worth.

Elaborations on the definition of evaluation in light of its use to examine programs are numerous. As Mark, Greene, and Shaw (2006, p. 6) stated, “If you ask 10 evaluators to define evaluation, you’ll probably end up with 23 different definitions.” The primary foci of these definitions have to do with the *purposes* and *uses* of program evaluation, such as decision-making about program continuation, program improvement, and increasing understanding about programs.

Purposes

The purposes of evaluation minimally include both *summative evaluation* purposes and *formative evaluation* purposes. These are reflected in Patton's (2008, p. 39) partial definition of evaluation as “the systematic collection of information about the activities, characteristics, and results of programs to make judgments about the program, improve or further develop program effectiveness, inform decisions about future programming, and/or increase understanding.” The summative purpose of an evaluation is to arrive at decisions about overall merit and worth and to use the results for deciding about future program operations or funding. A summative evaluation provides a summation; it is likely to address the outcomes of a program, the extent to which it achieved its intended objectives, or the degree to which the benefits of a program are worth its costs. Summative evaluation questions might address how well the evaluand performs, whether it is better than an alternative, or whether it is worth its costs, among other topics. Summative evaluation findings are used for oversight and compliance—for example, the extent to which programs address statutes and regulations or meet performance standards (Mark, Henry, & Julnes, 2000). Sometimes these are simply monitoring efforts. Summative evaluations can have high stakes, and many can be used to make “go/no-go decisions” (Cronbach et al., 1980). For example, legislative bodies mandate programs and want to know their effects when deciding about future program funding, so they require summative evaluations. Often, summative evaluations are required for the purposes of grant-making or contracting organizations; program personnel might not request or anticipate a need for these

evaluations, but funding agencies might need them for program accountability. Because of their consequences, the methods of summative evaluations require a good degree of rigor. The findings of evaluative studies that use unsound methods are not sufficiently warranted for supporting decisions about program continuation or funding.

A formative evaluation collects and reports information for improving evaluands. As Stake (2004) famously stated, the chef tasting the soup does a formative evaluation, and the customer tasting the soup does a summative evaluation. A formative evaluation helps in the formation of a program, when the results have leverage for making immediate, useful, and often minor program modifications. Just as consumer products are constantly revised when manufacturers identify flaws in their products, find better manufacturing materials, or learn ways to make products last longer, program personnel find ways to improve the delivery of their programs and thereby improve outcomes. Program personnel might want to know what additional resources should be devoted to a program, whether the activities and materials they have developed to implement a program need improvement, what program beneficiaries think might be changed, and so forth. Even though formative evaluation information is most useful in the earlier stages of a program, when changes are likely to be needed and summative evaluations are premature, evaluations might have formative purposes throughout the life of a study (Scriven, 1991a). Furthermore, the findings at any stage might be used for improving a program at a later date. Even at the end of an evaluation, it is likely that some of the summative results will point toward needed future modifications.

Note the emphasis of these purposes on the use of evaluation findings. Evaluations are conducted to provide useful information. Both summative and formative purposes are focused on the timing and manner of the use of evaluation results. Summative evaluation findings are more formal and comprehensive than formative evaluation findings, which often are presented quickly and without a great degree of formality, but both summative and formative evaluations focus on program stakeholders' use (i.e., examination and application in decision making) of the findings, albeit differently from one type to the other. The degree, form, and timing of the use of evaluation findings has been an issue discussed and debated among American evaluators since the 1970s, when large-scale studies tended to show minimal positive effects and evaluators

became concerned about the usefulness of their endeavors. Studies conducted and published relatively early in the program evaluation literature (e.g., Alkin, Daillak, & White, 1979; Cousins & Leithwood, 1986; King & Thompson, 1983; Patton et al., 1977) focused on issues of use; since then, it has probably been the most studied topic in the evaluation literature (e.g., Brandon & Singh, 2009; Cousins & Shulha, 2006; Fleischer & Christie, 2009; Hofstetter & Alkin, 2003; Johnson et al., 2009; Shulha & Cousins, 1997). Indeed, the use of evaluation findings is the major purpose of entire evaluation approaches such as *utilization-focused evaluation* (Patton, 2008, 2012) and *participatory evaluation* (Cousins & Chouinard, 2012).

Another primary purpose of evaluation described by some contributors to the literature is *to develop and test new general knowledge*. This purpose is more amorphous than others but also focuses on use. It reflects the reality that evaluations sometimes report information that cannot be or is not used for immediate program decision making but is useful at a later date for understanding a program's theory, methods, or effects in light of other similar programs. Sometimes, evaluation findings are out of date by the time they are produced, perhaps because the program context changed, program funding was eliminated for reasons having nothing to do with the evaluation, program personnel with new agendas and different evaluation questions took over the program, or newer program methods were developed. At other times, evaluation findings are ignored because no program personnel are held accountable, personnel and program funders are too busy to attend to the results, the evaluator has not taken steps to help enhance use, or evaluations are conducted strictly for political purposes. The findings of these studies might not be useful immediately, but the knowledge that they generate might be helpful in other settings. Furthermore, evaluators often publish the results of individual studies. For example, the journals *Evaluation Review* and *Evaluation and Program Planning* primarily publish the results of individual evaluations, thereby adding to the store of general knowledge about organized efforts to address social and educational needs and problems.

Finally, some evaluations are conducted for purposes that Weiss (1998, p. 22) has called "evaluation as subterfuge." Studies of this sort are sometimes conducted because evaluation agencies need funding to survive or because evaluators are unaware of the hidden purposes of evaluations. This might

occur when commissioning an evaluation to delay decision making, using evaluation findings to avoid organizational decisions that might provoke criticism, using evaluations as simple window dressing for changes that had already been made but were not made public for internal reasons, or simply commissioning studies for public relations when programs are already known to be successful. Stufflebeam and Shinkfield (2007) similarly list pseudo-evaluations conducted for public relations purposes, studies controlled by evaluation funders for political reasons, studies in which the evaluator panders to the client, and studies in which evaluation clients mislead evaluators about the intended uses of the findings. Evaluations for these purposes are appropriately criticized as a waste of resources, poor organizational leadership, and co-optation of professional ethics.

The Social Roles of Evaluation

In addition to the emphasis on the intended uses of evaluation findings in evaluators' definitions of evaluation, an emphasis on *social role* is also sometimes found. Smith (1999, p. 44) stated,

Although the technical purpose of evaluation is to assess merit or worth, the social ends to which this activity is put vary dramatically. The societal purpose of some forms of evaluation is to produce knowledge while, for other forms, its purpose is to promote social reform. The modern/post-modern debate in evaluation, for example, is as much about the proper societal role of evaluation as it is about a proper epistemology.

Social roles address the intended social ends of evaluations. Here, we are distinguishing between roles that have to do with the effects of evaluations on society and the roles of evaluators, such as serving as a "critical friend" of other evaluators, or as teacher, facilitator, collaborator, management consultant, organizational development consultant, program planner, scientific expert, and others (see Fitzpatrick, Sanders, & Worthen, 2004; Rallis & Rossman, 2003; Scriven, 1967). Social roles are apparent in the formative and summative distinction, with the former emphasizing helping the decision making of program personnel and the latter emphasizing the decision making of agencies that seek to hold programs accountable. The strong emphasis on use in the evaluation literature also reflects a social role of evaluations, as does an emphasis by some on conducting evaluations primarily to describe and explain programs (e.g., Stake, 2004).

Many contributors to the literature on evaluation theory and practice go beyond these functional aspects of evaluations and emphasize the values that evaluations should manifest or address. Most frequently, these values have to do with the place of evaluation in a democratic society. MacDonald's (1976) democratic evaluation approach, with its emphasis on allowing all participants in an evaluation to control the information that they provide and on publishing reports that are accessible by the public, represents one of the earliest widely known manifestations of this emphasis. The approach sought to ensure that powerful interests did not control evaluative activities. Cronbach et al. (1980, p. 4) stated that, "[i]nsofar as information is a source of power, evaluations carried out to inform a policy maker have a disenfranchising effect." Simons (1987) furthered the discussion, and House and Howe's (1999) deliberative democratic evaluation approach elaborated on methods for ensuring that stakeholders from all affected groups, as well as those with varying levels of organizational influence, participate in evaluation activities, particularly the discussion of results. Guba and Lincoln (1989) argued for progressing past previous "generations" of evaluation approaches by promoting consensus and negotiation among stakeholder groups that are participating in an evaluation. McTaggart (1991) pointed out some of the intraorganizational difficulties inherent in supporting democratic principles in evaluations, and others (e.g., Brandon, Lindberg, & Wang, 1993; Brandon, Newton, & Harman, 1993) pointed out the lack of program beneficiaries' participation in evaluations, which might detrimentally affect the validity of evaluation conclusions by ignoring important aspects of programs.

Greene (1996) reasoned that evaluation was a means to "democratize" the dialogue about critical social and educational issues. She stated that evaluators who employed democratic evaluation—in which the use of qualitative methodologies was said to be both necessary and appropriate—were manifesting their rights and responsibilities as "scientist citizens." The centering of qualitative evaluation around sociopolitical value dimensions is reflected in many examples in the literature over the last fifteen years involving evaluations of programs serving vulnerable populations throughout the world, such as diabetics in Spain (Santos-Guerra & Fernandez-Sierra, 1996), unemployed adults in France (Baslé, 2000), battered women in the

United States (Goldman & Du Mont, 2001), youth with HIV/AIDS in Madagascar (Rakotonanahary, Rafransoa, & Bensaid, 2002), low-income children in California (Sobo, Simmes, Landsverk, & Kurtin, 2003), and incarcerated substance users in Taiwan (Chang, Huang, & Chen, 2010).

Since the early 1990s, approaches emphasizing the social role of broadening participation in evaluations have burgeoned. These are grouped loosely under the label of collaborative approaches to evaluation. Cousins and Chouinard (2012) described three justifications for collaboration in evaluation and social science research, including social justice and democracy, having local participation define key features of a program and its evaluation (a constructivist rationale), and an emphasis on the use of evaluation results. Fetterman and his colleagues (e.g., Fetterman & Wandersman, 2005) have gone further by explicitly making the social and political self-determination, or *empowerment*, of program participants a key goal of evaluation. Wandersman and Snell-Johns (2005, p. 422) stated that “empowerment evaluation is not defined by its methods but by the collaborative manner in which methods are applied according to the empowerment evaluation principles.” Smith (2007, p. 175) stated that empowerment evaluation has “an overt political agenda of changing power differentials within the setting of interest, for if one thinks of social power as a relative commodity, then it can be increased for one group only at the expense of another.”

Issues of the degree and form of culturally responsive approaches to evaluation are another manifestation of a social role for evaluation:

At a basic level, cultural competence is appreciation and recognition of other cultural groups and acceptance of the inherent differences that exist among them. At its highest level, cultural competence involves designing appropriate programs, standards, interventions, and measures so that they are specific, relevant, and valid for each unique group. (Thompson-Robinson, Hopson, & SenGupta, 2004, p. 1)

In 2011, a task force of the American Evaluation Association (AEA) prepared a statement on cultural competence in evaluation (AEA, 2011*b*), asserting that (a) culture has implications for all evaluation phases, (b) all evaluations reflect cultural norms, (c) competence is particular to the cultural setting, and (d) evaluators need to cultivate awareness of the effects of their backgrounds on their understanding of culture. The statement urged evaluators to

use culturally appropriate evaluation methods that reflect the complexity of cultural identity, the recognition of the effects of power dynamics, and the propensity for bias in language.

Indigenous approaches are a narrower form of culturally responsive approaches to evaluation, focusing squarely on serving the social and political needs of indigenous peoples. Drawing on Smith (1999), evaluators working with indigenous peoples in New Zealand, Canada, the United States, and other former colonial societies have developed and espouse using native epistemologies as the foundation for research and evaluation. LaFrance (2004, pp. 39, 42) stated that

the goal of a competent evaluator, especially in Indian Country, should be to actively seek cultural grounding through the ongoing processes of appreciating the role of tribal sovereignty, seeking knowledge of a particular community, building relationships, and reflecting on methodological practices... Indigenous knowledge values holistic thinking... which contrasts with the linear and hierarchical thinking that characterizes much of Western evaluation practice.

It is asserted that nonindigenous evaluators might be blind to evaluation standards, such as those presented in the AEA’s (2004) *Guiding Principles for Evaluators*, unaware of the values and worldviews of indigenous communities, and unlikely to conduct evaluations that result in both “academic and cultural validity” (Kawakami, Aton, Cram, Lai, & Porima, 2008, p. 239). We expect that indigenous approaches to evaluation will be an expanding focus of the profession.

These evaluation approaches emphasize social roles that serve program stakeholders and beneficiaries as well as evaluators. In the eyes of some commentators, they are reactions to the proliferation of neoliberalism, the political theory that “promotes individual entrepreneurial freedom, frees capital to move across time and space by eliminating regulations, and assigns the state the role of facilitating competitiveness and privatization” (Mathison, 2009, p. 526). We elaborate more on evaluations that promote social roles favoring democratic values in the section on evaluation approaches and models.

How Evaluations Are Conducted

Many definitions of evaluation include something about the methods of evaluation, often referring simply to the *systematic nature* of evaluation

studies (e.g., Cronbach et al., 1980; Patton, 2008; Stufflebeam & Shinkfield, 2007; Weiss, 1998; Yarbrough, Shulha, Hopson, & Caruthers, 2011). The breadth of evaluation methods implied in this terminology is appropriate, because evaluators use most social science research methods, contingent on considerations such as the availability of time and funding and the breadth and depth of the evaluation.

During the first large-scale wave of American evaluations in the 1960s, evaluation methods by and large reflected traditional quantitative methods. Suchman (1967) produced one of the earliest textbooks on evaluation, in which it was clear that he “believed that the ideal study would adhere to the classic experimental model” (Stufflebeam & Shinkfield, 2007, p. 277). Milcarek and Struening’s (1975) bibliography of evaluation methods, published in an early handbook on evaluation, provided sections on conceptualization, measurement, design, and interpretation, with a total of seventy-five entries. Apart from a handful of general texts that conceivably covered a variety of methods and multiple research paradigms, nearly all the entries explicitly addressed quantitative research issues, and only one had the word *qualitative* in the title. An overwhelming emphasis on quantitative methods also was shown in *The International Encyclopedia of Educational Evaluation* (Walberg & Haertel, 1980).

In the 1970s and 1980s, the dominant use of quantitative methods in most social science disciplines came under attack in what some called the *paradigm wars* (Gage, 1989). Eisner (1979) demonstrated how educational criticism—the process of enabling others to see the qualities of something—expanded evaluators’ understanding of how they come to know, thereby creating new avenues for educational evaluation and research. Patton (1980) took this further with his tome, *Qualitative Evaluation Methods*, which provided evaluators and applied social scientists with a reference for expanding their methodological repertoire to include qualitative methods. Experimental designs, which long had been used successfully in small-scale research studies, began to be considered difficult or unworkable in the contexts of large-scale social and educational programs, resulting, in part, in findings showing program failures. House (1980, pp. 250–251) stated that approaches relying on “objectivist epistemology” (e.g., those using the methods of systems analysis or behavioral objective studies) failed because they relied on “the truth

aspect of validity to the exclusion of the credibility and normative aspects.” Evaluators taking an “interpretivist” stance considered evaluation findings to be about “contextualized meaning,” with reality viewed as socially constructed and truth an issue of agreement (Greene, 1994). Greene, Doughty, Marquart, Ray, and Roberts (1988) and Whitmore and Ray (1989) introduced the use of audits in qualitative evaluations to enhance “internal quality, external defensibility, and thus the stature and utilization of naturalistic evaluation” (Greene et al., 1988, p. 352).

Slowly, evaluators incorporated the interpretivist stance into their collection of epistemological perspectives (Greene & Henry, 2005). Some scholars trained in quantitative methods began to reject their training and quantitative perspective “as epistemologically inadequate and expressed a qualitative preference” (Cook, 1997, p. 33); for example, Stake, trained as a psychometrician, began advocating that evaluators provide both descriptive results and judgmental results in evaluation reports. He is now widely known as a case study expert. Furthermore, it was clear that, like qualitative evaluators, quantitative evaluators understood full well that knowledge is continually refined and that truth is not absolute (Reichardt & Rallis, 1994). Except perhaps for among some epistemological diehards, the qualitative-quantitative debates eventually quieted, with quantitative evaluators accepting the value of qualitative methods. Evaluation theorists, methodologists, and practitioners for the most part came to agree that the paradigms were not incompatible, that a partnership between the two was possible (Hedrick, 1994; Smith, 1994), and that evaluation content was more important than evaluation methodologies (House, 1994). Yin (2011, p. 287) concluded that

The harshness of the debate obscured the fact that contrasting methods had always coexisted in social science, with no method consistently prevailing over any other. Methodological differences had long been recognized and tolerated in such fields as sociology, well predating the disagreements in program evaluation.

This conclusion is exemplified well by the evaluator and evaluation theorist Michael Patton (1990, p. 39), who stated

Rather than believing that one must choose to align with one paradigm or the other, I advocate a paradigm of choices. A paradigm of choices rejects

methodological orthodoxy in favor of *methodological appropriateness* as the primary criterion for judging methodological quality. The issue then becomes... whether one has made sensible methods decisions given the purpose of the inquiry, the questions being investigated, and the resources available. The paradigm of choices recognizes that different methods are appropriate for different situations.

In the mid-1980s and early 1990s, the literature began to reflect the value of integrating quantitative and qualitative methods for triangulation purposes (Kidder & Fine, 1987; Smith, 1986; Smith & Kleine, 1986) and for improving the rigor and credibility of evaluations (Silverman, Ricci, & Gunter, 1990). These developments reflect the beginning of the profusion of mixed-methods evaluations (Greene, 2007; Greene & Caracelli, 1997; Rallis & Rossman, 2003), an approach reflecting the pragmatic considerations that evaluators have had about the use of both qualitative and quantitative methods, as evaluation needs require. The literature has demonstrated a gradual convergence of quantitative and qualitative evaluators toward the recognition of the value, use, and advancement of each others' methods. Qualitative evaluation methods have become widely used in program evaluation, usually in conjunction with quantitative methods. Their widespread use can also be attributed to what they lend to an evaluation: added depth of understanding of program processes and participant outcomes. In short, most evaluators now agree that multiple methods and multiple ways of knowing are essential to a program evaluation's overarching purpose and social role.

Methods Unique to Evaluation

No overview of program evaluation methods is complete without a discussion of those that are unique to (or at least largely used by) evaluators. The features of these methods reflect aspects of evaluation that are not shared with social science research. Some of the methods are not well known outside of evaluation circles, and some are not widely used by evaluators. We highlight three here to give a taste of how evaluation generates methods appropriate for its purposes.

Michael Scriven has been a major contributor to the development of evaluation methods outside the realm of the social sciences, beginning with the Goal-Free Evaluation approach (Scriven, 1974). The premise underlying this approach is that evaluations focusing solely on program goals and objectives might ignore the unintended effects

and unaddressed needs of a program's beneficiaries. Scriven and those following his approach maintain that evaluators can identify these in needs assessments: "At the very least, the evaluation team should make some effort to lay out the evidence of the need that led to the development of the evaluand in the first place" (Davidson, 2005, p. 39). Logic models, which are graphic displays of major components of programs, including resources, activities, outputs (i.e., the products or services provided), and outcomes at various stages, also are useful for ensuring that evaluations address not only long-term but also short- and intermediate-term goals (Davidson, 2005). Scriven (1976) also proposed the *modus operandi* method, a procedure for

identifying the cause of a certain effect by detailed analysis of the configuration of the chain of events preceding it and of the ambient conditions... The term refers to the characteristic pattern of links in the causal chain, which the detective refers to as the *modus operandi* of the criminal. These can be quantified and often configurally scored; the problem of identifying the cause can thus be converted into a pattern-recognition task for a computer. (Scriven 1991*b*, p. 234).

Scriven expanded on this approach in describing the General Elimination Methodology, in which the evaluator (a) develops a list of all possible causes, (b) considers the possible *modus operandi* for each cause, and (c) identifies which of the latter are present for each of the former. The *modus operandi*/General Elimination Methodology approaches rely on commonly used methods for examining effects in everyday life—an approach widely applied by Scriven, as in his discussion of *probative logic* (i.e., the logic of legal reasoning as applied to evaluation; Scriven, 1987, 2005). A similar approach, developed by Mayne (2001), labeled *contribution analysis*, "aims to compare an intervention's postulated theory of change against the evidence in order to come to robust conclusions about the contribution that it has made to observed outcomes" (White & Phillip, 2012).

A final example in our short list of novel evaluation approaches is the Success Case Method, "a carefully balanced blend of the ancient art of storytelling with more modern methods and principles of rigorous evaluative inquiry and research" that uses "sound principles of inquiry to seek out the right stories to tell," backed up with "solid evidence" (Brinkerhoff, 2003, p. 4). The method is used to

gather evidence about illustrations of the best-case scenarios of successful interventions, adapting the approach of analyzing extreme groups, as is done in some manufacturing. After identifying cases of success, evaluators use focus groups, interviews and survey questionnaires, key informants, journalistic inquiry methods, and other methods, largely qualitative, to identify the operations and context of the successful cases.

Major Evaluation Models and Approaches

Evaluators have a number of *approaches* for addressing the issues that have been debated among evaluators for years, such as the purposes, methods, social roles, and uses of evaluation. Some have called these approaches *models of evaluation* in the sense that “each one characterizes its author’s view of the main concepts in evaluation work and provides guidelines for using these concepts to arrive at defensible descriptions, judgments, and recommendations” (Madaus, Scriven, & Stufflebeam, 1983, p. xii–xiii). Others eschew the formality implied by the term *model* or believe it is too restrictive; for example, Scriven (2003) suggested the term *conceptions of evaluation*. Still others refer to the approaches as evaluation theories, but the word “theory” is better if restricted to “underlying fundamental issues such as the nature of evaluation, purpose, valuing, evidence, use, and so on” (Smith, 2010, p. 384). For our definition of model, we borrow from Smith (p. 384), who has defined models as “prescriptions for how to conduct an evaluation” that “incorporate positions on various underlying theories about fundamental issues.”

We provide a list, with definitions, of many of the models in Table 23.2. These have been developed and promulgated over the years. Schema for categorizing them began appearing early. Roughly ten years into the period in which evaluation in the United States began to flourish, Stake (1973) listed student-gain-by-testing, institutional self-study by staff, blue-ribbon panel, transaction-observation, management analysis, instructional research, social-policy analysis, goal-free evaluation, and adversary evaluation. Except for the last two in the list, the models reflected analytic approaches that existed previously. Five years later, House (1980) included systems analysis and art criticism in a largely similar list, again reflecting existing analytic approaches. The 1980s saw the development of more models stemming less from existing approaches and more from the exigencies of evaluative work, such as Stake’s responsive evaluation model (Stake,

2004), Stufflebeam’s Context-Input-Process-Product model (Stufflebeam & Shinkfield, 2007), Patton’s utilization-focused evaluation (Patton, 2008), and Cronbach’s ninety-five evaluation theses (Cronbach et al., 1980), among others. The 1990s and later saw the emergence and refinement of approaches emphasizing collaboration among evaluators and program stakeholders, such as stakeholder-based evaluation (Mark & Shotland, 1985), practical and transformative participatory evaluation (Cousins & Chouinard, 2012; Mertens, 2009), empowerment evaluation (Fetterman & Wandersman, 2005), and democratic deliberative evaluation (House & Howe, 1999), as well as attention to cultural issues (AEA, 2011a; Thompson-Robinson et al., 2004) and the evaluations of indigenous peoples (Kawakami et al., 2008).

Stufflebeam has provided probably the most exhaustive listing and description of models (Stufflebeam, 2001; Stufflebeam & Shinkfield, 2007). Not all will agree with his list and categorization; for example, he ignores models that focus on indigenous and cultural issues, perhaps in part because they have received attention mostly since his work was published, and he classifies empowerment evaluation as a pseudo-evaluation, despite vociferous disagreement by Fetterman and his colleagues (Fetterman, 1995). The schema has five groups categorizing twenty-six models: pseudo-evaluations, models focusing on evaluation questions or methods, models focusing on improvement or accountability, models on social agenda or advocacy, and eclectic approaches (a category including only utilization-focused evaluation). As in most categorizing, the group labels do not reflect the central features of each of the members of the group; some models seemed to be forced into the groups. It is indeed the case, however, that the five groups reflect basic aspects of evaluation, such as the formative and summative purposes of evaluation, as suggested by the improvement and accountability group; adherence to inclusive and deliberative principles, as suggested by the social agenda and advocacy group; and the prominence of social scientists within evaluation, as suggested by the questions-and-methods group.

Two Current, Widely Used Approaches

Of major evaluation approaches, two that have received considerable attention in recent years focus both on methods and on intended uses. These are studies employing experiments as their primary design feature and collaborative evaluations.

Table 23.2 Evaluation approaches and models

Model or approach	Summary	Theorist
Appreciative inquiry	A strengths-based and future-oriented method and approach to inquiry that seeks to understand what is best about a program, organization, or system, to create a better future. The underlying assumptions of appreciative inquiry suggest that the focus becomes the reality, that there are multiple realities and values, that the very act of asking questions influences thinking and behavior, and that people will have more enthusiasm for and motivation to change if they see possibilities and opportunities for the future.	Hallie Preskill; Tessie Catsambas
Collaborative evaluation	A type of evaluation midrange on the continuum of those involving program stakeholders in evaluations. The concept of working together, of “co-laboring” suggests a more coequal responsibility for the evaluation process, with program people concerned with issues of the evaluation’s utility and feasibility and the evaluator concerned with issues of its propriety and accuracy.	Rita O’Sullivan
Connoisseurship	In the context of educational programs, this model likens evaluators to educational connoisseurs and the evaluation process as being analogous to art criticism. There are three components: the <i>descriptive component</i> , which attempts to capture the aesthetic or “feeling” dimensions of the phenomenon being evaluated; the <i>interpretive component</i> , which uses social science theory to make sense of social phenomena; and the <i>evaluative component</i> , which does not use a predefined standard or set of standards to assess phenomena.	Elliott Eisner
Context-Input-Process-Product (CIPP)	A comprehensive framework for guiding formative and summative evaluations of programs, projects, personnel, products, institutions, and systems. Context evaluations refer to needs, problems, assets, and opportunities as bases for defining goals and priorities and judging outcomes. Input evaluations assess alternative approaches, competing action proposals, and associated budgets for meeting targeted needs and achieving objectives. Process evaluations assess the implementation of plans to guide activities and later judge program performance and help explain outcomes. Product evaluations identify intended and unintended outcomes, both to help keep an enterprise on track, and, ultimately, to gauge its success in meeting targeted needs.	Daniel Stufflebeam
Cost-benefit analysis	A principal tool for evaluating alternatives when economic constraints and limited resources are considered. This tool uses the typical methods of evaluating outcomes, but also considers results in the light of the resource investments required to obtain them. Cost-benefit analysis provides answers to the question of whether the intervention is “worth it” in the sense that its benefits will exceed its costs. And, among those alternatives that meet the first criterion, it aims to identify which have the greatest benefits relative to costs.	Henry Levin
Countenance model	This model makes a clear distinction between description and judgment, clarifies the kinds and sources of data in evaluation, and acknowledges the complexity of any evaluand.	Robert Stake
Culturally responsive evaluation	Evaluation that addresses issues relating to the influence of cultural context in educational programs and settings. Evaluators who are culturally responsive are culturally informed and committed to designing, conducting, and reporting culturally responsive educational evaluations.	Stafford Hood; Karen Kirkhart

Deliberative democratic evaluation	An approach to evaluation that uses democratic concepts and procedures to determine evaluative conclusions. It attempts to arrive at unbiased conclusions by considering all relevant interests, values and perspectives; by engaging in extended dialogue with major stakeholders; and by promoting extensive deliberation about the study's conclusions, in addition to employing traditional evaluation methodologies.	Ernest House
Developmental evaluation	The purpose of developmental evaluation is to help develop the intervention or program. Evaluators become part of the program design team or an organization's management team. The evaluator's primary function on the team is to facilitate discussions by infusing evaluative questions, data, and logic and to support data-based decision making in the developmental process.	Michael Quinn Patton
Emancipatory evaluation	Similar to empowerment evaluation and other participant-oriented evaluation approaches, this approach aims to address the needs of those people with least power, so they might be better able to influence their own destiny.	Donna Mertens
Empowerment evaluation	The use of evaluation concepts, techniques, and findings to foster improvement and self-determination. It is guided by a commitment to truth and honesty and is designed to help people help themselves and improve their program using a form of self-evaluation and reflection. Program participants conduct their own evaluations, and an outside evaluator often serves as a coach or additional facilitator.	David Fetterman
Feminist evaluation	Like other evaluation approaches, feminist evaluation is concerned with measuring the effectiveness of programs, judging merit or worth, and examining both formative and summative data to promote change. Unlike other approaches, it emphasizes gender issues, the needs of women, and the promotion of change.	Denise Seigart
Fourth-generation evaluation	The successor model incorporating three earlier generations of evaluation models (objectives, description, and judgment) and moving beyond them to include intensive stakeholder participation in determining both the course of the evaluation and, as well, what actions should be taken on the evaluation results.	Yvonna Lincoln
Goal-free evaluation	This approach determines that an evaluation should examine the value of a program by investigating what it is doing rather than what it is trying to do. It is an approach designed to reduce bias in evaluation. It relies heavily on needs assessment to judge the quality and fit of the program to client needs.	Michael Scriven
Illuminative evaluation	This concept grew out of a paper written by Parlett and Hamilton (1977) that explored evaluation alternatives with respect to a nonprogrammatic English curriculum innovation, Resources for Learning. It emphasized that critique should always be concrete and transcendental rather than oppositional—critique that is beyond the status quo and that offers fresh horizons. Essentially, “illumination” assumes insightful knowledge and creates power to act and rearrange the world according to particular values.	Malcolm Parlett; David Hamilton
Indigenous evaluation	An indigenous evaluation framework is defined by its physical and cultural location, in which context and the indigenous group involved determine both epistemology and method choice.	Joan LaFrance

(continued)

Table 23.2 (Continued)

Model or approach	Summary	Theorist
Interactive evaluation	An evaluation in which the individuals in the organizational entity being evaluated have the opportunity to react to the content of a first draft of an evaluative report that has valid criticisms or additions.	Jean King
Objectives-based evaluation	This approach refers to a class of evaluation approaches that centers on the specification of objectives and the measurement of outcomes. It focuses on generating information for accountability and decision making by developing and measuring the appropriate objectives for these purposes.	Ralph Tyler
Participatory evaluation	This approach involves program staff or participants actively in decision making and other activities related to the planning and implementation of evaluation studies. The reasons for participant involvement include the desire to effect change in individuals, in programs or organizations, and to build the capacity of a group or institution to conduct additional evaluations.	J. Bradley Cousins
Realist evaluation	This approach, which is rooted in a realist philosophy of science, focuses on developing explanations of the consequences of social actions that contribute to a better understanding of why, where, and for whom programs work or fail to work (Henry, 2005). The emphasis is on identifying the mechanisms that produce observable effects and testing these mechanisms and the other contextual variables or individual characteristics (or moderators) that may influence the observed effects.	Melvin Mark
Responsive evaluation	Responsive evaluation favors personal experience and draws on the ordinary ways people perceive quality and worth. It also emphasizes a program's activity over its intents, responds to stakeholders' requirements for information, and values the cultural perspectives of the evaluation's participants.	Robert Stake
Stakeholder-based evaluation	Evaluation in which stakeholders have a significant role. The way in which stakeholder groups are selected for participation in the evaluation and the consequences for involvement can vary, as can the type or level of stakeholder involvement required for effective participation.	Anthony Bryk
Success case method	A process that combines analysis of extreme groups with case study and storytelling. The purpose of the method is to determine how well an initiative is working by highlighting the contextual differences between successful and unsuccessful adopters of new initiatives.	Robert Brinkerhoff
Transformative evaluation	The primary objective of transformative evaluation—a form of participatory evaluation—is to facilitate key stakeholder groups to determine what really matters in transformational development and then to plan and implement a technically sound evaluation around their conclusions (Mertens, 2003).	Donna Mertens
Utilization-focused evaluation	This approach is concerned with how people apply evaluation findings and experience the evaluation process. The goal of this approach is intended use (of the evaluation results) by intended users.	Michael Quinn Patton

Randomized experiments were frequently the design of choice used to study the US federal evaluations of large-scale social and educational programs funded in the 1960s as part of President Lyndon's Johnson Great Society endeavor. Legislative bodies and government decision makers wanted convincing answers about program effects and saw the advantages of experiments in providing such answers. When the experiments found programs to be ineffective, evaluators (particularly of education programs) began to question the extent to which the studies were subject to validity threats. Theorists debated the relative importance of internal validity versus external validity, with Donald Campbell supporting the former while extolling randomized field studies and Lee Cronbach supporting the latter in the interest of providing results useful for program decision making.

Evaluation funding of program effects was diminished considerably at the US federal level during the years of the Reagan presidential administration, resulting in fewer expensive experimental designs in evaluations, at least of education programs. In the 1970s and 1980s, most large-scale interventions were "demonstration projects," with the majority of evaluations only examining service delivery inputs into projects while ignoring their effects (Boruch, 1991). In the ensuing years, however, an increasing number of experiments were conducted of health, criminology, manpower training, and other social welfare programs, as well as of some education programs addressing substance abuse, and a cadre of evaluators continued to support their use and implementation. Meanwhile, a movement toward evidence-based decision making, emanating in large part within medicine, began to take hold. Organizations began collecting studies and subjecting them to meta-analyses and other review procedures that were touted as rigorous examinations of methods and effects, with the Cochrane Collaboration focusing on healthcare studies beginning in 1994, and the Campbell Collaboration broadening the focus to social programs in 1999. Experiments in education evaluation began again in earnest with the beginning of the George W. Bush presidential administration in 2001, pushed in large part by the arrival of a psychologist as the head of the Institute of Education Sciences, the reconstituted research arm of the US Department of Education. This resurgence was perhaps most strongly exemplified in the US Department of Education's push for "scientifically based research," a pseudonym

for *randomized controlled trials* (also variously called *randomized clinical trials*, *randomized control trials*, *randomized comparative trials*, or *randomly controlled trials*).

The increase in funding provided for education research and evaluation has resulted in many additional experimental and quasi-experimental studies. It has even resulted in providing federal seed money to develop a new professional association, the Society for Research on Educational Effectiveness, which meets regularly to present the results of experimental studies of education. In 2010, the president of the association commented at an association meeting that about 1,000 new evaluators proficient in conducting experiments had been trained over the previous decade. Despite strong negative reactions of some in the program evaluation community (e.g., AEA, 2003), it is apparent that, in the new push for experiments in education evaluation, multiple approaches to evaluation serving varying social roles remain alive and well.

Collaborative models also have recently received considerable attention in recent years. Strains of the approach have been seen in action research for years. Beginning in about the mid-1990s (e.g., Cousins & Earl, 1995), evaluators began discussing participatory evaluations, including those conducted primarily for practical reasons, such as practical participatory evaluation, and those conducted to enhance the social, political, and organizational power of program personnel and program beneficiaries. Depending on the model, the intended benefits and effects of collaborative evaluations include the acceptance, promotion, and application of evaluation results; enhanced learning about the organization functioning, purposes, procedures, culture, context, and so forth; increased evaluation validity by gleaning stakeholders' knowledge and skills about the program; building the evaluation knowledge and skills of participating stakeholders; and providing program personnel and other stakeholders with the means and results to enhance their organizational, political, and social influence:

What sets participatory evaluation apart from traditional and mainstream approaches to evaluation (e.g., evaluations framed by experimental or quasi-experimental designs) is the focus on the collaborative partnership between evaluators and program community members (i.e., program developers, managers, implementers, funders, intended beneficiaries, or other relevant

stakeholder groups), all of whom bring a specific complementary focus and value to the inquiry. On the one hand, evaluators bring knowledge of evaluative logic and inquiry methods, standards of professional practice, and some knowledge of content and context. On the other hand, program community members bring a detailed and rich understanding of the community and program context, understanding of program logic, and often some understanding of research methods and evaluation, depending upon their prior level of knowledge and experience. It is the relationship that emerges, along with the dialogue and conversations that ensue, that effectively define the parameters of participatory practice and the knowledge that are ultimately co-constructed as a result of these practices. (Cousins & Chouinard, 2012, p. 5)

Like studies using experimental designs, studies adopting participatory evaluations have also had their detractors. Some have criticized the emphasis on the use of evaluation findings that is central to all forms of collaborative evaluations (Donaldson, Patton, Fetterman, & Scriven, 2010); others have criticized transformative models (particularly empowerment evaluation) as being a form of pseudo-evaluation (Stufflebeam & Shinkfield, 2007) or as having a very thin empirical base establishing its effectiveness as a model (Miller & Campbell, 2006). Nevertheless, as reflected in numerous published reports of collaborative evaluations, as well as by a steady stream of books (Cousins & Chouinard, 2012; Fetterman & Wandersman, 2005; O'Sullivan, 2004; Rodriguez-Campos, 2005), the models are finding a widening audience among evaluators (particularly in international development studies, studies with diverse populations, and in small local studies) and have established themselves within the mainstream of evaluation practice.

The Major Features of Qualitative Methods in Evaluation

It is beyond the scope of this chapter to provide an overview of the methods of evaluation, but it is consistent with the focus of this handbook to present some insights about using qualitative data collection methods in evaluations. Mixed-methods designs, used to balance types of methods to varying degrees, are ubiquitous in evaluations, and the results of interviews, focus groups, observations, and document reviews are found in many evaluative studies. Using qualitative methods allows evaluators

to learn about programs in enough depth to provide rich descriptions (Stake, 2004) of program purposes, contexts, resources, activities, products, and outcomes at various stages of development and implementation. Reports of studies that have used qualitative methods can help evaluation audiences learn about programs in all the complexities of their daily functioning. They are particularly advantageous when evaluation issues are not clear in advance, as can often be the case when evaluations are mandated, and when funded program proposals are insufficient for preparing evaluation designs and plans, as can often be the case for evaluations of small programs or interventions. Evaluators use qualitative methods to learn program personnel's perspectives—and, if they dig deep enough, program beneficiaries' perspectives—thereby enhancing evaluation validity in a manner roughly similar to establishing content validity in test development. Furthermore, evaluators using qualitative methods are better placed to know the history of a program and to learn about unplanned events. For studies focusing on outcomes, qualitative methods help evaluators learn and understand mediating variables between implementation and outcomes. In Greene and Henry's (2005, p. 348) words, "understanding outcomes cannot happen outside of understanding the nature and form of program participation." Furthermore, studies using qualitative methods can obtain "one-of-a-kind insights.... The question is not 'How representative is this?' but 'Does it happen even once?'" (Stake, 2004, p. 88).

Weiss (1998, p. 265) stated, "Qualitative evaluation has a special advantage for finding out when program operations depart from expectations and new theories are needed." Even when program theory is well-explicated at the beginning of an evaluation, in many studies, it is fluid and adapting to contingencies as they arise. Qualitative methods allow evaluators to improve evaluation designs and plans as their studies proceed, thereby helping to improve evaluation validity to a greater depth than in evaluations that do not use the methods. Qualitative methods are particularly appropriate for formative evaluations occurring as programs are developed. They allow for rapid feedback of results and for making recommendations for improvement following unanticipated variations in program design, development, and implementation.

Qualitative evaluations and, in some instances, mixed-methods evaluations with significant qualitative components, are best achieved under circumstances in which (a) there is sufficient time to

study programs in their natural cycles, (b) intensive inquiry can be conducted without concern for ethical violations, (c) the evaluation is conducted as unobtrusively as possible, (d) data sources are diverse, and (e) program stakeholders and evaluators agree about the methodological approach (Shaw, 1999). Evaluators doing qualitative evaluations should address potential pitfalls such as inadvertently promoting discord while attempting to represent diverse perspectives, particularly between program personnel and program beneficiaries (Bamberger, Rugh, & Mabry, 2006), as well as finding it difficult to summarize findings in a sufficiently short report, particularly when reporting the perspectives of multiple stakeholder groups.

Narratives and complexity may increase the length of reports unhelpfully, and representation of diverse viewpoints may encourage conflict as to how evaluation results should be used. To offset length, qualitative practitioners try to select for reporting those narratives that are most informative and interesting and that best convey complex implications that numbers may only suggest. (Bamberger et al., p. 274)

Information that reflects badly on individuals needs to be reported without attribution or identification. Furthermore, if evaluators' relationships with program personnel become deep and personal, bias might result (Weiss, 1998). The likelihood of biased results is counterbalanced by the payoff of deeper understanding, the probability that stakeholder groups will recognize and challenge biases in evaluation reports, and by triangulation of results among methods.

Aspects of Evaluands That Are Evaluated

Many of the definitions of evaluation that evaluators have proposed over the years address the aspects of programs that should be examined. For example, Weiss (1998, p. 4) mentioned the "operation and/or the outcomes" of the program; Mark, Henry, and Julnes (2000, p. 3) listed program "operations, effects, justifications, and social implications;" Rossi, Lipsey, and Freeman (2004, p. 2) mentioned "the workings and effectiveness of social programs;" Stufflebeam and Shinkfield (2007, p. 16) discussed "probity, feasibility, safety, significance, and/or equity" in addition to merit and worth; Patton (2008, p. 39) discussed the "activities, characteristics, and results of programs;" and Yarbrough et al. (2011, p. 287) simply

specified "defined dimensions." In the terminology of Scriven's *general logic of evaluation*, these are known as the *criteria* on which programs and other evaluands are evaluated (Scriven, 1991*b*). Scriven gives the establishment of evaluation criteria as the first step of his general logic.

Clearly, if programs are to be evaluated, the criteria that the evaluation will address must be established first. Understanding evaluation criteria requires understanding programs, of course. Shadish, Cook, and Leviton (1991) discussed three elements of a theory of social programs, including their internal structure and functioning, the external constraints on programs, and aspects of social change and program change. Internal structure includes program personnel, program beneficiaries, the resources available to the program, its administration and budgeting, its facilities, its internal organization, its intended outcomes, and the social norms within which it operates. It is important that evaluators understand the background characteristics of communities served by programs, how program beneficiaries are recruited and selected, the nature and purposes of program activities, the necessary skills of personnel, the materials that are needed to successfully deliver programs, and intended program outcomes. The external constraints have to do with political constituencies, external program stakeholders, the availability of community resources, and the political and economic values of the community and society. These are issues of context that are increasingly seen as essential to understand when conducting evaluations (Rog, Fitzpatrick, & Conner, 2012). Understanding change has to do with knowing that programs can change incrementally by adopting demonstration projects or by large shifts in values and priorities. The success of the evaluation depends in part not only on how well programs are structured and organized and their social and community contexts but also on the sophistication and elucidation of theories of change; indeed, a full understanding of *program theory* is thought by a number of evaluators (e.g., Chen, 1990; Donaldson, 2007; Funnell & Rogers, 2011) to be necessary for evaluations to be successful. Increasingly, programs theories serve as the foundation for logic models.

Addressing Value in Evaluation

No overview of evaluation is complete without discussing the definition and role of the concept of *value* in evaluation theory and practice. Values are the underpinning for claims about what constitutes

merit or worth. Consistent with the definitions of merit and worth, something can hold intrinsic value or extrinsic value. Schwandt (2005, p. 443) further categorized values as aesthetic, moral, or effective:

Consider, for example, an evaluation of a recycling center in a local community. Whether it is good (has value) would most certainly involve judging instrumental value [i.e., whether it was a means to an end]; it might also involve a judgment of moral value in considering whether workers involved with the technology are operating in a safe environment; and it could well involve aesthetic judgment in determining whether the center is in some sense an intrusion on the surrounding landscape.

Disputes stemming from long-standing philosophical disagreements, such as those about positivism, have focused on the distinction between facts and values. Scriven (2005, p. 236) firmly stated,

We can in fact infer validly from factual premises to evaluative conclusions by using definitions that bridge the gap: Because they are definitions, they do not count as value premises. The simplest cases are those in which propositions unpacking the meaning of “a good (or bad) X”—for example, “a good watch,” combined with a number of facts about the performance of a particular watch—fully justify the conclusion that this is a good or bad watch.

Thus, a watch has intrinsic characteristics that specify the extent to which value can be ascribed.

Scriven continued by stating that various *premises*, such as that a program addresses an important need or societal problem, can serve as the basis for ascribing value if they are validated by the appropriate research. Premises, he stated, “can be directly validated in commonsense ways”, an approach that assumes that “we consider the assumptions of practical life to be sensible ones” that are not “highly contested assumptions of an ethical or political kind” (p. 236). House and Howe (1999, pp. 7–8) posited,

Whether a statement is true and objective is determined by the procedures established by professional evaluators according to the rules and concepts of their profession. Some human judgments are involved, constructed according to criteria of the institution, but their human origin does not necessarily make them objectively untrue...

Evaluative statements can be objective in the sense that we can present evidence for their truth or falsity, just as for other statements.

Similarly, Davidson (2005, p. 95) suggested,

The values on which a solid evaluation is based are defensible insofar as there is sufficiently widespread agreement within the relevant context about those values that they can reasonably be treated as givens... We must remember [that] we are not usually (if ever) looking for 100% certainty in our conclusions; rather, we are seeking just enough to meet the requirements for certainty in the relevant decision-making context.

Disagreements about the sources of values in evaluations have long existed in the evaluation literature. The most common source of value since the earliest years of systematic evaluation in the 1930s (Tyler, 1991) has been program goals and objectives. Scriven, Davidson, and others have touted needs, identified in rigorous needs assessments, as the source of values. Others, such as Stake, believe that there is no one source of value and that value lies in the eyes of the reader. House and Howe (1999) based values on stakeholder claims that evaluators have confirmed through careful examination. Evaluators can examine and validate those stakeholders’ values identified when preparing or conducting an evaluation.

Others have critiqued each of these sources. Mark, Henry, and Julnes (2000) pointed out issues about using program goals, including that they (a) are often incomplete; (b) might not address the actual causes of problems; (c) might differ from the actual, privately held intentions of program personnel; or (d) might have been set too ambitiously to help obtain funding or set too low to ensure success in evaluations. Not infrequently, objectives are not sufficiently well developed because they are written hastily into proposals or are promised without sufficient consideration when state or local government officials receive mandated federal funds to conduct programs, thus adding to their already over-busy schedules. Mark, Henry, and Julnes also argued that needs are a troublesome source of values because they are difficult to define and select, tend to emerge as programs develop and are implemented, and are difficult to rank by priority. Furthermore, they stated that stakeholder input can be problematic because wants are not often differentiated from needs and because stakeholder group representation is not sufficiently wide.

How Evaluation Differs from Research

Discussions have long occurred about the differences between evaluation and research. The discussions tend to revolve around several themes about

the nature of research versus evaluation. One theme has to do with the breadth of evaluands considered in the discussion. Some, such as Scriven, define evaluation broadly and, assuming the discussion is about rigorous inquiry (as opposed to, say, movie reviews), distinguish between research activities and skilled judgment that, say, occurs in some events in the Olympic Games. Others (indeed, most commentators in the evaluation literature) think of evaluation strictly as program evaluation and find few differences between research and evaluation, except in light of differing purposes.

A second theme is about the use and effects of evaluation findings, expressed in terms of the widely touted notion that evaluations are about making decisions and research is about simply arriving at conclusions. This notion ties evaluations into the effects of producing findings and limits research to simply producing findings. Evaluation findings might be ignored for a variety of reasons, however (e.g., if an evaluation is conducted for symbolic uses or if an evaluation's audience disagrees with its findings). Furthermore, the findings of action research are intended to have immediate effects, such as addressing social problems.

A third theme is that research findings are intended to generalize but evaluation findings are not. This perception has some basis in practice, yet findings are said to generalize to similar programs and settings (Cronbach et al., 1980; Stake & Turnbull, 1982), and evaluation findings can contribute to theory development, whereas some research findings, such as ecological studies of the Galapagos Islands, do not generalize (Mathison, 2008). Thus, the decision-conclusion dichotomy has a stronger basis on paper than in reality. The caricature of researchers asking "What's so?" or "Why so?" and evaluators asking, "So what?" often does not hold.

A fourth theme is that evaluation addresses value and research does not. Research need not address the merit or worth of an object of study, whereas evaluation, by definition, does. However, much research involves comparisons between choices, with conclusions about which choice is better, and some evaluations, such as the descriptive studies espoused by Stake (2004), do not conclude with statements about winners and losers.

A fifth theme is that research and evaluation are not different methodologically because evaluation borrows from the panoply of research methods. This is indeed the case, but evaluation includes methods developed outside of the realm of social science

research, such as needs assessments, the success-case method, and the modus operandi method.

Because evaluation necessarily examines issues like needs, costs, ethicality, feasibility, and justifiability, evaluators employ a much broader spectrum of evidentiary strategies than do the social sciences. In addition to all the means for accessing or creating knowledge used by the social sciences, evaluators are likely to borrow from other disciplines such as jurisprudence, journalism, arts, philosophy, accounting, and ethics. (Mathison, 2008, p. 192)

Ways in which evaluation differs most starkly from social science research have less to do with academic or scientific differences and more to do with the real-world contexts in which the majority of evaluations are conducted. Many evaluators often need to be methodological generalists; many researchers are more likely to know the nuances of a set of methods than are evaluators. Research funding usually comes in the form of grants, whereas evaluation funding comes in the form of contracts. This often gives researchers much more autonomy than evaluators; for example, researchers set hypotheses, but evaluators alone do not establish evaluation questions, which come from many sources. This is a result of the attention that evaluation must give to a range of stakeholders, a feature absent from most research. Evaluators have clients and usually have intended users; researchers might have no sense of who will use their findings except to hope that their colleagues in their field will attend to them.

Essentially, evaluations occur within the political arena. This aspect manifests itself in several ways. First, most program evaluations are conducted to provide findings to those in positions of power, typically in government or foundations. Program personnel might lead evaluators to ignore evaluation questions about goals or program strategies that would result in negative findings. Evaluators who are intent on focusing on serving the intended users of evaluations might particularly be subject to the undue effects of serving the client (Alkin, 1990). Evaluation studies can be used to further the power of those in charge; as pointed out in our discussion about the social roles of evaluation, commentators such as House (1980) urge that evaluations enhance democratic processes by ensuring that the powerless are well-represented.

Fundamental Issues in Evaluation

The multifaceted nature of evaluative activity, complete with the influence of politics, effects of

context, existence and promotion of multiple models and approaches, and use of many methods results in a number of fundamental issues about the theory, methods, practice, and profession of evaluation that have been addressed over the years. Fundamental issues in evaluation are

those underlying concerns, problems, or choices that continually resurface in different guises throughout our evaluation work... They are periodically encountered, struggled with, and resolved reflecting contemporary values, technical considerations, political forces, professional concerns, emerging technologies, and available resources. There can never be a final, "once and for all" resolution to a fundamental issue. The resolution of such issues is often a point of contention, disagreement, and debate as the profession struggles to shed old ways of dealing with the issue and adopt a newer, more effective position. (Smith, 2008, pp. 2, 4).

Unlike social science research practices that have historically largely occurred independently of political, organizational, and social forces in Western nations, fundamental issues about evaluation have emerged and reemerged repeatedly in the professional discussions and in program evaluation literature since the 1960s. Some examples of recurring topics that Smith (2008) listed have to do with the purpose of evaluation, its social role, the type of evidence that is considered acceptable for evaluation claims and the methods for arriving at understanding quality, the best way to involve stakeholders, and the most effective approaches to ensuring high-quality evaluation practice. The disagreements among evaluations about topics such as these are apparent in the preceding sections of this chapter. We discuss two in greater depth here.

Perhaps the most vivid examples of disagreements about fundamental aspects of evaluation have to do with the choice of method to employ in evaluations, with the qualitative-quantitative debate exemplifying this most strongly over the years. As discussed earlier, the dispute was grounded in conflicting deeply held assumptions about inquiry, with quantitative evaluators resting their work on deduction and independence, "implying that expectations about program effects are based on theory set up before an evaluation begins" (Greene & Henry, 2005, p. 346). For these evaluators, theories explain observations, with evaluators having "a sense of distance from the program so that evaluators can make judgments on the basis of evidence without contamination" (Greene & Henry, p. 346). In contrast,

for qualitative evaluators, "[u]nderstanding behavior doesn't come from testing hypotheses but by capturing the meanings constructed by various participants" (Greene & Henry, p. 347). The détente between the two camps suggests that this issue has been resolved but "tensions persist, and skirmishes occur from time to time" (Greene & Henry, p. 350). Indeed, the current methodological disagreements on the role of experiments and quasi-experiments in evaluation, which Scriven (2008) called the "causal wars," is sometimes considered an extension of the quantitative-qualitative debate. The push for "scientifically based evidence" by federal education officials and the depiction by some of experiments as the "gold standard" for research and evaluation has resulted in strong rebuttals by many evaluators. The AEA (2003) issued a statement that randomized control trials are not the only types of studies efficacious for understanding causality, sometimes are not the best for examining causality, sometimes are unethical, and, on occasion, are inappropriate because data sources are too limited. The disagreement has been discussed widely in articles (e.g., Cook, Scriven, Coryn, & Evergreen, 2010) books (e.g., Chen, Donaldson, & Mark, 2001; Donaldson, Christie, & Mark, 2009), and formal debates (Donaldson & Christie, 2005). Often, however, evaluators overlook that disputes about methods actually reflect disagreements about the purpose of evaluation. Part of the controversy over touting experiments as the gold standard is due to disagreements over the purposes of evaluation; many evaluators argue forcefully that evaluation funding should not be targeted largely on examining the effects of evaluations. As Greene (1994) aptly pointed out, what distinguishes one methodology from another is not the methods but "rather whose questions are addressed and which values are promoted" (p. 533).

Another vivid example of disagreement about fundamental issues has to do with the purpose and social role of evaluations. In the early 1990s, Fetterman introduced the empowerment evaluation approach, which promised to conduct highly participatory evaluations (essentially self-evaluations of programs) in a manner that empowered program personnel's self-determination in their communities and enhanced their ability to conduct future evaluations (Fetterman, 1994; Fetterman, Kaftarian, & Wandersman, 1996; Fetterman & Wandersman, 2005). The approach gained wide traction, and numerous conference presentations and articles were written describing empowerment evaluations; a

Wikipedia entry on the topic claims that it “is a global phenomenon.” Others quickly responded to the expanded definition and its emancipatory focus. Stufflebeam (1994) stated that evaluation should focus on merit and worth; Patton (1997) stated that it should be limited to uses for social or political liberation; Scriven considered it false, amateur evaluation; and Smith (2007) labeled it an ideology, stating, “We need to examine not only its effectiveness in actual studies but its worthiness as a political agenda” (p. 177). This debate has quieted in public fora but with little resolution. The difference between empowerment evaluations espousing self-determination as an intended effect and more traditional approaches that focus on merit, worth, significance, and even the use of evaluation findings is stark and perhaps irreconcilable.

Research on Evaluation

It might be expected that the foundation for the methods, practice, and theory of evaluation—a discipline and profession that arrives at conclusions empirically—is based firmly on empirical findings. However, as Shadish et al. (1991, p. 478) stated,

Evaluators take for granted that the data they generate about social programs provide theoretical insights and help illuminate theoretical debates about social programs. Too often, they forget to apply this principle reflexively to their theories of evaluation.

Consequently, for decades, many in the evaluation literature have called for conducting empirical research on evaluation. Worthen (1990) stated that these calls began in the early 1970s with Stufflebeam, Worthen, Sanders, and others. Smith (1993), Cousins and Earl (1995), Alkin (2003), and Henry and Mark (2003) reiterated the calls for research on evaluation. Smith (1993) advocated conducting research on evaluation as a means of knowing how evaluators operationalize evaluation models, thereby helping to develop descriptive theories of practice that give guidance about which practices are viable in which organizational and evaluation contexts. Mark (2008) stated that the results of such research might help improve the terms of debate by adding evidence to rhetoric, documenting our understanding of evaluation’s contributions, facilitating appropriate claims about what evaluation can do, stimulating efforts to improve evaluation practice, increasing professionalism among evaluators by making it clear that evaluation is worthy of study, and moving evaluators past standards and guiding principles to a more empirically

supported understanding. He developed a typology of four categories of research on evaluation, including evaluation context, activities, consequences, and professional issues, and suggested that four inquiry modes that address description, classification, causal analysis, and values inquiry (Mark et al., 2000) could be used to classify the kinds of research that is done. Stufflebeam and Shinkfield (2007) suggested a number of topics that might be examined empirically, including (a) the degree to which evaluation standards have enhanced the quality of evaluations, (b) the effects of stakeholder involvement on evaluation use, (c) the financial and temporal costs of stakeholder involvement, (d) the effects of collaborative approaches to evaluation on program effectiveness and the use of evaluation findings, (e) the deleterious effects of politics on evaluations, (f) the degree to which and manner in which program theory and logic models improve the programs and their evaluations, (g) the effects of needs assessment on conclusions about programs, and (h) the effects of evaluating program implementation on program functioning, as well as a number of hypotheses about methodological issues. Some other topics include the degree to which evaluations should go beyond examining merit and worth and include the identification of causes and mediators, how strong the evidence for evaluation conclusions should be, and the degree to which issues of feasibility affect the breadth and depth of evaluation studies.

With a few exceptions, substantial efforts have not been made to fund research on evaluation. With federal funding, several years of work on the topic were conducted by the Northwest Regional Educational Laboratory and at the University of California at Los Angeles in the late 1970s and early 1980s. The National Science Foundation funded the Evaluative Research and Evaluation Capacity Building program in the early years of the first decade of the century and currently funds the Promoting Research on Innovative Methodologies for Evaluation program. Other work has largely been comprised of ad hoc studies, with the widespread perception that these studies have been few. In addition to a lack of funding support, King and Stevahn (2013) suggested that little research has been conducted because of the relative newness of the discipline, the lack of agreement on what evaluation means and how it should be conducted, and the practical focus of evaluation that inhibits time and funds for theory building.

An alternative case about the breadth and depth of research on evaluation can be made, however.

Evaluators often report how they conducted and what they accomplished in an evaluation, and some conduct small add-on studies. For example, they might report trying out a novel way of conducting, say, a theory-driven evaluation or a new way of approaching culturally relevant evaluation, including findings on how well the method or approach worked. They publish instruments that they have developed and validated within studies. Furthermore, reviews of the research on evaluation are a growing source of empirical findings about evaluation. From 2004 to the present, ten have been published in the *American Journal of Evaluation* (*AJE*) alone (Brandon & Singh, 2009; Chouinard & Cousins, 2009; Christie & Fleischer, 2010; Coryn, Noakes, Westine, & Schröter, 2011; Johnson et al., 2009; Labin, Duffy, Meyers, Wandersman, & Lesesne, 2012; Miller & Campbell, 2006; Peck, Kim, & Lucio, 2012; Ross, Barkauoi, & Scott, 2007; Trevisan, 2007). Summarizing the reviews of the research on evaluation conducted during a period of about thirty years, King and Stevahn (2013, p. 60) concluded, “Even as we acknowledge the relative limitations of the existing research base, taken together these syntheses provide direction for program evaluators who want their use-oriented practice to be evidence-based.” Furthermore, researchers have continued to examine experimental methods, albeit usually in publications outside the mainstream of evaluation journals (e.g., Cook, Shadish, & Wong, 2008).

Consistent with the all-inclusive range of evaluation methods, research on evaluation can be defined to cover a comparable range of methods. A review of 586 articles (excluding book reviews and editorial notes) published in *AJE* from 1998 through 2011 showed that 219 (37 percent) could be considered research on evaluation (Brandon, Vallin, & Philippoff, 2012). Of these, 73 percent were reports of single case studies, of evaluation instruments or methods, or of the reflections of evaluators about their work. An additional 16 percent were the reports of literature reviews, multiple-case studies, or oral histories. Only a smattering of other types of designs were found. These results suggest that the literature is replete with studies, largely descriptive, of evaluation practices and methods without manipulating variables, following cases over time, conducting simulations of evaluation scenarios, and so forth.

Elsewhere, the first author has argued that reflective narratives lack sufficient information to evaluate the methodological warrants for empirical

evidence (Brandon & Singh, 2009; Brandon & Fukunaga, 2014). However, a reasonable argument can be made that narrative accounts of the art of evaluation practice have considerable value because (a) they come from actual practice, (b) they might be more accessible to many readers because of their communicative narrative style, (c) they reflect widely accepted constructivist principles that we learn contextually and from material that is meaningful to the reader at various stages and levels of understanding, (d) they are acceptable to non-Westerners, (e) participant observation and rich description help contribute to the credibility of reports, and (f) the practical wisdom of evaluators is key to building a good theory of evaluation (Cousins & Chouinard, 2012).

Opportunities abound for drawing from other disciplines in the pursuit of answering questions of importance to evaluators engaging in research on evaluation. A thriving social psychology literature underlies the development and implementation of programs and treatments, but,

in addition to serving as a wellspring for program theory, social psychology contains a wealth of research and theory that are relevant to the challenges that arise in the practice of evaluation. Examples of practice challenges of evaluation include questions about how to guide interactions among stakeholders who vary in power and in their views and interests; gather information about stakeholders' views about such matters as a relative importance of various possible program outcomes; seek consensus across stakeholders with different interests; develop and maintain trust with stakeholders, while also eliciting continued participation; alleviate anxiety about the evaluation; maintain compliance with data collection protocols; make sure that evaluation procedures address cross-cultural issues and meet cultural competency standards; measure behaviors that take place repeatedly over time; make sense of the mixed patterns of results, whereby a program that does well on one outcome does poorly on another; and facilitate the use of evaluation findings. (Mark, Donaldson, & Campbell, 2011, p. 381)

The Expanding Evaluation Profession

For the past fifty years, evaluation has grown from an activity largely addressing courses and curricula (e.g., Cronbach, 1963), with experiments being the dominant design (Suchman, 1967) for evaluating programs, to a multifaceted discipline

and profession encompassing multiple evaluation models and built on an expanding and diverse theoretical base (Shadish et al., 1991). It is generating an increasing number of methods unique to evaluation (e.g., White & Phillips, 2012) and is becoming ubiquitous in public and private organizations (AEA, 2011*a*; W. K. Kellogg Foundation, 2004). Evaluation models were developed and evaluation journals were launched in the early 1970s, a few years after the federal government began funding large-scale evaluations (Madaus & Stufflebeam, 2000). An informal network of leading evaluators formed the May 12th group in the 1970s, followed by two formal organizations—the Evaluation Network and the Evaluation Research Society—that merged into AEA in 1986. As of 2013, AEA had about 8,000 members, mostly North American, with a growing international contingent. The number of evaluation professional associations is growing; there are now national and regional associations worldwide, numbering about fifty by 2000 (International Organisation for Cooperation in Evaluation, 2012). In the first decade of the century, these associations formed the International Organisation for Cooperation in Evaluation for the purpose of supporting and expanding program evaluation activities worldwide. By the early 1980s, standards for educational program evaluation had been developed and published (Joint Committee on Educational Evaluation, 1981). The Evaluation Research Society developed its own standards, as well (Evaluation Research Society Standards Committee, 1982), and AEA published its *Guiding Principles* in 1994, followed by later revisions (AEA, 2004). Others have followed with statements of independently developed competencies (King, Stevahn, Ghere, & Minnema, 2001; Russ-Eft, Bober, de la Teja, Foxon, & Koszalka, 2008). The Canadian Evaluation Society approved its voluntary Professional Designations Program in 2009, in which evaluators who meet the appropriate criteria are designated “Credentialed Evaluators.” In 2009, it was reported on the Cable News Network website that program evaluation was one of “10 little-known fields with great job opportunities” (Zupek, 2009).

Evaluation is becoming increasingly institutionalized through the vehicle of performance management and measurement (Nielsen & Hunter, 2013), as manifested in the United States by the federal Government Performance and Results Act of 1993, which was updated in 2010, and the federal Program Assessment Rating Tool that was used from 2002 to the end of the George W. Bush administration. The

number of formal graduate programs for evaluators has diminished over the years in the United States, but they have grown worldwide: on its website, the AEA shows fifty “graduate programs or certificate programs either directly in evaluation or with available concentrations in evaluation” (AEA, 2012). It is to be expected that this expansion and maturing of evaluation as a discipline and profession will continue, and it is to be hoped that the influence of evaluators in a plethora of fields will grow, as well.

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Community-Based Research: Understanding the Principles, Practices, Challenges, and Rationale

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Abstract

Community-based research challenges the traditional research paradigm by recognizing that complex social problems today must involve multiple stakeholders in the research process—not as subjects but as co-investigators and co-authors. It is an “orientation to inquiry” rather than a methodology and reflects a transdisciplinary paradigm by including academics from many different disciplines, community members, activists, and often students in all stages of the research process. Community-based research is relational research where all partners change and grow in a synergistic relationship as they work together and strategize to solve issues and problems that are defined by and meaningful to them. This chapter is an introduction to the historical roots and subdivisions within community-based research and discusses the core principles and skills useful when designing and working with community members in a collaborative, innovative, and transformative research partnership. The rationale for working within this research paradigm is discussed as well as the challenges researchers and practitioners face when conducting community-based research. As the scholarship and practice of this form of research has increased dramatically over the last twenty years, this chapter looks at both new and emerging issues as well as founding questions that continue to be debated in the contemporary discourse.

Key Words: community-based research, community-based participatory research, campus–community research partnerships, engaged scholarship, collaborative research, synergistic research community empowerment

It is best to begin, I think, by reminding you, the beginning student, that the most admirable thinkers within the scholarly community you have chosen to join do not split their work from their lives. They seem to take both too seriously to allow such disassociation.

—C. W. Mills, (1959, 195)

Community-based research challenges the traditional research paradigm by recognizing that complex social problems today must involve multiple stakeholders in the research process—not as subjects but as co-investigators and co-authors. It has roots in critical pedagogy, as well as critical and feminist theory, and is research centered on social justice

and community empowerment. Community-based research is not a methodology; it is an “orientation to inquiry” where researchers and community stakeholders collaborate to address community-identified problems and investigate meaningful and realistic solutions. Community-based research came out of a growing discontent among academics,

researchers, and practitioners with the positivist research paradigm and instead argues that research must be “value based” not “value free.” It is relational research that fosters both individual and collective transformation. Community-based research also challenges disciplinary silos and instead fosters a transdisciplinary research paradigm.

There has been a growing interest and expectation within academia and community organizations that campus–community research partnerships provide benefits and challenges. We have seen a proliferation of research partnerships, courses, workshops and trainings on how to collaborate with community partners in community-driven research projects. There has also been a substantial increase in the literature (books and articles) describing best practices providing exemplars, and discussing methodologies. Israel, Eng, Schulz, and Parker (2005) argue that within the field of public health “researchers, practitioners, community members, and funders have increasingly recognized the importance of comprehensive and participatory approaches to research and intervention” (3).

This chapter begins with a discussion of the historical roots and theoretical background to this form of inquiry and a clarification of terminology. I include a discussion of the rationale and evaluation literature that offers convincing evidence for new and experienced researchers to consider this alternative research paradigm. Building on the work of others, I discuss seven core principles of community-based research and a list of skills often useful in the practice of engaged scholarship. This chapter argues that, as community-based research continues to grow, it is important that our scholarship includes exemplars, reflection, evaluation, and a critical discussion of best practices. This chapter hopes to contribute to this discourse.

Background

I cannot think for others or without others, nor can others think for me. Even if the peoples thinking is superstitious or naïve, it is only as they rethink their assumptions in action that they can change. Producing and acting upon their own ideas—not consuming those of others. —Freire, 1970, 108

The epistemology of community-based research can be traced back to many roots—Karl Marx, John Dewey, Paulo Freire, C.W. Mills, Thomas Kuhn, and Jane Addams to name but a few. Community-based research as it is practiced today has been enriched by the diversity of thoughts, methodologies, and

practices that has been its foundation. The practice and scholarship of community-based research can be found in many disciplines: sociology, psychology, economics, philosophy, education, public health, anthropology, urban planning and development, and social work. Different historical traditions and academic disciplines have led to contemporary differences in the form or focus of engaged scholarship, but what has united many practitioners and scholars is a social justice mission and the desire for personal and structural transformation. Lykes and Mallona (2008) argue:

Critical pedagogy (Freire) and liberation theologies (Berryman, Boff, Gutierrez, Ruether, Cone) and liberation psychologies (Martin-Baro, Watts, and Serrano-Garcia, Moane) emerged within relatively similar historical moments characterized by widespread social upheavals including armed struggle and broad-based non-violent social movements. A belief that the poor could be producers of knowledge and lead the transformation to a new social reality. [114]

Today you can find community-based research pedagogy, practices and scholarship across disciplines and collaboration between disciplines including new areas such as medicine, native or aboriginal research, conflict studies, history, and archeology. The expansion of community-engaged scholarship as epistemology reflects an important paradigm shift towards understanding multiple ways of knowing and experiential learning as critical to good research practices.

While it is not possible to include an extensive summary of the history and development of community-based research here, a brief review is necessary to provide the context and rationale for this major epistemological paradigm shift across multiple disciplines. Wicks, Reason, and Bradbury (2008) identify the influence of critical theory, civil rights, feminist movements, liberationists, and critical race theory—“critiques of domination and marginalization” and “critical examination of issues of power, identity and agency” (19). The historical roots and scholars who, I believe, have most influenced the development of community-based research are critical pedagogy (Paulo Freire and John Dewey), critical theory (Karl Marx and C.W. Mills), the epistemology of knowledge (Thomas Kuhn), and feminist theory (Jane Addams).

While Marx is noted for his writing about the conditions of the working class in Europe and his theories of alienation and oppression under

capitalism, he was also an active participant in the French Revolution. According to Hall (cited in Ozerdem and Bowd, 2010) Marx was not only doing research and theorizing *about* the working classes but actively working with the workers to educate and raise consciousness. In addition to building theory, Marx and Engels sought to radically change and improve the political, economic, and social structure of society. The need to work *with* those most disadvantaged to challenge institutional inequality and power relationships is reflected in the principles of community-based research today. Many academics and scholars working from a critical theoretical perspective found a synergy with the principles and practices of community-based research.

Within education, John Dewey and Paulo Freire were reformers, activists, and key figures working to challenge traditional pedagogy and positivist research practices. Both were very influential in connecting research, theory, action, and reflection to social reform. John Dewey (1859–1952) questioned the relevance of much of what was considered “education” by asking, “How many found what they did learn so foreign to the situations of life outside the school as to give them no power or control over the latter” (cited in Noll, 2010, 8). Dewey saw educational institutions as agencies of social reform and social change through providing opportunities for learning and engagement with the world beyond the classroom. Summarizing Dewey, Peterson (2009) wrote:

Dewey believed that learning is a wholehearted affair; that is, you can't sever knowing and doing, and with cycles of action and reflection, one's greatest learning occurs. Dewey was interested in the learning that resulted from the mutual exchange between people and their environment. [542]

Dewey argued that learning—action and reflection—must take place in commune with one's environment. Learning is co-created rather than unidirectional; a challenge to the traditional view of knowledge transfer from teacher to learner. Co-education and co-learning are key principles of community-based research.

Paulo Freire (1921–1997), the founder of critical pedagogy, also challenged conventional educational pedagogy and traditional research paradigms and saw education's potential as liberation from oppression. His most famous and widely distributed book, *Pedagogy of the Oppressed* (1970), was a call to action for both teacher and student to work together for

social change and social reform. Freire saw learning as a two-way process involving “conscientization”—critical analysis and reflection leading to action. It is only through theory and practice, action and reflection, that real social change is possible. He also saw that the poor and oppressed can and *must* be leaders of their own liberation. Freire's work—in challenging pedagogy and demanding researchers and academics to work *with* and learn *from* those most oppressed—has greatly influenced the practice of community-based research today.

Sociologist C.W. Mills also influenced critical pedagogy and engaged scholarship. In his classic work *The Sociological Imagination* (1959) he wrote:

An educator must begin with what interests the individual most deeply, even if it seems altogether trivial and cheap. He must proceed in such a way and with such materials as to enable the student to gain increasingly rational insight into these concerns, and into others he will acquire in the process of his education. . . . [187], We are trying to make the society more democratic. [189]

Similar to Freire, Mills challenged the social sciences to educate and through experiential education to foster democratic citizenry. Mills saw the connection between personal troubles and public issues and the role of sociology in helping others *see* the larger structures in society and how they reinforced inequality.

Another scholar who had a major influence on the development of community-based research is Thomas Kuhn in his classic book *The Structure of Scientific Revolutions* (1996). Kuhn's work regarding the theory of the subjective nature of knowledge raised epistemological questions of “how we know what we know” and “what it is that we value as knowledge” (Wicks, Reason, & Bradbury, 2008). This became critically important in the development of engaged scholarship as academics and researchers began to respect and validate local knowledge, expertise, and other ways of thinking as equal to the knowledge and skills they could offer. Kuhn's work led to questions about the privileged position of the researcher and how this privilege has denied or denigrated the experiential knowledge and understandings of oppressed groups.

It is also important to note the influence of feminist theory, in particular Jane Addams, on the development of community-based research and scholarship. Addams (1860–1935), a social activist and sociologist, played a key role in the development of engaged scholarship and community

research. Naples (1996) writes that feminists argued for “a methodology designed to break the false separation between the subject of the research and the researcher” (160). Addams employed hundreds of women to go into their communities to interview, observe, and understand the experiences of other immigrant women in Chicago early in the twentieth century.

Addams also saw the need to make research relevant to the communities in which it originated. Much of the data gathered in Chicago was published as *Hull House Maps and Papers* (1895) and was for the benefit of the community, not for an academic audience. Her focus was social justice and social change, not theoretical conceptualizations of urban poverty. In writing about Jane Addams and the Chicago School, Deegan (1990) stated that Addams wrote “all the book’s royalties would be waived as we have little thought about the financial gain” (57). Deegan goes on to argue that Addams’ interests were in “empowering the community, the laborer, the elderly and youth, women and immigrants” (255). Addams, similar to Dewey and later Freire, was also very critical of traditional education, which reproduced inequality. Deegan (1990) writes that Addams articulated a goal of “generating reflective adults” (283).

Definitions, Terminology, and Subdivisions

We have exemplars of the methods of participatory research and canons for their practice, even if we cannot as yet agree on a single name. —Couto (2003, 69)

Clarification of terminology is necessary before beginning a discussion of the principles and skills of community-based research. Broadly defined, campus–community research collaboration can be referred to as community-based research (CBR), community-based participatory research (CBPR), collaborative research, engaged scholarship, participatory research (PR), participatory action research (PAR), action research (AR), aboriginal community research, popular education, participatory rural appraisal, public scholarship, university–community research collaboration, co-inquiry, and synergistic research. New terms and subdivisions continue to emerge. Strand, Marullo, Cutforth, Stoecker, and Donohue (2003a) suggest that practitioners of CBR come “from within and outside academia and work in areas throughout the world—all of which makes any commonly-accepted definition problematic” (6).

It is not my intent here to minimize or ignore the different historical roots or traditions reflected in the above forms of campus–community research, but a discussion of the distinct nature of each is beyond the scope of this chapter. Acknowledging that there are differences, this chapter will focus on commonalities and core principles that can apply broadly to campus–community research partnerships. Generally, the term “community-based research,” or CBR, is used here, although I have tried to include the terms used by authors when describing their own research. Other scholars have also focused on similarities rather than differences. Atalay (2010) suggests that, “regardless of the terminology used, the central tents remain the same” (419). CBR aims to connect academic researchers with individuals, groups, and community organizations to collaborate on a research project to solve community-identified and community-defined problems. CBR is intended to educate, empower, and transform at the individual, community, and structural level to challenge inequality and oppression.

While using a broad brush to be inclusive of all campus–community research partnerships, it is important to address what I see as two important differences in the goals and outcomes within CBR. For many practitioners, the ideal is a long-term, collaborative, and egalitarian partnership that builds community, fosters transformation, and promotes social change. Academics conduct research with and for the community, and all participants teach and learn in a synergistic relationship. Clayton, Bringle, Senor, Huq, and Morrison (2010) argue that campus–community relationships can be short term (transactional) or, ideally, a partnership in which both parties grow and change because of a deeper and more sustained (transformative) relationship.

For others, (e.g., McNaughton & Rock, 2004; Nygreen, 2009–2010) the relationship between academic researchers, the university, and the community is always contentious, and power is rarely equal. For this reason, some CBR practitioners advocate community members learn the skills and knowledge necessary to conduct their own research within their communities. Nyden, Figbert, Shibley, and Burrows (1997) write, “Participatory Action Research aims at empowering the community by giving it the tools to do its own research and not to be beholden to universities or university professors to complete the work” (17). Academic researchers within this tradition are looking to empower local communities to

be researchers and authors of their own transformation. The goal is to foster self-determination and self-reliance of the disenfranchised and powerless so they can be self-sufficient (Park, 1993).

From this perspective, a long-term or sustained partnership with academic researchers could be seen as exploitive and disempowering.

Another major difference is that, for many, the goal of CBR includes pedagogy (Strand, 2000). CBR provides an opportunity to involve students in a research project with community partners, often as part of their curriculum requirement. Strand, Marullo, Cutforth, Stoecker, and Donohue (2003b, xxi) suggest CBR is a way to “unite the three traditional academic missions of teaching, research and service in innovative ways.” CBR as pedagogy can bring students together with faculty and community partners to address community problems, as well as learn valuable skills regarding democratic research processes, communication, and civic responsibility. Porpora (1999, 121) considers CBR “the highest state of service learning” and important as a way to promote engaged citizenship among students. There is an extensive body of research discussing the benefits, challenges, and practice of CBR as pedagogy that has generally found substantial benefits to students.

What is meant by “community” within the term community-based research requires some clarification. Alinsky (1971, 120) noted that “in a highly mobile, urbanized society the word ‘community’ means community of interest, *not* geographic location.” This suggests a collective identity with shared goals, issues, or problems, or a shared fate (Israel, Eng, Schultz & Parker, 2005). This has been particularly evident in the growing number of international community–researcher collaborative partnerships. Pinto et al (2007) writes:

International researchers need to become members, even if from afar, of the communities that host their studies, so that they can be part of the interactions that affect social processes and people’s understanding of their behaviors and identities. These interactions may occur at physical, psycho-social and electronic levels, encompassing geographic and virtual spaces and behaviors, social and cultural trends, and psychological constructs and interpretations. [55]

Accepting that today individuals and groups can participate in numerous “communities of interest” at the local and global level, many exemplars of CBR are situated in geographically defined communities. The community, however, is rarely a unified

or homogenous group. It often includes groups within groups, competing and contentious factions, and members with diverse perspectives, needs and expectations (Atalay, 2010). The diversity of participants within CBR projects reflects both the strengths and the challenges of engaged scholarship and will be discussed later in this chapter.

A final clarification with regards to CBR is that it is not the same as community organizing or advocacy. CBR includes scientific investigation respecting research ethics, methodologies, and analysis. CBR practitioners and community partners are seeking knowledge and understanding through data collection and analysis. The findings will inform decisions as to community organizing, social action, or advocacy work. Fuentes (2009–2010, 733) makes the distinction between “*community organizing*,” which usually focuses on the development and support of leaders and “*organizing community*,” which “centers on community building, collectivism, caring, mutual respect, and self-transformation.” CBR is about organizing community to create research partnerships to address inequalities. Another misconception is that CBR is a form of public service. Public service implies a one-way transfer of knowledge, expertise, and action from the campus to the community. CBR is a multi-directional process that results in shared and collaborative teaching, learning, action, reflection, and transformation.

Rationale

We both know some things, neither of us knows everything. Working together, we will both know more, and we will both learn more about how to know. —Maguire (1987 37–38)

There is universal agreement that research is critical in terms of planning, implementing, and evaluating policies and programs. Nyden and Wiewel (1992, 44) state, “research is a political resource that can be used as ammunition” to provide credible evidence regarding funding, programs, and or policy decisions. So why do CBR? For engaged scholars and activist working within a CBR paradigm, the reasons for doing so are numerous—personal and structural transformation, co-education, community empowerment, capacity building, and a belief in the need to democratize the research process. Even though engaged scholarship has not always been given the support and resources needed within academia, many argue that it is the *only* type of research that really makes a difference. Reason and Bradbury (2008) assert “indeed we might respond

to the disdainful attitude of mainstream social scientists to our work that action research practices have changed the world in far more positive ways than conventional social science” (3). Rahman (2008) in summarizing the early work of Budd Hall in the 1970s states, “Participatory Action Research is a *more* scientific method of research because the full participation of the community in the research process facilitates a more accurate and authentic analysis of social reality” (51).

For many engaged scholars, ethical research requires working *with* and *for* individuals and groups, not doing research *on* or *about* subjects. Collaboration with multiple stakeholders allows for an opportunity to re-conceptualize problems and come up with innovative solutions. For many, this form of research is “more than creating knowledge; in the process it is educational, transformative and mobilization for action” (Gaventa; 1993; xiv–xv). Community-based researchers acknowledge that this form of inquiry is not the only way, but often it is the best way to address the magnitude and complexity of contemporary social programs. It requires researchers across disciplines and from multiple perspectives, together with activists and community members, to join as equal partners and to think about and strategize solutions that are meaningful and beneficial to them. The benefits of combining scientific methods and lived experiences to re-conceptualize problems and find solutions are clear. Involving community stakeholders in all stages of the research process also increases the chances that solutions will be relevant and meaningful to community members. CBR is ideally situated to inform best practices as it is research generated from the ground up.

For more traditional social scientists, the reasons for considering CBR may reflect pressure from outside funders or community members. There has been a growing frustration with traditional research that the findings have not been applied or benefited the community or broader society. Nyden, Figert, Shibley, and Burrows (1997, 3) state, “Traditional academic research has focused on furthering sociological theory and research” and not social action or social justice. Forty years ago, Fritz and Plog saw traditional research methods as no longer viable within archeology, stating:

We suspect that unless archaeologists find ways to make their research increasingly relevant to the modern world, the modern world will find itself increasingly capable of getting along without archaeologists. [Cited in Atalay, 2010, 419].

This concern has been raised within other disciplines and is reflected in the development of CBR and scholarship.

There are also very good reasons for institutions of higher education to align their mission to reflect a commitment to serve. Boyer (1994) suggests that the historical roots of higher education as a service to the community and a “public good” have diminished. He argues for the “New American College”—an institution that celebrates and fosters action, theory, practice, and reflection among faculty, students, and practitioners to solve the very real problems facing communities today. Colleges and universities must respond to and engage with communities to listen, learn, and work together on solutions. Netshandama (2010) describes how the University of Venda in South Africa changed over the course of four years to “align its vision and mission to the needs of the community at local, regional, national, continental and international levels” (72). Netshandama (2010) argues that the university did not just support faculty or add resources; their vision was to “integrate community engagement into the core business of the university” (72).

Methodology and a Transdisciplinary Paradigm

CBR is not a research methodology. Researchers and community members use a variety of methods to gather data about a community issue or problem and then seek solutions. It reflects a radical paradigm shift away from positivist methods of inquiry to what Leavy (2011) refers to as “a holistic, synergistic, and highly collaborative approach to research” (83). It can be best understood as a “*philosophy of inquiry*” (Cockerill, Meyers, & Allman, 2000) or an “*orientation to inquiry*” (Reason & Bradbury, 2008) that seeks to create participative communities of inquiry to collaborate to address community problems. Practitioners of CBR recognize and value multiple ways of knowing and do not privilege the knowledge or skills of the researcher over local experiences, skills, and methodologies. Torre and Fine (2011) suggest that PAR “represents a practice of research, a theory of method and an epistemology that values the intimate, painful and often shamed knowledge held by those who have most endured social injustice” (116). At its best, CBR reflects a democratization of the research process and a validation of multiple forms of knowledge, expertise, and methodologies. It is a shift away from research “subjects” to research collaborators and colleagues.

Although CBR is not a methodology, it does address the recent methodological questions concerning the role of “reflexivity” in research design and practice. Subramaniam (2009) states, “After adopting reflexivity as a valid research process, the researcher must make decisions about *her status* vis-à-vis those being researched and become conscious about *their status* in relation to her, the researcher” (203). This has led to further methodological questions concerning the validity of traditional binaries such as “researcher/researched,” “insider/outsider,” and “objective/subjective.” These statuses are addressed openly and critically in CBR projects. For example, critical psychologists often face an ethical dilemma when involved in CBR projects. Baumann, Rodríguez, and Parra-Cardona (2011, 142) refer to this dilemma, citing the American Psychological Association (APA) Code of Ethics that states psychologists must refrain from “multiple and dual relationships with clients and community members.” For CBR practitioners, research is relational. Scientific “objectivity” is problematic and does not strengthen the validity of research outcomes.

CBR lends itself to mixed method design and often reflects a transdisciplinary research paradigm. According to Leavy (2011), “Transdisciplinarity is a social justice oriented approach to research in which resources and expertise from multiple disciplines are integrated in order to holistically address a real-world issue or problem” (35). Leavy argues that “transdisciplinarity does not mean the abandonment of disciplines (34)” but rather knowledge gained through this form of inquiry transcends traditional disciplinary silos. I would agree that CBR reflects a “transdisciplinary research paradigm” and that this also includes community scholars outside academia.

Although data can result from many methods, there are core principles or tenets of CBR that are generally agreed upon by most practitioners. Scholars do disagree on the number of core principles. However, the unique nature of every CBR project allows for flexibility and differences. The principles represent guidelines or best practices, and are helpful for setting goals and for praxis,—continuous reflection, and action. They are also interconnected and interdependent. Each principle can be conceptualized along a continuum. For example, Schwartz (2010) suggests that PAR can include research that has minimal collaboration to projects that have full participation of all stakeholders in every stage of the research process with most projects falling somewhere in the middle.

Principles of Community-Based Research

Strand, Marullo, Cutforth, Stoecker, and Donohue (2003) suggested three core principles that define CBR: collaboration, democratization, and social action for social change and social justice. Atalay (2010) expands on these three and suggests five core principles of CBR: community driven, participatory, reciprocal, power sharing, and action oriented. As the number of community-based researchers, practitioners, projects, and disciplines involved has multiplied and the scholarship of CBR has increased, so have the number of core principles. Leavy (2011) suggests seven principles: collaboration; cultural sensitivity, social action and social justice; recruitment and retention; building trust and rapport; multiplicity and different knowledges, participation and empowerment; flexibility and innovation; and representation and dissemination. Still other practitioners have identified nine (Puma, Bennett, Cutforth, & Tombari, 2009; Israel, Eng, Schultz, & Parker, 2005).

An understanding of the core principles that define CBR is important, but how each principle is negotiated and understood will reflect contextual, social, and historical differences within each project. Synthesizing and building on the work of others, I discuss seven principles of CBR that I believe represent best practices within this orientation to inquiry: collaboration, community driven, power sharing, a social action and social justice orientation, capacity building, transformative, and innovative. Summaries of CBR projects are also provided as brief case studies. They are intended to reflect the challenges and benefits of this work and how the principles of CBR are negotiated and reflected in unique ways.

Collaboration

Collaboration between the researcher and community is a fundamental principle of CBR. It is defined as working in partnership with all stakeholders to identify, understand, and solve real problems facing their community. Collaboration happens in all stages of the research process—including problem definition, methodological decisions, data collection and analysis, dissemination of the findings, and evaluation of the project. Collaboration between the researcher and the researched is a fundamental paradigm shift from the traditional scientific method. Within CBR, the distinction between the researcher and the researched is no longer valid or acceptable. This does not remove differences

between stakeholders or between community members and researchers but rather recognizes and validates different ways of knowing, experiences, skills, and methods equally. Mandell (2010) states:

Ultimately, what the activist sociologist has to offer social change organizations is her or his detachment from the immersion in the work, grounding in social change theoretical perspectives and the power to ask questions and to make outside observations. The outsider perspective of an action researcher with the insider views of community partners makes for a powerful combination. [154]

To collaborate with community members it is critical that the project is transparent and inclusive of all stakeholders. It is a reflective process that continues throughout the project and is based on trust, respect, and equality between all participants. Mandell (2010) states that a “successful trust filled researcher-community partnership is built over time, through rigorous self-examination and regular communication” (154). Trust can often be fostered by researchers participating in additional community events and activities and by attending celebrations that are not directly related to the research project. Listening to and supporting participants’ own professional and personal goals also fosters trust and builds collaboration (Baumann, Rodriguez, & Parra-Cardona, 2011).

To foster collaboration, the researcher needs to understand some basic principles of group processes and group dynamics. CBR success depends on participatory democracy and open communication between members. This facilitates understanding and enables all members to share their strengths and skills, to set priorities, and to accomplish tasks. However, inclusivity and collaboration with multiple stakeholders can lead to questions about project size. Generally, large projects with multiple stakeholders can lead to hierarchies in decision making and discussion and may leave some voices silenced. Small projects with few members can lead to concerns about burnout and/or reinforcing power inequality within the community. There is no ideal size for maximum collaboration. Each project will need to negotiate and reflect upon collaboration and inclusivity in an ongoing dialogue or “multilogue” with the community. Sometimes community education about what CBR is may be necessary before collaboration is possible. This can add months or years to the expected timeline and may alter the original CBR project.

Case Study: A CBPR Project in Catalhoyuk, Turkey

Atalay (2010) was involved in an archeological excavation site in Catalhoyuk, Turkey, and wanted to include the community in a CBPR project. She stated that her first priority was to “[d]etermine if the community was interested in becoming a research partner, and what their level of commitment was. This required substantial up-front investment both to explain CBPR and to demonstrate how their role as collaborators would differ from their previous role as excavation labor or ethnographic informants” (422). In conducting interviews with local residents to invite collaboration, individuals felt they could not contribute to the research partnership until they received “archeology-based knowledge.” Atalay found that “contrary to what I had initially expected, the first several years of the project focused on community education rather than on developing and carrying out an archeology, heritage management or cultural tourism-related research design” (423).

The CBPR project started with archeology education that resulted in “an annual festival, archaeological lab-guide training for village children and young teen residents, a regular comic series (for children), and a newsletter (for adults)” (423). After some time, Atalay began moving the community towards a research partnership. The CBPR project initiated a local internship program and archeological theatre. Both were community-led and community-driven projects that fostered capacity-building and recognized the importance of local knowledge and experiences. Atalay acknowledged that the work was slow and did not take the direction she had initially intended. However, she argues that “collaborative research with communities in a participatory way offers a sustainable model, and one that enhances the way archeology will be practiced in the next century” (427).

This CBPR project illustrates that collaboration is only possible when partners are not only seen as equal by the researcher but when they experience it themselves. Freire (1970) reminds us we must always begin where the community is: “All work done for the masses must start from their needs and not from the desire of any individual, however well intentioned” (94). Atalay’s work also reflects the challenges and benefits of collaborative research partnerships. Problems and solutions are identified by the community and it is the community that is the primary beneficiary of the research project.

Community Driven

Classic social science research focused on social problems that the researcher and the academic community defined as important or worthy of study. Generally, a research project was initiated and controlled by the researcher. It was the researcher who benefited and subjects were often treated as objects. CBR was a response by engaged scholars and practitioners to end exploitive and oppressive research practices that left community problems intact, inequality unchallenged, and often community members feeling used. Ideally, community-based projects should be community driven from conception to dissemination of the findings and evaluation of the project. Comstock and Fox (1993) suggest that local communities and workplace groups should decide on the nature of the problem and participate in the investigation of local and extra-local forces sharing their lives. Collectively they may decide to take action based on the research findings.

However, Maguire (1987) suggest that “realistically, such projects are often initiated by outside researchers” (43). If many CBR projects do not originate within the community, how can practitioners and researchers foster *community-driven* projects? Whether the community is local or global, participants in CBR projects will often have conflicting interests, sentiments, expectations, and priorities. To be inclusive and have all stakeholders as participants in the research project means tension, conflict, and challenges are inevitable. Bowd, Ozerdem and Kassa (2010) remind us that:

Participation literature is also criticized for ‘essentializing’ the word community as a homogeneous entity where people have egalitarian interests to produce knowledge, work with partners and decide on matters of common good in undisputed manners. In reality however, communities are characterized by protracted ethnic, linguistic and professional cliques and interest groups. [6]

Engaged scholars and practitioners need practice, patience, skills, and knowledge to ensure all stakeholders are heard and encouraged to participate. Democratization of the research process requires participatory democracy within the community, and this cannot be expected or assumed.

It is also important to ask *who* speaks for the community. For example, community-based researchers and practitioners have been heavily criticized for not paying close attention to the exclusion of and silencing of women within many CBR

projects—the continuing “androcentric paradigm” of social science research methods (Maguire, 1987; Decker, 2010). Maguire (1987) writes, “Women are often invisible, submerged or hidden in case study reports or theoretical discussions. Gender is rendered indistinguishable by generic terms like ‘the oppressed,’ ‘the people,’ ‘the villagers,’ and ‘the community’” (48). The challenge of CBR is that often the most oppressed within the community lack any organizational structure or resources to participate in research projects. It is critical for engaged scholars and practitioners to be conscious of *who* is participating in, excluded from, or silenced in CBR projects and take responsibility for encouraging and supporting the most disenfranchised to participate equally. It is often the researcher or “outsider” who is best situated to *see* who is excluded and what must be done to rectify this.

Power Sharing

Knowledge, discussion, and reflection about power, power sharing, and power dynamics within the community are critical for successful partnerships. Engaged scholars and activists need to encourage, support, and foster a climate where all stakeholders and researchers share power. This can be difficult when researchers often have privileged statuses that can intimidate or silence community partners. For the researcher it is often difficult to cede power and control to community members who may have less formal education or training in research methods or less knowledge of the larger issue. However, Mdee (2010) address this problem in her PRA project in Tanzania and argues: “absolute equality in the process is an impossibility given imbalances in knowledge, power and resources, and it is not helpful to pretend otherwise” power sharing is necessary and fundamental to CBR partnerships. Shared decision making includes problem definition, methodological concerns, analysis and dissemination of the findings, funding and budgetary decisions, where and when to hold meetings, as well as ethical questions such as whether to pay participants. While community-based researchers and practitioners may believe in the principle of power sharing, they may be unaware of their privileged status that continues to influence and inhibit collaboration.

Case Study: Youth Empowerment at an Alternative High School

Nygreen (2009–2010) discusses the challenges and dilemmas of a PAR project she undertook with

recent graduates and current students in an alternative high school to “examine issues of social and educational inequality” (17). Nygreen found that, over the course of the two-year project, there was high turnover of student participation, several group conflicts, and although the youths said they learned a great deal, she saw little evidence of social change. Through reflection it became clearer that wanting and believing in equitable partnerships is not the same as achieving it. She found that, in working with youth on issues of social justice, understanding power dynamics was important. She said, “I insisted that we all had an equal voice in decision-making and we were all accountable to each other. In reality, though, my posture reflected a false egalitarianism that obscured and reinforced real power differences. Despite my promises that the youth could veto decisions they did not like, I was the only member of the group with absolute veto power.” (18)

Nygreen acknowledges that PAR in and of itself does not necessarily negate the problems related to power inequality. Although PAR seeks to equalize power between participants, “in practice PAR projects may quite easily *reproduce* and *exacerbate* power inequalities while obscuring these processes through a discourse of false egalitarianism (19).” She explains, “I conflated the political and ethical values of PAR with the practice and process of PAR. What I learned, instead, is that no series of methodological steps can protect a social scientist from the dilemmas of power, authorship, and scale” (28). She advocates a “de-coupling” of the method of PAR from the political and ethical values that inform it. This PAR project highlights the critical tensions she experienced between the *values* of PAR and the *practice* of PAR. Nygreen identified the dilemmas of power and privilege—including white privilege when university-based researchers work with historically oppressed communities—and reminds us that critical reflection through dialogue and the complexities of power relations must be understood.

Although much of the research concerning power within CBR projects has focused on the imbalance between the researcher and the community, we must understand the multifaceted and fluid nature of power as it is negotiated and experienced within communities. Bowd, Ozerdem, and Kassa (2010) suggest that “participation literature seems to be infested with binary models of power such as the urban elite and the rural poor, the uppers and lowers, the north and the south, academics and practitioners. Power relationships, however, are fluid and do not usually fall into such rigidly stated categories” (6).

Participation within CBR projects can reflect local hierarchies, and therefore “empowering” the community may reinforce inequality. Bowd, Ozerdem, and Kassa (2010) state, “Whilst the theoretical basis for these approaches may be well intentioned, in practice participation is not an emancipatory exercise for many due to the fact power dynamics within societies and communities are not accurately and comprehensively understood by those who instigate the use of such approaches. Thus local knowledge is a construct of the powerful” (15). CBR practitioners and engaged scholars must better understand power and how it gets used and negotiated within the community and within the research partnership. This demands reflexivity, a willingness to cede power, and an ability to recognize and challenge powerful community individuals and groups. Capacity building is one way to begin to empower those most disadvantaged and silenced by building skills and knowledge at both the individual and community level.

Capacity Building

CBR practitioners seek to build capacity within the communities they work with. This means that the researcher and practitioner organize, facilitate, motivate, train, educate, and foster community members, groups, and organizations to become architects, leaders, and authors of their own histories. The principle of capacity building requires that researchers not only “do no harm” but that they also leave communities empowered and strengthened as a result of the research project. Participants co-learn research and advocacy skills, communication and group working skills, and about participatory democracy. The skills and knowledge learned can be transferred and applied to other projects or personal experiences. Capacity building extends the goals of CBR beyond the immediate project to the future. In doing this, community-based researchers recognize local knowledge, skills, expertise, and resources and help participants see these strengths within their community.

Social Change and Social Justice Orientation

The commitment to social change and social justice work within CBR projects is often multidimensional and multilayered; there is an expectation that participation in the project will lead to personal transformation, community empowerment, and macro-structural changes. Involving those most affected by issues and problems within their own communities in the research process is an act of social justice. Collaboration and power sharing

within the research process is empowering. Fiorilla et al. (2009) summarize the experiences participants shared as a result of their involvement in a CBR project involving students and women who were experiencing homelessness.

The students report how growth and change in the relationship is accompanied by listening with warmth, and empathy, and genuineness. For Dawn and Laura, however, this is not enough. The research process for them must move beyond this to having their experiences and expertise acknowledged and applied to action, action aimed at developing solutions for the problems they see as meaningful in their lives and others within their community for whom they give voice. The student researchers also underlie the power of sharing stories as they begin to connect as co-researchers, co-creators and, as they articulate, most importantly, as women. [9]

It is important to acknowledge that CBR has primarily but not exclusively focused on empowering disenfranchised individuals and communities. Partners can cut across social categories—which can lead to both benefits and challenges for all participants. While CBR practitioners may see possibilities for change as a result of the research gathered, it is critical that the decision as to what will happen as a result of the findings rests with the community. Even if the decision is taken not to act, the expectation is that personal transformation and lasting benefits to the community are likely.

Transformative

Clayton, Bringle, Senor, Huq, and Morrison (2010) contend that “the terms ‘relationships’ and ‘partnerships’ are not interchangeable” (5). They argue that relationships are interactions between individuals and can be short in duration and transactional whereas partnerships are transformational and characterized by “relationships wherein both persons grow and change because of deeper and more sustainable commitments” (7).

Case Study: Exploring “Voice” and “Knowledge” With People Living in Poverty

Krumer-Nevo (2009) argues that, in the first decades of the state of Israel, poverty was denied as it did not resonate with the dominant Zionist social democratic ideology. Until the beginning of the twenty-first century, poverty was presented as “a temporary problem for new immigrants” (283). Krumer-Nevo writes that the “voices, the knowledge and the actual presence of people who live in

poverty are absent from the public debate” (284). This PAR project was designed to give those living in poverty a “voice” equal to academics, policy-makers, social practitioners, and social activists to change attitudes about the poor. Krumer-Nevo used her “privileged” status to raise the idea of creating a PAR partnership between four ethnic groups who had little contact or trust of the other.

What was particularly interesting is that Krumer-Nevo realized as the project continued that a lack of voice was not the problem. She explained, “Most of the participants were eager to take part in the initiative, wanting their voices and knowledge to be heard by powerful people” (287). They were willing to share their personal experiences and knowledge as well as articulate what needs to change. Krumer-Nevo states, “The lesson we learned was that the real challenge was not the ‘empowering’ of people in poverty, since they were eager to participate in the public debate, but the fashioning of the discourse to become not merely formally inclusive but truly and deeply so” (292).

Krumer-Nevo found that giving voice to those who live in poverty is not enough. What must also happen is transformation—a multidirectional exchange of ideas, experiences, knowledge, and understanding where all stakeholders grow and where change happens as a result of the partnership.

Innovative

A final core principle of CBR is innovation: multidisciplinary groups including academics, practitioners, and community members are better able to think creatively and strategize how to research complex issues and problems. Morisky, Marlow, Tiglaio, Lyu, Vissman, and Rhodes (2010) describe their use of “a CBPR framework in which the collective knowledge, perspectives, experiences, and resources of these diverse partners, representing a broad spectrum of community stakeholders, helped guide the development, implementation, and evaluation of the interventions designed to reduce HIV risk among female bar workers (FBWs)” (372). Previous intervention strategies had not been successful in reducing HIV risk within this population. Morisky, Marlow, Tiglaio, Lyu, Vissman, and Rhodes (2010, 381) argue that it was this innovative CBPR project that provided new ideas for intervention with this vulnerable group of women. They state:

We used a CBPR approach that included community members, organizational representatives, and academic researchers to design, implement, and

evaluate the interventions. It seems clear that this type of partnership approach to research yielded interventions that were culturally congruent and highly acceptable to a broad spectrum of stakeholders, including: FBWs, establishment managers, floor supervisors, and customers. Coupled with their being informed by sound science and established health behavior theory, the developed interventions were as “informed” as possible. The approach also ensured that data collection methodologies were realistic to yield more valid and reliable data. [381]

Sessa and Ricci (2010) discussed their innovative PAR project involving scientists, citizens, and policymakers aimed at addressing what they see is a lack of “evidence-based policy-making and improve the science-policy interface” (50). Sessa and Ricci suggest that while the applied researcher acknowledges that the “legitimate” result of their research is to help policymakers make sound decisions that benefit individuals and communities, often there is a “lack of transfer” (5) of the research findings. They argue that the way to improve this transfer of research outcomes to policymakers is to involve a third party—citizens and stakeholders affected by the research. Research that involves all stakeholders is more likely to find solutions that are meaningful and applicable to the lives of those most affected by the data (Goh et al. 2009).

Skills and Practice of CBR

To conduct CBR requires skills that are often not taught in traditional social science programs or research institutes. CBR requires a major paradigm shift in the way we think about research—what we research, why we do it, and when and how we do it. This paradigm shift requires community-based researchers to learn and practice new skills. Additional skills can include community organizing, group work skills, and relational skills. A preliminary list of skills useful for CBR is as follows:

- **Research skills**—Knowledge of research methods, practices, and analysis are necessary for good CBR work. Methods can include quantitative, qualitative, and mixed-methods design. The research may involve random sampling, case studies, historical data, and art-based research. Decker, Hemmerling, & Lankoande (2010) reviewed twelve completed CBPR health intervention projects and found that studies with the strongest outcomes had higher-quality research designs.

- **Communication skills**—In partnering with communities and fostering their participation, it is critical that the researcher is able to communicate with and listen to all stakeholders and be able to foster communication between and within the community. Communication skills include written, oral, observational, and listening skills.

- **Relational Skills**—The community is often weary of outsiders and mistrust academic or external researchers coming in to their communities, so forming and building relationships can take time. CBR is relational research yet researchers often do not get training in “how” to build relationships with community members. Trust, respect, care, humility, deference, and honesty are all skills and behaviors that can foster partnership and collaboration.

- **Reflexivity**—Reflexivity is the awareness of and an analysis of self. It is being aware of who we are and how our behaviors, attitudes, values, and experiences influence how we think and behave with others. Without reflection there can be no action that is meaningful. Naples (1996, 169) states, “Who we are personally affects how we go about our work. Whether we want to own that or not, whether we are self-conscious about this fact or not our standpoint shapes the way we proceed to gather information and draw conclusions from that information.” We must practice self-reflection and self-awareness and model it in our work. Community-based researchers recognize “a self-reflective, engaged and self-critical role” (Israel, Schulz, Parker, & Becker, 1998; 181) is necessary.

- **Facilitation skills**—Begun, Berger, Otto-Salaj, and Rose(2010; 560) suggest that for successful partnership “there is a need for all partners to successfully integrate their different backgrounds, expertise, values, and priorities” (52). They acknowledge that, while CBR requires the full and active participation of the community, there are often barriers to participation. These can include time, financial restraints, language, culture, feelings of intimidation, and burnout. The CBR practitioners must minimize barriers and facilitate participatory democracy.

- **Organizational and group work skills**—Knowledge and skills related to group work and group processes is helpful for anyone working with community groups and organizations. There is extensive literature discussing group work skills, practices, and community organizing strategies that is helpful to know and understand. (See for example Staples, 2004).

• **Motivational skills**—Motivating community participants to engage in CBR projects can be difficult. Community members are often overstretched in terms of work and family commitments and/or they can be frustrated from previous research in their communities that provided few if any benefits. Motivation may also wane if community members leave or reduce their involvement and commitment for any number of reasons. The pace of CBR work can also be slow, and this too may require effort to keep participants engaged and involved.

• **Cultural competency**—Working in communities with diverse individuals and groups requires an awareness of and sensitivity to differences in language, ethnicity, race, social class, gender, sexual orientation, and other statuses. There is a large body of research that addresses cultural competency that cannot be addressed here but it is important to know, understand, and reflect on one's own, often privileged statuses as well as the cultural similarities and differences within and between our partners. Cultural awareness and competency is critical if CBR is to be inclusive, collaborative, and transformative. When involved in an international collaborative research project that takes place in a foreign country, the researcher must do intensive preparation work. Pinto (2000) suggests the researcher "start by studying the language, history, geography, social structures and politics of that country and of the specific community he or she proposes to study" (55).

• **Capacity-building skills**—Capacity building skills include educating, supporting, mentoring, and acknowledging the experiences and different ways of knowing of all stakeholders. Engaged scholars foster co-learning, understanding, and application of all the skills listed above so that community partners can use them in multiple ways in the future.

Entering the Field

Anyone new to a CBR paradigm begins by asking, "How do I start?" Recognizing that campus-community partnerships ideally should be initiated by community members, researchers often begin the process of establishing a collaborative research partnership. There are many ways that researchers can "enter the field." Naples (1996) suggests:

Some activist researchers search for a community-based site through which they might

assist in the political agendas defined by community members. A second avenue develops when a group, community, or organization seeks outside assistance to generate research for social change. Another avenue to activist researchers occurs when we enter "the field" as participants who are personally affected by the issues that is the focus of our work. Many of us who choose to use our personal and community-based struggles as sites for activist research did not begin the work with a research agenda in mind." (96)

Wallerstein, Duran, Minkler, & Foley (2005) confirm that it is always easier to form a research partnership with a community in which you have previous positive connections. If a connection has not been made, it is difficult and time consuming to build trust and foster a participatory and collaborative research partnership.

Building Trust

Researchers must gain knowledge of the community: individuals, groups, organizations, services, and the issues and concerns of residents. This can be through key informants, reports, census data, flyers, organizations, service providers, and spending time in the community and with community members. If the partnership is initiated by the researcher, one of the first tasks is to consider who is affected by or concerned about this problem. Netshandama (2010) acknowledges that identifying community stakeholders is not an easy task and suggests that the safest way of identifying community stakeholders is to pinpoint the most obvious participants without ruling out any groups and to make the process of selection open and transparent. Polanyi and Cockburn (2003) also identify that the initial stages of the CBR project can lead to some confusion and frustration as to the goals of the project. At the beginning of their CBR project with injured workers, some members were interested in research, but others felt they already had enough information and wanted to take action. Clarification and agreement to form a community-based *research* partnership is important; the distinction needs to be made between CBR, community organizing, and social action.

Questions for Consideration and Reflection

When beginning a CBR project, it can be helpful to think about questions and issues other practitioners have identified as important. A list of guiding questions is provided here for consideration,

dialogue, negotiation, and reflection when beginning and throughout a CBR project (adapted from Mandell, 2010, 153):

1. Is the CBR project transparent and inclusive of all stakeholders?
2. Do the researcher and community partners orient themselves within the same fundamental paradigm of social justice and social change?
3. Is there general agreement as to the nature of the social problem(s) and the range of possible solutions?
4. What is the scope of the research project including the research question(s), the methodologies, and the timeline for data collection, analysis, and final reporting? How will the findings be disseminated?
5. Have research ethics been addressed, including informed consent and confidentiality?
6. Have expectations, roles, responsibilities, and power sharing been discussed. Is there a sense of trust between partners?
7. Will there be collaboration at each stage of the project, including dissemination of the findings and co-authorship of any reports or journal articles?
8. In what ways will all stakeholders and the community benefit from participating in this research project?

Funding and Resources

Before beginning a CBR project, funding, resources, and budgets may be discussed. There are always benefits and challenges to receiving outside funding or grants. To participate in a CBR project takes time, money, and resources, and the scale of this will depend on the size of the project and what is already available from the campus or community. Projects can falter with little outside funding or resources. Resources can be administrative, including computers, meeting and office space, printing flyers and advertising materials, and research guides. Help with transportation may also be necessary to include all stakeholders. Resources can also include staffing; administrative help, and/or a project coordinator. A translator or cultural broker may also be necessary if one is working with individuals and groups from different cultural backgrounds. Polanyi and Cockburn (2003) state that the outside funding they received allowed them to “hire a (part-time) project coordinator, cover expenses for conferences and meetings with injured workers, and provide injured workers with an honorarium for their participation” (21). However, outside funders

may require explicit details regarding the sample, research methods, and questions to be asked and the objectives and expected outcomes. This may leave little flexibility that most CBR projects require. Outside funders may also want a “principal investigator,” usually affiliated with an academic institution or agency, to be accountable for budgets, data collection and analysis, and the final report. Academic institutions and funding bodies may be supportive of collaborative research projects but still find it difficult to agree to collective decision making and shared responsibilities.

Flicker, Wilson, Travers, et al. (2009) developed a survey to investigate use and effectiveness of CBR, specifically looking at facilitating and barriers to CBR work with AIDS service organizations (ASOs) in Ontario, Canada. They found that increased funding was critical to facilitating CBR and that “lack of funding and resources (space, computers, time and staff)” and “too many competing demands” were the greatest barriers. The qualitative interviews with community organization staff also found:

The interviews revealed that issues surrounding funding are complex. Agencies were frustrated about how rare it was for community-based organizations to get compensated for their investment and contribution to partnered research endeavors. As such, the issue was not simply about increasing funding but also relocating and reconfiguring budgeting practices so that ASOs could (1) be the direct recipients of research grants and/or (2) increase their internal capacities to conduct research and maintain an active research programs. (95)

When decisions about resources are not shared, any intent to foster power sharing can reflect a “false egalitarianism” (Nygreen, 2009–2010) and generate mistrust. There is a need to educate funding organizations around issues of democratic decision making, collective responsibility, and capacity building.

Emerging Issues Research Ethics and Professional Boundaries:

Community-based researchers are similar to ethnographers: they need to “get up close and personal” to gain trust and establish a collaborative partnership. As we get to know our partners, questions and concerns can surface about professional boundaries. When is it appropriate to advocate or provide services to community members or to intervene into their personal lives? When does the

CBR project end—after dissemination of the findings and the final report is completed or should community-based researchers continue their work into advocacy? How should we navigate our multiple roles, responsibilities, and relationships with our community partners to build trust, respect professional ethics and not exploit our partners? In reviewing the APA Code of Ethics, Baumann, Rodrilguez, & Parra-Cardona (2011) discuss the difficulties CBR practitioners have in negotiating their professional responsibilities. They state, “Establishing multiple and dual relationships with clients and community members carries the risk of becoming harmful and exploitive” (142). The APA Code of Ethics recommends “detached objectivity,” but CBR is about building trust and relationships.

There are also questions regarding the balance between scientific rigor and community needs. Baumann, Rodrilguez, & Parra-Cardona (2011) ask:

How can we balance science and community support? If methodology is changed based on community needs what are the implications to the validity of the methods? To the validity of the findings? (144–145)

The balance between scientific methods and community needs may be challenged at all stages of the research process—for example when community partners are eager to get the voices of certain community members yet random sampling is possible. Researchers may also find that their care and concern for their community partners makes scientific rigor sometimes difficult to uphold. For example, Schwartz (2010) asked students and community participants for their feedback on CBPR partnerships they were involved with and found that problems with communication and issues of power and control surfaced between partners, students, and the instructors. Students identified that they sometimes “felt pressure from their agencies to produce positive results” (8).

Another concern is confidentiality. Special consideration is needed when community members are involved in collecting data from their own communities that may be sensitive or stigmatizing. Smikowski, Dewane, Johnson, Brems, Bruss, & Roberts (2009, 462) suggest caution:

Given the unique challenges presented in community—researcher partnerships, additional ethical issues arise that often put the researcher in conflict with more traditional research ethics. For example, when community members share in

all aspects of the study, there may be difficulties maintaining confidentiality, or a heightened burden for participants with stigmatizing illnesses. [462]

This may require additional training and education regarding research ethics. While this training may extend the timeline for data collection, it builds capacity for future community-initiated research projects. Another dilemma that can arise is the pressure to collect data that fits with stakeholders’ experiences and/or expectations.

Collaboration or Exploitation

There needs to be a continuing discussion of the role of academia and power sharing within CBR partnerships. Can we have long-term and sustained partnerships between academics and community partners without them being exploitive or oppressive? Jackson and Kassam (1998) argue that participatory research programs have been “much criticized for becoming a new form of colonialism whereby western perspectives and priorities are imposed on oppressed groups” (cited in Ledwith & Springett 2010; 94). In discussing a PR project in Kyrgyzstan investigating health concerns, Jackson and Kassam discuss what they found: “Observations I made on a recent visit there indicate that the approach has had a substantial impact on the development of skills within rural communities. However, as the process has developed, agencies and government departments and the medical profession with their own agendas have tried to coerce communities into addressing needs that reflect their interests or perceptions” (cited in Ledwith & Springett 2010; 96).

Any discussion of power must include questions about “voice” and whose voice is heard and represented in CBR work. Community-based researchers must exercise caution when working with individuals or groups who may not represent the most oppressed or disenfranchised within the community. Working with community-based organizations or institutions can provide access to community members, but they may also function as “gatekeepers.” When we “partner up” with powerful community-based organizations, the staff may restrict access to less-powerful community residents if they are likely to challenge their position of dominance.

Case Study: A Thwarted CBR Project Concerning High School Dropout Rates and Absenteeism

In the spring of 2011, a senior staff member of a large public school department contacted our

Office of Community-Based Learning to inquire about the possibilities of a CBR partnership to look into high dropout rates and absenteeism at an alternative high school. I was asked and agreed to meet with the senior coordinator of alternative education programs for the district to learn more about the alternative high school—the programs offered and the students, faculty, staff, and resources available. I was introduced to the background and history of alternative education generally and the specific history of this school. The public school department in this district was not an organization that I had partnered with before. Although many of our students had interned, volunteered, or completed student teaching at schools in the district, there had not been a connection with this particular school. The senior coordinator explained they were interested in learning from students, parents, teachers, staff, and truancy officers about why the alternative high school did not substantially reduce absenteeism and dropout rates as expected.

It was agreed that this could form the basis of a pilot study, a small CBR project with my students in an upper level sociology of education course that fall. They were interested in interviews, observations, and focus groups with multiple stakeholders involved in the research design, data collection, and analysis of the project. To get approval of this small CBR project, we needed to meet with the director of research and evaluation for the district. In meeting with the director, it was explained to us that, while it would be “interesting” to learn more about the high dropout rates and absenteeism from multiple stakeholders involved with the alternative high school, there was no “political will” to do so at this time. It was explained that the *politics* of public schools are complex and that the bureaucracy is extensive. He was confident that this was not the time to collect data about the successes or failure of any of their alternative education programs. He politely said we could submit a research proposal for this pilot CBR project, but we would be denied at this point in time. He could not say when might be a better time to explore this issue. It did not matter that the senior coordinator of alternative education programs had informal agreement from some parents and teachers to participate. The project ended before it even began.

This case study indicates that, while partnering with community-based organizations can provide benefits, they can also function as gatekeepers that reinforce power inequality within communities. It is necessary to continue to understand and reflect how

power and privilege is negotiated, experienced, and challenged in dialogue and action. At this point, the CBR project is not being pursued.

Professional Barriers

Maguire (1987) lists difficulties often encountered by researchers doing PR work and suggests time as one of the greatest challenges for researchers and community partners. CBR can take a great deal of time—especially if one is partnering with a previously unknown organization or group. Building trust can take months or even years before collaboration and partnership are possible. Polanyi & Cockburn (2003; 23) in their work with injured workers also identified time commitments as extensive: “Academic participants spoke of how difficult it was to find the time needed to support this intensive process of collaborative inquiry, given heavy teaching, research, and publishing requirements.” Extensive time commitments may be necessary to build motivation and engage community members to establish a research partner. Tandon (cited in Maguire 1987) noted in reference to his personal assessment that most of his experience with PR had been a failure: “We simple underestimated people’s passivity” (42–43). Passivity can be experienced by both community members and faculty and can result from a number of factors, but to change this requires support—often institutional supports that are missing.

Institutional Barriers

There has been an increasing demand for academic institutions and funding bodies to facilitate CBR projects. Faculty often feel that their academic institutions do not recognize the scholarship of CBR in their tenure applications, the pedagogy of engaged scholarship, or their commitment to research and social justice work in their communities. Schwartz (2010) surveyed academics to get their feedback about CBR projects and found that faculty highlighted institutional barriers to CBR work as most problematic—time, lack of curriculum flexibility, resources, and the ethics approval process. Cancian (1996) makes the distinction between academic research and activist research and argues that to navigate both worlds of engaged scholar and academia is very difficult to do. She states:

Activist research is “for” women and other disadvantaged people and often involves close

social ties and cooperation with the disadvantaged. In contrast, academic research aims at increasing knowledge about questions that are theoretically or socially significant. Academic research is primarily “for” colleagues. “It involves close ties with faculty and students and emotional detachment from the people being studied. Social researchers who do activist research and want a successful academic career thus have to bridge two conflicting social worlds.” [187] “[P]articipatory research is so strongly oriented to the community that it is difficult to maintain an academic career. It is especially difficult to produce the frequent publications required by a research university on the basis of research that faithfully follows the tenets of participatory research. [194]

Academic organizations must also recognize and support transdisciplinary research and scholarship within a CBR paradigm. Levin and Greenwood (2008) write, “Action Research’s democratizing agendas and necessary transdisciplinarity run right into the brick walls of academic professional silos and disciplinary control structures to preserve disciplinary power and monopolies over positions and terms of employment and promotion of their disciplines” (212). Votruba (2010) refers to this as the need to “institutionalize this work—provide campus leadership; faculty incentives and rewards; planning and budgeting; annual evaluation, awards, and recognitions; and public policy aligned to support the scholarship of engagement” (xiv).

Twenty-five years ago, Boyer (1996) argued that we should not expect institutions of higher education to lead in tackling some of the world’s greatest problems—that in fact they were part of the problem. He wrote:

[W]hat I find most disturbing... is a growing feeling in this country that higher education is, in fact, part of the problem rather than the solution. Going still further, that it’s become a private benefit, not a public good. Increasingly, the campus is being viewed as a place where students get credentialed and faculty get tenured while the overall work of the academy does not seem particularly relevant to the nation’s most pressing civic, social, economic, and moral problems. [11]

Today there has been much progress within many institutions. However, this must continue as institutional leadership is critical to expanding CBR to tackle contemporary social problems within our communities and globally. Glass and Fitzgerald (2010) have written a “Draft Recommendations for

Engagement Benchmarks and Outcomes Indicator Categories” as a way to evaluate the extent to which institutions and faculty are involved and supported in campus–community partnerships. They suggest that the conceptualization of “scholars” and “scholarship” be broadened to reflect the community—creating “the community of scholars” and “community scholarship” to give full support and recognition of all partners.

Evaluation

CBR is difficult to evaluate in terms of assessing our successes and failures. What is a successful outcome of a CBR project? How can we assess or determine if “collaboration,” “empowerment,” or “capacity building” took place and to what extent? Peterson (2009) suggested that there is a growing body of research addressing the question of evaluation:

With the bulk of early research on community-based education focusing on the academic, civic, and moral benefits for students, many researchers in the late 1990s problematized the paltry research that had been conducted on the ways in which communities benefit or are burdened by the involvement of faculty and students in their community work. As a result, in the last 10 years a variety of studies have been conducted to assess this impact (544).

For example, in a comprehensive evaluation of published peer-reviewed articles related to the use and outcomes of CBPR in clinical health trials De Las Nueces, Hacker, DiGirolama and Hicks (2012) found CBPR projects “had very high success rates in recruiting and retaining minority participants and achieving significant intervention effects” (1379). They also found that authors often reported community participation in detail but were less likely to discuss participant involvement in the interpretation and dissemination of the research findings.

However, evaluation research of engaged scholarship is still limited. When projects take a very different direction than originally intended (as in Atalay, 2010), can it still be considered a successful CBR project? If the researcher does not see any evidence of transformation, but community members suggest they have learned a great deal (as discussed by Nygreen, 2009–2010), is this still success? Votruba (2010) challenges us to critically look at how we determine success. He states:

We need to do a far better job of assessing our engagement work. We’ve made progress in this regard

but, until we have reached agreement regarding what constitutes excellence in this domain, it will remain difficult to measure and reward. For example, should we focus on assessing activities or outcomes? What role does self-assessment play? How about peer assessment? Absent of appropriate and generally accepted standards for evaluating the scholarship of engagement, faculty members are less likely to embrace it because of the risk that it will not be recognized and rewarded. [xiii–xvi]

There are few guidelines as to how to evaluate CBR projects. As said previously, the core principles of CBR are not intended as evaluation criteria. A preliminary question might be “who” decides on the guidelines and criteria for success? Bowl, Tully & Leahy (2010) suggest, “In reflecting views that some parties to the research would disagree with, we were vulnerable to charges of selectivity and bias. Ensuring the validity of our findings was a challenge.” (47). They suggested an alternative way to approach validity in the research, by focusing on credibility rather than truth, stating, “Credibility entails a sense that researchers understand the field within which they research, and that they respect those with whom they research. The researchers themselves and not just their tools need to be ‘trustworthy’” (48).

As scholars and researchers working from a social justice and social change paradigm, we often reflect on whether our CBR work has made a significant difference and in what ways. Is social change an important criterion for evaluation of CBR projects? Lykes and Mallona (2008) suggest that engaged researchers and scholars have not been as successful as they might hope in making substantial, lasting change. They state, “A vast literature has emerged documenting and evaluating individual development projects and the ways in which they have or have not contributed to social change. Despite local contributions there is little evidence that the cumulative effect has either redressed social inequalities or reduced structural violence” (113). While this may be true, it suggests the need for continued reflection and action—praxis, not defeat. Small successes do matter, and the cumulative effects may still be emerging. We also need to “mainstream” CBR within academic institutions, communities, and funding bodies to increase opportunities through additional supports and resources.

There has been a huge increase in the scholarship of CBR for engaged scholars to learn from others in the field. Unfortunately, so much of the literature

about CBR principles, strategies, and exemplars is written for an academic audience rather than written for community members. Couto (2003, 71) In his review essay of Minkler and Wallerstein’s edited book *Community-Based Participatory Research for Health*, states, “Despite the wonderful examples of CBPR *for* and *with* community partners, we still have the challenge to develop methods that will permit community groups to conduct research *of* their own and *by* themselves. Only by striving to turn research *for* and *with* them into tools that community partners can use to do their own research will we really be pushing the cutting edge of concepts such as ‘empowerment,’ ‘community development,’ ‘community organizing,’ ‘representation,’ and ‘participation.’” Fuentes (2009–2010) also challenges community groups not only to participate in research projects but to take ownership and control over research concerning their communities and recognize their capabilities of being both subjects and architects of research.

Conclusion

CBR is a collaborative research project between researchers, community members, and sometimes students to formulate problems and find solutions that are meaningful and practical for all stakeholders. It has a rich history in critical pedagogy, critical theory, feminist theory, and the epistemology of knowledge that continues to influence the principles and skills that define CBR. Today we have exemplars that help guide new practitioners in their consideration of and engagement with community partners to form a collaborative and transformative relationship. If we use subjective measures to determine “success,” we have an abundance of evidence that suggests CBR and engaged scholarship has had substantial success in finding innovative solutions to complex problems in our communities. Successful projects have occurred in disciplines such as public health, psychology, sociology, anthropology, urban development, and archeology. It has also included projects that are transdisciplinary in design and practice. Success has also been found within diverse communities of interest: children and youth, aboriginal peoples, female bar workers, HIV and AIDS clients, injured workers, and immigrant families to name just a few discussed here. Evaluation research suggests that this paradigm shift to a new “orientation to inquiry” has fostered campus-community partnerships that address the traditional inequities in the research process as a result of the positivist paradigm.

The strength of CBR and scholarship is its diversity and willingness to be transparent in addressing challenges. Practitioners and scholars of CBR continue to struggle with issues related to power and control—how power is used and experienced by the researcher, community members, and other community-based organizations. Questions continue to be raised about encouraging sustained partnerships or developing community scholars who do not need or want outside researchers from academic institutions. At this point, it seems that there is a growing awareness that academic institutions should revisit their public mission to serve, to collaborate with community partners on community-defined issues. I am not convinced that community organizations and/or community members are developing this same mission. However, if independence from academic institutions is a sign of capacity building, then “success” may result in continuously new partnerships. This may be more challenging for researchers and practitioners and warrants further consideration.

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Lineages: A Past, Present, and Future of Participatory Action Research

Sarah M. Zeller-Berkman

Abstract

Participatory action research (PAR) in the twenty-first century asserts a democratization of who has the right to create knowledge, engage in participatory processes, research social conditions, and take action on issues that impact their lives. PAR is an approach to doing research that is based on a set of commitments. PAR theory and practice is a collective creation, benefiting from the thoughtful work of hundreds of people from more than sixty countries. This chapter traces three of PAR's historical lineages, explores a current convergence of lineages called *critical PAR*, and offers some areas for future consideration.

Key Words: Participatory action research, critical participatory action research, action research



Looming large in this mosaic created from 14 definitions of Participatory action research (PAR) are the words “knowledge,” “process,” “research” and “action.” PAR in the twenty-first century

asserts a democratization of who has the right to create knowledge, engage in participatory processes, research social conditions, and take action about issues that impact their lives. Although PAR

is important to include in a methods handbook, PAR transcends method (Cammarota & Fine, 2008; Fine & Torre, 2004; Fals Borda & Rahman, 1991; Tolman & Brydon-Miller, 1997; Torre & Ayala, 2009). Participatory action research is an approach to doing research based on a set of commitments (Fals Borda, 1997b; Torre, Fine, Stoudt, & Fox, 2012). PAR theory and practice is a collective creation, benefiting from the thoughtful work of hundreds of people from more than sixty countries (Hall, 1981; McTaggart, 1997). In this chapter, I offer a brief outline of three of PAR's historical lineages, explore a current convergence of lineages called *critical PAR*, and offer some areas for future consideration.

Lineages

Quantum physics tells us that no matter how thorough our observation of the present, the (unobserved) past, like the future, is indefinite and exists only as a spectrum of possibilities. (Hawking & Mlodinow, 2010, pp. 105–106)

Just as there are multiple definitions of PAR, there are also multiple histories. Many people have traced the roots of PAR (Adelman 1997; Brydon-Miller 2001; Fals Borda 2006; Hall, 2005; Kindon, Pain, & Kesby, 2007; McTaggart, 1997; Torre et al., 2012) and have articulated histories that intersect and diverge from each other. Some argue that PAR is the result of a convergence of theoretical and practical traditions in many fields such as social work, education, agriculture, health, obstetrics, housing, and community development (McTaggart, 1997). The kaleidoscope of fields, intellectual traditions, popular movements, and “people’s knowledge” that has contributed to PAR grows more intricate as one examines the history of PAR in other parts of the world, such as Germany (Altrichter & Gstentner, 1997), the United Kingdom (Adelman, 1997); Colombia (Fals Borda 1991; 1997b); Australia (Grundy, 1997); Venezuela (Dinan & Garcia, 1997), and Spain (Brezmes, 1997).

In this chapter, three historical PAR lineages are examined. One emanates from the notable psychologist Kurt Lewin and his attempts to address “the minority problem” in the United States in the 1940s. Another emerges from committed activists and scholars operating in the midst of revolutions, failed development policies, and large-scale popular reform movements in Latin America. A final lineage traces the emergence of critical PAR. The history constructed by critical PAR theorists and

practitioners memorializes activist-scholars not often recognized for their contributions, specifically women and scholars of color. I offer a historical tracing and brief critique of each lineage in order to ground the second part of the chapter, which explores key concepts, methods, and potential new directions for critical PAR in its sociohistorical context.

Lewin

Philosophers have only interpreted the world in various ways; the point is to change it. (Karl Marx, *Theses on Feuerbach*, 1845, p. 72 in Engels, F (1888) The best way to understand something is to try to change it. (Kurt Lewin, in Greenwood & Levin, 1998, p. 19)

In 1933, Germany’s growing anti-Semitism forced a German social-psychologist by the name of Kurt Lewin to flee to the United States. A promising psychologist in Germany, Lewin continued to publish, lecture, and create theory as he settled into his life in the United States (Smith, 2001). In his first few years in the States, Lewin worked at Cornell University and for the Iowa Child Welfare Research Station at the University of Iowa. By the time he became a naturalized citizen, in 1940, Lewin was able to see both the differences and frightening similarities between the United States and Germany in relation to “minority problems.” Drawing on the Marxist orientation he had developed during his time at the Frankfurt School in Berlin before coming to the United States Lewin knew that he must simultaneously study anti-Semitism in the United States and take action. By World War II, Lewin began to devote concerted time and energy to examining the psychological problems confronting minority groups. As director of the Center for Group Dynamics at MIT, a position he held at that time, he began developing methods to alter prejudice and discrimination.

As Lewin became increasingly committed to using research to effect social change, he decided that it was best to do this work outside of the academy. In 1945, the Commission on Community Interrelations (CCI) was officially launched by the American Jewish Congress, with Kurt Lewin as chief consultant (Cherry & Borshuk, 1998). This institute’s mission was to help the United States handle its “group problems” more efficiently and less prejudicially (Cherry & Borshuk, 1998). The emphasis on action research that became the predominant form of research conducted at CCI was a

direct result of the fact that the organization received funding from the American Jewish Congress. Lewin felt that the institute could not receive continued community support for CCI projects unless it met community needs head on. At CCI, he encouraged his staff to join with community groups to study real-life situations and produce results that could be used to effect change.

At CCI, Lewin focused his research agenda on the transformation of groups, communities, and institutions and away from problematizing individuals. This notion ran counter to the prevailing psychological approach of the time. His research and writings implicated all members of society as responsible for changing the conditions that create so-called minority problems. In a seminal piece entitled “Action Research and Minority Problems,” published in the *Journal of Social Issues* in 1946, Lewin wrote, “In recent years we have started to realize that so-called minority problems are in fact majority problems, that the Negro problem is the problem of the white, that the Jewish problem is the problem of the non-Jew, and so on” (p. 44). Although his analysis focused predominately on the Jewish minority, Lewin was adamant that his work should not only focus on Jewish people but be an intercultural project. Lewin utilized the power of groups to engage in collective inquiry aimed at changing communities and social institutions.

Initially, Lewin delineated four different varieties of action research: diagnostic, participant, empirical, and experimental. Lewin’s PAR was based on a spiral model of self-reflective cycles of planning a change, fact-finding, acting, observing and evaluating the process and consequences of change, reflecting on these processes and then replanning, acting, observing, and so forth. For scientific knowledge to be the basis for social action, Lewin wrote, “fact-finding has to include all the aspects of community life—economic factors as well as political factors or cultural tradition. It has to include the majority and the minority, non-Jews and ourselves” (Cherry & Borshuk, 1998, p. 126). Diverse group members would begin Lewin’s research process with a “thematic concern” (Kemmis & McTaggart, 1988) or general idea that some kind of improvement or change was desirable. The group would then engage in Lewin’s cyclical approach to research.

An early example of the work produced by CCI was the use of the Community Self-Survey to research integrating housing, equal employment opportunities, the training of community leaders,

and the best handling of street gangs (Marrow, 1964). The Community Self-Survey was initially developed by Charles S. Johnson and colleagues at the Race Relations Department (later Institute) at Fisk University, then systematized for use as a tool for democratic nation building by Margot Hass Wormser and Claire Selltitz, housing activists and research associates at CCI (Torre et al., 2012). The hallmark of the method was large-scale community participation and educational practices throughout the research process, particularly during data collection (Torre et al., 2012). Lewin felt that it was essential that the community being studied take part in the process in order to “instill fact-finding procedures, social eyes and ears, right into social-action bodies” (Marrow, 1969).

This nascent version of PAR in the United States was characterized by Lewin as research for social management or social engineering (Marrow, 1969). Recent critiques of PAR have focused on the negative aspects of PAR’s use in social management because it has commonly been deployed within the international development context (Cooke & Kothari, 2001). At the time, however, Lewin believed that social engineering had potential as a new type of science that could make a viable contribution to the maintenance of democratic traditions (Cherry & Borshuk, 1998). Although Lewin believed that both the process and the products (such as reports, articles, etc.) of PAR should be participatory, the literature of the time suggests that he did not include nonacademic co-researchers in theorizing about how to improve race relations or engage in community development (McTaggart, 1997).

The critique of Lewin’s brand of PAR in no way minimized the impact of its contribution or the contribution of Lewin as a teacher and mentor. Lewin had many intellectual descendants and colleagues who name him as an influential figure in their lives. Psychologist such as Morton Deutsch studied under Lewin at MIT and worked with him on projects related to group tensions and racial attitudes. Deutsch’s later work on integrated housing led to a reversal of the policy of segregated public housing. Deutsch is regarded as the founder of modern conflict theory, has acted as the president of many psychological associations, and has published prolifically. Deutsch also mentored scholars such as Michelle Fine, who has spent her academic career using social science to promote social justice and who has worked with her own students to develop critical PAR (see the section on critical PAR later in this chapter). Other notable students

and colleagues include Elliot Jaques, a student of Lewin's who helped found the Tavistock Institute of Human Relations, and Ronald Lippitt, who collaborated with Lewin on the establishment in 1945 of the National Training Laboratories. Last, Festinger and Cartwright suggest that it is because Lewin showed how research could tackle complex social phenomena that many regard Lewin as a founder of modern experimental social psychology.

Vivencias: PAR's Latin American Roots

The greatest humanistic and historical task of the oppressed: to liberate themselves. (Freire, 1970)
There was a lot to do, to fight for, to plan. We had to stop crying and start fighting. (Che Guevara, as quoted in Castaneda, 1998, *Companero*, p. 83)

In 1960, the success of the Cuban revolution sent shockwaves of change reverberating across Latin America. People felt hopeful, powerful, and inspired to mobilize. According to Fals Borda, a pioneer of PAR from Colombia, this is one of the starting points for PAR (or participatory research/PR) in Latin America (Fals Borda, 1991). Participatory action research's Latin American lineage intertwines the contributions of various theorists, philosophers, and educators, braiding a revolutionary research paradigm that influenced people around the world.

Failed international development policies of the 1960s and 1970s fueled a desire of social scientists in the Latin American lineage to promote self-sufficiency in their research processes (Fals Borda & Rahman 1991). In fact, many social scientists began to reject the positivist paradigm advanced by international development agencies because they felt it served to maintain the status quo (Maguire, 1987). There was an urgent need for a new approach that would both transform people and also attempt to change structures while remaining independent of outside intervention. The goal was to engage in a process of collective inquiry and action to solve problems identified by those most directly impacted (Fals Borda, 1997a). It was the distinctive viewpoint of PAR practitioners in this lineage that "domination of the masses by elites is rooted not only in the polarization of control over the means of material production but also over the means of knowledge production, including control over the social power to determine what is useful knowledge" (Rahman, 1991, p. 14). This lineage was concerned with using "common" people's expertise to shape policy and action.

In this PAR lineage, trained social scientists or researchers from academic institutions were referred to as "animators" who facilitated the transformation of common knowledge to critical knowledge within a research collective (Fals Borda & Rahman, 1991). The ultimate goal of this "animator" was to become unnecessary as the collective became better skilled at engaging in inquiry, producing knowledge, and using it to resolve their own problems. Valuing eventual self-sufficiency of community collectives from "outside" researchers necessitated a focus on capacity building throughout the research process. This sensibility cultivated a "creativist" view of development in which activist scholars worked with people to research, design, and enact social change. This model rejected the approach of "consuming" from those in power an idea of how development/change should happen (Fals Borda, 1997b).

Also in the 1960s, Paulo Freire, a Brazilian educator, philosopher, and critical theorist, emerged as a major figure challenging existing models of education and research. Freire developed a democratized research process to support people's participation in knowledge production and social transformation (Freire, 1972; Kindon et al., 2007). Freire's process emphasized the importance of raising critical consciousness or *concientizacao* in order to create social change. Freire believed that to develop critical consciousness one must learn to perceive economic, political, and social contradictions through inquiry, reflection, and action (Freire, 1970; Kindon et al., 2007).

Freire's ideas and approach to community change connected with those who were fighting against imperialism, colonialism, oppressive "development" strategies, and positivistic models of research. Freire acted as a conduit, bringing the ideas of Fals Borda and other Latin American social scientists to the attention of people in other parts of the world. In 1971, Paulo Freire visited social scientist Marja-Liisa Swantz, who was doing work in Tanzania. Drawing on Freire's methods, Swantz then coined the term "participatory research" to describe her Tanzanian work promoting community-led development projects (Hall, 2005; Kindon et al., 2007). Rajesh Tandon of India used the name "community-based research" to describe a similar approach that he developed from the ideas in this lineage (Hall, 2005; Kindon et al., 2007).

Many of the PAR theorists in this lineage believed that PAR had the ability to radically alter the sociopolitical climate of their countries from the bottom upwards (Fals Borda, 1997a). Fals Borda, a

prominent social scientist in Colombia, was determined that PAR had the potential to create a new type of state:

In the same way, the creative sociopolitical force set in motion by PAR may also lead to the conformation of a new type of State which is less demanding, controlling and powerful, inspired by the positive core values of the people and nurtured by autochthonous cultural values based on a truly democratic and human ideal. (Fals Borda, 1991, p. 6)

Fals Borda and other founders of PAR in this lineage were adamant that creating a “people’s science” would have a profound impact on society as a whole.

The PAR work being done in the majority world until 1977 was characterized by an activist and somewhat antiprofessional bent because many of the researchers quit university posts (Rahman & Fals Borda, 1991). There was a strong rejection of established institutions related to government, traditional political parties, churches, and academia (Rahman & Fals Borda, 1991). This early activism and radicalism eventually gave way to people working with PAR in a spectrum of contexts, both within and outside academia, while maintaining their revolutionary spirit and commitment (Rahman & Fals Borda, 1991). In this phase, many of the founders began to clarify some of their theoretical positions.

Although many theorists shaped PAR’s Latin American epistemological roots, the most influential were Marx and Gramsci. Drawing on Marx’s theory of dialectical materialism, a PAR approach in this lineage views institutions and practices as socially and historically constituted but able to be reconstituted as a result of human agency and action (Kemmis & McTaggart, 1988; Oquist, 1978). Marx considered *praxis*—the process by which a theory, lesson, or skill is enacted, practiced, embodied, or realized—as an essential component of knowledge creation. Similar to the concept of praxis, Spanish philosopher Jose Ortega y Gasset, who, according to Fals Borda, (1991), had a profound influence on the PAR of Latin America, developed the concept of the *vivencia*. *Vivencia* means that “through the actual experience of something, we intuitively apprehend its essence; we feel, enjoy and understand it as reality, and we thereby place our own being in a wider, more fulfilling context” (Fals Borda, 1991, p. 4). Theory in this lineage emerges from experience and collective action. Value is placed on those members of the research team who have experienced the issue under investigation. Additionally, all co-researchers are

strongly encouraged to reflect on their own experience to generate theory, create research, take action, and then reflect on those actions to refine theory.

The notion of the dialectic expounded on by both Marx and Gramsci is a key concept in Latin American PAR. Contradictions such as subjective–objective and oppressed–oppressor are analyzed in terms of the relationship and interdependence of the terms (Oquist, 1978). Perceiving the dialectical relationship between commitment and praxis led those in this lineage to reject other asymmetrical relationships in traditional academic research paradigms such as subject–object and researcher–researched. The Latin American PAR lineage promoted the idea that there was immense potential for creativity once dichotomies are broken down. PAR researchers believed that offering a seat at the table to those people who have been historically denied translates into better research and action. Researchers and community members in this lineage hoped that PAR would valorize common knowledge and democratize knowledge production in service of the oppressed. In fact, Gramsci called for a new kind of intellectual to be at the forefront of this counterhegemonic process: the “organic intellectual” (Gramsci, 1971). “Organic intellectuals” are described as being critical of the status quo and using common language and culture to activate the existing intellectual activity of the masses. Breaking the monopoly of knowledge by those in power was a core aim, and many academics doubled as revolutionaries attempting to break down hierarchical structures in solidarity with those most effected (Fals Borda & Rahman, 1991).

Although this PAR lineage was resurrected mainly in Latin America, Africa, and some parts of Asia, a consciousness was growing in Europe and North America as well. In Canada, Stinson developed methods of evaluation along action research lines for community development work (Hall, 1981). Bud Hall became a major figure in Canadian PAR work and around the world. Dr. Hall has either founded or led a variety of organizations and networks that utilize and promote PAR, including the International Participatory Research Network and the North American Alliance for Popular and Adult Education. In the Netherlands, Jan de Vries has explored alternative research paradigms from a philosophical base (Hall, 1981). In Britain, the National Institute for Adult Education pioneered participatory research in evaluating its adult literacy campaign (Hall, 1981). In Italy, Paolo Orefice applied PR research to investigations of community and districts’ awareness of power and control (Hall, 1981). And, finally, in the

United States, in addition to those working with PAR from the Lewinian lineage, from the 1930s on the Highlander Center in Tennessee became a space for activists to use PAR and popular education in the service of workers' rights, land issues, civil rights, immigrant rights, and youth movement building.

In 1977, the Participation Research Network was formed to support people from Africa, Asia, Europe, Latin America, and North America engaging in ongoing dialogues on how to improve PAR/PR. In 1982, Mohammad Anisur Rahman, a founding PAR researcher from Bangladesh, made a formal presentation on PAR in academic circles during the Tenth World Congress of Sociology in Mexico City. According to Rahman and Fals Borda (1991), in the 1980s PAR showed signs of intellectual and practical maturity as encouraging information arrived from fieldwork and through publications in several languages. Cross-cultural dialogues continued to flourish and reassessment and evaluation became an integral part of this growing movement. This "second wave" of PAR happened mainly in the community development and international development contexts (Kindon et al., 2007), characterized by methods like *rapid and participatory rural appraisal* (RRA and PRA). By the 1990s, PAR grew in popularity both inside and outside of institutions.

Participatory action research emerging from this lineage continues to speak back to power, work the hyphen between activist-scholar, democratize knowledge, and define anew who is considered expert. Yet there are valuable critiques and fears voiced from within and without the PAR community. Some, such as Lykes and Coquillon (2009), say that as PAR has come to be utilized more regularly "in universities, in the work of governments, international organizations (e.g., the World Bank) and non-governmental organizations, in schools and universities, and in the research literature... it is becoming divorced from its revolutionary roots (p. 12). They argue that PAR is becoming a tool for improving practice (Lykes & Coquillon, 2009), not a means of using social science to expose inequity.

Cooke and Kothari (2001) argue that it is important to examine the discourse of participation itself, what they describe provocatively as the "tyranny of participation." Among other cautions, they suggest that the particular forms of "democratic participation" championed in action research and in development may marginalize indigenous ways of knowing and purposefully silence knowledge that actually challenges the status quo. In "Do You Believe in Geneva? Methods and Ethics at the

Global Local Nexus," indigenous PAR scholar Eve Tuck echoes warnings against blindly promoting notions such as "democratic participation" without thinking about issues such as sovereignty and sacred knowledge (Fine, Tuck, & Zeller-Berkman, 2008).

Feminist PAR scholars have also questioned what "democratic participation" looks like in PAR projects when those who *do* participate are in many instances those who *can* participate (Lykes & Coquillon, 2009). In Lykes and Coquillon's (2006) piece on PAR and feminism, the authors mention the notable absence of women from many PAR initiatives, citing barriers such as the duration of an action project (i.e., more hours than a participant can spare from minimum-wage or day work) and location at distance from the duties of participants (e.g., away from the field or children).

In addition to exposing the absence of women from many of the PAR projects, feminists critiques have noted that gender oppression and heterosexism were rarely topics of study (Lykes & Coquillon, 2009) and that women, more specifically feminists, were marginalized in professional gatherings of PAR practitioners (Brinton Lykes & Coquillon, 2006; Maguire, 2001). Womanists also charge that the participatory norms of PAR fail to challenge gendered hierarchies or transform traditional relations that oppress women (Lykes & Coquillon, 2006) and that PAR that emphasizes local issues, dynamics, and change does not directly address larger political and economic structures (Lykes & Coquillon, 2006; Brydon-Miller, Maguire, & McIntyre, 2004).

Notable PAR scholar and feminist Patricia Maguire (2002) has argued that the intersection of PAR and feminism may actually be a way to reinvigorate, re-energize and re-politicize participatory work (see <http://www.einaudi.cornell.edu/parfem/parfem.htm>). It is with the cautions of these feminist, critical, and indigenous scholars in mind that a new lineage of PAR was formed, *critical PAR*. The scholars in this lineage acknowledge the equally significant, yet often invisible scholars, particularly women and men of color who have shaped the theory and practice of PAR today (Torre et al., 2012). In the next section I retrace a history of critical PAR and, from this historical foundation, move into a discussion of contemporary commitments, methods, and potential new directions of critical PAR.

Critical PAR

We wanted more. We knocked the butt ends of our forks against the table, tapped our spoons against

our empty bowls; we were hungry. We wanted more volume, more riots. (Justin Torres, *We the Animals*, 2011 p. 1)

There are social scientists who want more. They want the words “activist and scholar” in their names. They want theory and action. They want communities of scholars in graduate institutions but also in community centers, staff lounges, prisons, youth programs, and rural fields. They want to evoke ancestors such as Lewin and Freire, but they also want to uncover hidden parts of their family tree, including silenced aunts and great grandparents of color. With intellectual descendants in both the Lewinian and Latin American lineages and “drawing on critical theory (feminist, critical race, queer, disability, neo-Marxist, indigenous and/or post-structural), critical PAR is an epistemology that engages research design, methods, analyses, and products through a lens of democratic participation” (Torre et al., 2012, p. 1). Drawing heavily on the work of critical PAR theorists Torre et al. (2012), here I trace the history of this lineage.

Critical PAR theorists, like critical psychologists, feminist psychologists, and key quantum physicists, reject notions of rationality, objectivity, and absolute truth. In fact, a critical PAR history like the one outlined by Torre et al. (2012) in the *APA Handbook of Research Methods in Psychology* (Cooper, Camic, & APA, 2012) begins in the 1800s, with early psychologists like Wilhelm Dilthey who were wary of the growing trend toward positivism even in the beginning era of the field. Writing in the 1800s, Dilthey called for the field of psychology to distinguish itself as a holistic science that placed human experience into its sociopolitical context. He promoted multiple methodologies that moved between deep and broad, capturing the complexity of human experience (Dilthey, Makkreel, & Rodi (1989); Fox et al., 2010; Torre et al., 2012). Dilthey urged psychologists to move away from causal explanations for human behavior and social relations (Fox et al., 2010). Along with early psychologist such as Wilhelm Wundt, he expressed concerns about the limits of experimental psychology for fully capturing the importance of sociohistorical context and the unique experience of individuals (Danzinger, 1990; Torre et al., 2012).

Later in the 1800s, W. E. B. Du Bois took on the challenge to design research studies that would adequately capture the sociohistorical contributions to psychological phenomena, as well as to locate the “problem” not in individuals or groups but in the conditions in which they live (Torre et al., 2012). Du

Bois launched a series of studies focused on the social conditions of African Americans in the United States at the Sociological Laboratory at Atlanta University, where he was director and professor of economics and history from 1897 to 1910. Du Bois created a series of now famous studies—the Philadelphia Study (1986–7), the Farmville Study, the Virginia Study (1897), and the Atlanta University Studies (1897–1910)—which utilized some of the first large-scale community surveys in the United States to examine regional economics, birth and death rates, conjugal relations, occupations, wages and class distinctions, business and trade, communal organizations, and the experience of group lives (Torre et al., 2012). These surveys, which required the participation of many community members, were the precursors to the Community Self-Surveys utilized in the Lewin PAR lineage. This history, however, is rarely mentioned beyond critical PAR theorists.

Du Bois’s studies created a detailed account of structural racism at the turn of the century (Du Bois, 1898). Du Bois adamantly believed that scholarship drawing on diverse methods joined with structural analyses could be used to inform policy and create social change. The Atlanta Sociological Laboratory became a center for social inquiry, producing quality empirical research designed to use social science to support the transformation of oppressive conditions. This commitment of using social science for social justice is one that undergirds critical PAR today.

In addition to Lewin, critical PAR scholars identify Marie Jahoda, Lazarsfeld and Zeisel, Ruth Benedict, Gene Weltfish, Goodwin Barbour Watson, Robin Williams, and Claire Selltiz and Margot Haas Wormser as progressive scholars creating vibrant and important social action-oriented research in the 1930s–1950s (Torre et al., 2012). These scholars used PAR to speak back to economic and racial segregation.

Jumping continents and decades, critical PAR historians locate their roots in Latin America, specifically in the work of Freire and Martín-Baró. Although not as well known as Freire, Jesuit priest and social psychologist Martín-Baró transformed the notion that one could explain human behavior independently of the sociopolitical, historical, and cultural context in which it is situated. Martín-Baró argued that decoupling the role of social structures and/or oppression from its impact on psychological well-being incorrectly attributed sociopolitical problems to the individual. With the focus mainly on the individual, Martín-Baró articulated that it was no surprise that psychology was “serving the interests of the established social order, as a useful instrument for reproducing the system” (Martín-Baró, Aron, & Corne, 1994, pp. 37–38).

Liberation psychology reorients from an individual to a social orientation and therefore necessitates methods and actions that explore social injustice and inspire social change (Torre et al., 2012).

Working in the context of war and state-sponsored terror in El Salvador in the 1980s, Martín-Baró developed public opinion polls that were designed to reveal the social conditions and lived realities of El Salvadorians. Martín-Baró used his scientific instruments as social mirrors that would simultaneously interrupt propaganda spread by those in power and orient people toward what ought to be. He created a science “of the oppressed rather than for the oppressed, that designs research from the perspective of those most impacted by injustice” (Torre et al., 2012, p. 11).

Martín-Baró charged psychologists to engage in three critical tasks: recover historical memory, de-ideologize everyday experience, and utilize people’s virtues. Critical PAR theorists have taken on this charge by creating research firmly based in historical context, generating research on forgotten alternatives in the history of science, and drawing on the strengths of all those in research collectives to produce important research and action that disrupt injustice (Torre et al., 2012).

In 2009, two visionary activists/scholars—Michelle Fine and Maria Torre—created a home for critical PAR researchers in the Public Science Project (PSP) at CUNY Graduate Center in New York City. The Public Science Project grew out of more than a decade’s worth of PAR at the Graduate Center of the City University of New York (CUNY) being done by a coalition of activists, researchers, youth, elders, lawyers, prisoners, and educators (with which I identify). In varied settings, the collective has “focused on the history and accumulation of privilege and oppression, the policies and practices of reproduction, the intimate relations that sustain inequity, the psycho-dynamic effects on the soul, and the vibrant forms of resistance enacted by individuals and collectives” (Fine, Tuck, & Zeller-Berkman, 2007, p. 498). Projects nurtured

in this vibrant space for critical PAR scholars include those that examine opportunity gaps (Fine et al., 2005; Guishard, 2009), heteronormativity in education (Linville & Carlson, 2010), sexual harassment (Smith, Huppuch, & Van Deven, 2011; in conjunction with Zeller-Berkman) and violence (Stoudt, 2006), the critical relationships between health and education (Freudenberg & Ruglis, 2007; Ruglis & Freudenberg, 2010), pushout practices (Tuck, 2012), school restructuring (Ayala & Galleta, 2012), and college access (Cowan & Chajet, 2012) across elite and underresourced schools. Many research collectives have examined the long arm of the carceral state, including policing practices (Fine et al., 2003; Stoudt, Fine, & Fox, 2012), prisons (Fine et al., 2001; web.gc.cuny.edu/che/changing_minds.pdf), as well damage, resilience, and resistance to the collateral consequences of mass incarceration (Muñoz Proto, 2012; Zeller-Berkman, 2007 Boudin & Zeller-Berkman, 2010) and those who have served their time (Marquez, 2012). Other research collectives look at the embodied consequences of gentrification (Cahill, 2004), resilience of LGBTGNC young people dealing with economic injustice (Welfare warriors, 2010), the streets as a site of resilience for young African-American men (Payne, 2011), hyphenated selves (Sirin & Fine, 2008 Zaal, Sala, & Fine, 2007), the relationship between young people and adults in the United States (Zeller-Berkman, 2011), privilege (Stoudt, 2009; Stoudt, Fine, & Fox, 2012), and participatory public policy (Fine, Ayala, & Zaal, 2012). Some projects such as “polling for justice” collect data that move across the spheres of education, policing, and health (Fox & Fine, 2012), whereas PAR theorists such as Fine and Ruglis (2009) theorize across those “circuits of dispossession.” Last, PAR theorists are studying social movements, offering activists tools to create shorts in those same circuits of dispossession through the use of collective action (Muñoz Proto, et. al. (in press) <http://www.memoscopio.org/>; OCCUPAR):

Critical PAR exemplars

- Investigating the subjectivities and heteronormative violence of white elite masculinity within exclusive private all-boys school (Stoudt, 2009)
- Documenting the material and psychological consequences of opportunity gaps in wealthy desegregated school (Fine, Roberts, Torre, Bloom, Chajet, Guishard, & Payne, 2004).
- Developing school-based internships in which students in small progressive public schools investigate finance inequity and college access (Bloom (2007); Cowan, & Chajet, (March, 2012); Chajet (2006)).
- Collaborating with mothers and youth in varied communities of the Bronx organizing for educational justice (Family-to-Family: The Guide to the Schools of Hope http://www.lehman.edu/deanedu/thebronxinstitute/Media_And_Publications/ENLACE_Family-to-Family_Guide.pdf)

- Mobilizing with youth pushed out of their high schools, researching the politics of the GED, the subjectivities of educational desire, and meritocracy (Tuck, 2012)
- Facilitating research as queer youth document the sexuality climates and heteronormativity in schools and beyond (Linville, & Carlson, 2010)
- Researching, in a longitudinal design, with urban youth, educators, and parents in the midst of school restructuring (Ayala & Galletta, 2009).
- The Street Life Project, a systematic historic, quantitative, and qualitative analysis of the “streets” as a site of resiliency for young men of African descent (Payne, 2011).
- The Fed Up Honeys, with young women from the Lower East Side of New York “fed up” with the stereotypes that spew across their neighborhoods (Cahill, Rios-Moore, & Threats, 2008)
- The Corporate Disease Promotion project, in which youth from elite and neglected communities document the promotion of disease by corporations selling alcohol tobacco and low-nutrition foods (Ruglis, 2008)
- The “Anything can happen with the police around” a quantitative survey, produced, disseminated, and interpreted by youth researchers and completed by more than 900 young people on the streets of New York City, documenting their experience of police surveillance, including sexual harassment by police (Fine, Freudenberg, Payne, Perkins, Smith, & Wanzer, 2002)
- The “Weight of the Hyphen” study of Muslim-American young women living in post-9/11 and post-homeland security New York City and negotiating surveillance by the state, media, community, family, and self (Zaal, Salah, & Fine, 2007).
- Projects to document, assess, and resist the collateral damage provoked by mass incarceration of people of color and a series of projects designed to document, assess, and resist the collateral damage provoked by mass incarceration of people of color: one in a women’s prison in New York State, documenting the impact of college on women in prison, the prison environment, and on the women’s post-release outcomes; (see Fine, Boudin, Bowen, Clark, Hylton, Martinez, Missy, Rivera, Roberts, Smart, Torre and Upegui, 2001; www.changingminds.ws); one with the children of women in prison (Boudin, Kathy & Zeller-Berkman, 2010); and one with women and men who have served long sentences in prison for violent crimes (Marquez, 2012)
- The Morris Justice Project, an intergenerational collective of researchers from the Morris Avenue section of the South Bronx, the Public Science Project at the CUNY Graduate Center, and Pace University Law Center. Working together since the spring of 2011, they have documented community experiences with the police, surveying and interviewing more than 1,000 residents of the neighborhood ([http:// morrisjustice.org/about-us](http://morrisjustice.org/about-us))
- OCCUPAR, a collective of graduate, undergraduate, alumni activists, and scholars from the CUNY system who studied the occupy CUNY movement. Working together from the beginnings of the Occupy CUNY movement in 2011, this collective has collected more than 300 surveys from across CUNY campuses to explore the barriers and opportunities for student political engagement at CUNY.

Many of these projects have been youth PAR projects. In most of these projects, research camps set the stage for research designed to bring together differently positioned people around a common table to design and implement the research. Whether collaborating with youth or adults, formerly incarcerated people, students or other activist-scholars, the following principles and commitments guide the work. In the hope of maximizing participation within these critical PAR projects and addressing some of the historical critiques of PAR, Torre has outlined the following set of agreements (see [www. publicscienceproject.org](http://www.publicscienceproject.org)):

- To value knowledges that have been historically marginalized and delegitimized (i.e.,

youth, prisoner, immigrant) alongside traditionally recognized knowledges (i.e., scholarly).

- To share the various knowledges and resources held by individual members of the research collective, across the collective, so members can participate as equally as possible.

- To collaboratively decide appropriate research questions, design, methods, and analysis, as well as useful research products.

- To create a research space where individuals and the collective can express their multiplicity and use this multiplicity to inform research questions, design, and analyses.

- To encourage creative risk-taking in the interest of generating new knowledge (i.e.,

understanding individuals and the collective to be “under construction,” with ideas and opinions that are in formation, expected to grow, etc.).

- To attend theoretically and practically to issues of power and vulnerability within the collective and created by the research.
- To strategically work the power within the group when necessary to benefit both individual and collective needs/agendas
- To excavate and explore disagreements rather than smooth them over in the interest of consensus (because they often provide insight into larger social/political dynamics that are informing the data).
- To use a variety of methods to enable interconnected analyses at the individual, social, cultural, and institutional levels.
- To conceive of action on multiple levels over the course of the PAR project.
- To think through consequences of research and actions.
- To an ongoing negotiation of conditions of collaboration, building research relationships over time.

These agreements have been shaped by Lewin, Freirer, Du Bois, Marie Jahoda, and Martín-Baró and take seriously the critiques leveled against PAR in the 1990s. Feminist critical scholarship continually molds, reminds, and propels contemporary critical participatory action researchers. For example, Patricia Hill Collins’s (1990) and Dorothy Smith’s (1987; 1990) discussions of how power shapes gender relations within and across racial, class, and sexual diversities are reflected in the work of Fine, Torre, Maguire, Lykes, McIntyre, and others who work at the intersection of feminism and action research (Lykes & Coquillon, 2006). A feminist perspective is reflected in the value critical participatory action researchers place on diverse ways of knowing, encouraging dissent, exploring silences, developing relationships, and interrogating how one’s position in structures of subordination shape one’s ability to see the whole in a way that may not be possible from a top-down perspective. Critical PAR theorists such as Maria Torre and Jennifer Ayala use Gloria Anzaldúa’s lens of *borderlands* to capture multiple identities and positionalities in PAR research (Torre & Ayala, 2009). Like other womanist PAR researchers before them (Brydon-Miller, Maguire, & McIntyre, 2004), Torre and Ayala argue that feminist scholarship is useful for delineating aspects of critical PAR that retain its commitment

to liberation as opposed to aspects of PAR that have been co-opted and misused (Torre & Ayala, 2009). Feminist scholarship reminds PAR practitioners to maximize PAR’s transformative potential, making an explicit commitment to social justice.

Feminists and indigenous scholars alike have honed critical PAR researchers’ attention to the nuances of power in a global twenty-first-century society. Scholars such as Parpart claim that researchers cannot afford to focus their power analysis only on local dynamics, considering that “national and global power structures constrain and define the possibilities for change at the local level” (Parpart, 2000, p. 18). Globalization, neoliberalism, and glocal politics require critical participatory action researchers to explore an expanded notion of “generalizability.” Fine et al. (2008) have articulated the concept of *intersectional generalizability*: “work that digs deep and respectfully with community to record the particulars of historically oppressed and colonized peoples/communities and their social movements of resistance, and work that tracks patterns across nations, communities, homes and bodies to theorize the arteries of oppression and colonialism.” (Fine et al., 2007, p. 516). While assuring that the results of PAR work be accountable to local history and desire, critical PAR theorists are currently being asked to bring an awareness of what stretches topographically across circuits of privilege and oppression, boroughs, countries, and continents (Katz, 2004).

A critical PAR lineage, mentored by feminist and indigenous scholarship, also interrogates what it means to have evidence:

The work of proving, long colonized to mean the work of men, of progress, of the whitestream, the work of scientists, the work of the academy, is reclaimed through participatory research. Participatory research, mentored by Indigenous concepts of “researching back,” infused by a call for knowingness, analysis, and recovery (LT Smith, 1999) means the proof is under our fingernails, in our melting footprints, on our park benches, in our clusters, in our flights, on our backs, our chapped lips, in our stories and the grandmothers who told them. (Fine, Tuck, & Zeller-Berkman, 2007, p. 519)

This expanded notion of proof means thinking critically about the products that emerge from the research process. In Cahill and Torre’s 2010 piece, “Beyond the Journal Article,” the authors explore the politics of representation, audience, and presentation of research, posing questions such as: What

kinds of research products speak to what kinds of audiences? How do we engage new audiences with our research? Should some audiences be privileged? How might the research provoke action?

Although critical PAR researchers have produced many academic articles, chapters, and books, other products include using huge projectors to display data on buildings, creating videos, designing stickers, writing reports and white papers, and performing the data, to name a few. In fact, designing multiple products aimed at impacting multiple audiences is key for many critical PAR research collectives. Most importantly, all members are involved in the collective imagining of what products best represent the evidence for a particular audience.

Many critical PAR theorists are intentional about capitalizing on the point of contact between participants in the study and researchers to conceptualize methods that use the process, not only the product of research, as a potentially transformative experience. Critical PAR asks researchers to take into consideration the research questions, transformative potential of the method, the audiences for the research, the strengths of the research collective, and time constraints, as well as the needs of funders or institutions when choosing methods. Because PAR is a design, not a method, researchers have often utilized multiple methods, including large-scale surveys (Fine et al., 2003; 2004; Stoudt, Fine, & Fox, 2011); autoethnography (Cahill, 2004), focus groups (Zeller-Berkman, 2008, and many others); and visual texts including collages, collective drawing (Brinton Lykes, 2005), interviews (Anand et al., 2001; Guishard et al., 2005; Fine et al., 2001; Segalo, 2013; Hondagneu-Sotelo, 1996), participatory video (Hume-Cook et al., 2007), participant participatory GIS (Elwood et al., 2010), photo-voice (Krieg & Roberts, 2010), participatory theater (Guhathakurta, 2008; Fox & Fine, forthcoming; Cieri & McCauley, 2007), participatory art (Tolia-Kelly, 2010), participatory mapping (Futch & Fine, 2014 Sanderson et al, 2010), participatory diagramming (Alexander et al., 2007), and life histories (<http://highlandercenter.org/resources/library-and-resource-center/>). Like many researchers, critical PAR research teams often use multiple methods in order to triangulate their data, layering traditional and creative methods in the same design.

Although acknowledging that participation exists on a continuum (Hart, 1997; Kindon et al., 2007), critical PAR practitioners strive to operate on the more participatory end of the spectrum. Joint decision making is encouraged while developing research

questions, designing the project, collecting data, analyzing data theorizing, and developing products. Critical PAR researcher Maria Torre (2005) has written extensively about how to work the contact zone between very differently positioned researchers in a collective to support maximum participation. Other critical PAR scholars have explored how to use cycles of critical reflection and action to deepen participation on multiple levels (Cahill, 2007; Williams & Lykes, 2003; Zeller-Berkman, 2008). Speaking back to earlier critiques of Lewin that claim nonacademics are often left out of components of the research process such as statistical analysis and theorizing, many in the critical PAR lineage have ensured that all co-researchers participate in deciding which questions to ask of their data and in uncovering what their findings reveal about theory and/or practice (Cahill, 2004; Stoudt et al., 2011).

Being grounded in an ethical framework is essential when doing critical PAR (Smith, 1999, Tuck & Guishard, 2013). Because institutional review boards (IRBs) vary in their knowledge of PAR, at times contesting whether PAR is in fact research, whether nonacademic co-researchers are actually participants, and whether PAR research is too risky for educational institutions to sanction, critical PAR researchers continue to push each other to adhere to ethics that mirror the ideological commitments of critical PAR. Critical PAR theorists Tuck and Guishard (2013) envision an ethics of doing participatory research that moves beyond IRB approval and assures the individual rights and autonomy of research participants. Tuck and Guishard (2013) argue for a relational ethics framework that values partnership, commitment, accountability, and social justices as core tenets.

With its deep roots and strong commitments, contemporary critical PAR builds on its history to branch out into new spaces, create innovative methods, revive forgotten ones, and push the boundaries of what it means to be an activist-scholar. Training grounds have emerged to support those interested in doing this type of research. Annually, people from all over the country and many from other countries come together at the Public Science Project for five days of seminars, roundtables, and hands-on workshops with seasoned PAR researchers. It is at these institutes, as well as at community lunches, in think tanks, and during virtual conversations that scholars, educators, students, activists, and community members who work the hyphens between these identities continue to push PAR theory and practice (see publicscienceproject.org). It is in these

spaces that PAR scholars contribute to and contend with future areas for consideration.

Future Questions for Consideration

Some questions for PAR researchers to consider include¹:

- How can PAR researchers use technology to broaden notions of participation? How can technology increase access in local settings, as well as open the possibility for global collaborations?
- How can technology (mobile data collection with real-time analysis, “big” data, apps, etc.) be used to speed up or scale up cycles of research, action, and reflection?
- How can PAR researchers utilize meta-analysis across PAR projects united by a common theme (i.e., educational equity) to create a cycle of reflection, research, and action on this level?
- How can PAR theorists and practitioners engage in an education campaign for IRBs to remove institutional barriers to this form of research? How do PAR researchers outside of the academy deal with ethics?
- How can the use of theory of change modeling push PAR theory and practice?
- How can funders support the use of PAR by adjusting proposal formats to allow for participatory research processes and for adequate time to engage its cycles of reflection, research, and action?
- How can we incorporate PAR and the arts, where arts aren’t used only as a potential product, but integral to the process?

Note

1. Special thanks for feedback/contribution to the future questions for consideration from Jennifer Ayala and Eve Tuck.

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Qualitative Disaster Research

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Abstract

This chapter provides an overview of the history of qualitative disaster research since the 1920s. Challenges associated with conducting disaster research, particularly field-based studies, are presented. The chapter also discusses ethical challenges related to homeland security and the emotional impacts of disaster research on human beings. Sections then lay out issues specific to the life cycle of disasters (preparedness, response, mitigation, and recovery), data-gathering techniques commonly used (interviews, documents, observations, visual data), and strategies for data analysis. A final section links efforts to strengthen the trustworthiness and credibility of qualitative research to disaster studies.

Key Words: Qualitative disaster research, quick response research, naturalism, feminism, interviews, visual data, observations, documents, data analysis

Today, disaster researchers worldwide conduct studies that generate theoretical insights and practical value for academia, disaster managers, non-governmental organizations, community-based organizations, and government agencies. Much of that research arrives through scholarly journals, textbooks, and technical reports on the desks of users through qualitative research methods. Indeed, qualitative research methods have characterized the field of disaster studies since its inception. Samuel Prince (1920) first used observations, documents, and interviews to investigate social responses to a massive explosion in Halifax, Canada. Subsequent efforts by disaster scholars and disaster research centers have continued the qualitative tradition. Researchers have produced an impressive body of knowledge from hundreds of qualitative studies, including many undertaken during arduous field investigations.

This chapter aims to introduce readers to the rich history of using qualitative methods for disaster research, to describe some of the challenges unique to conducting qualitative disaster research (QDR),

and to outline methodological approaches and data analysis techniques.

A Brief History of Qualitative Disaster Research

Imagine the setting: in 1917, a massive ship, the *Mont Blanc*, entered the harbor of Halifax, Canada, intending to deliver munitions. It collided with another vessel, resulting in a massive explosion that killed 1,963 people or 22 percent of Halifax's population (Scanlon, 1988). Tasked with conducting dissertation research, a young scholar named Samuel Prince used interviews, observations, and documents to gather and analyze data. Where would you begin as the first scholar interested in studying what happened? Prince laid a foundation for later scholars who studied topics such as emergence, convergence, blame and scapegoating, relief distribution, and recovery (Scanlon, 2002). His efforts earned him a doctorate, setting a precedent for today's Samuel Prince Dissertation Award, given by the International Research Committee on Disasters.

Fast forward now, to the 1950s in the United States. To set the stage for understanding the big leap made by QDR, remember that this is the Cold War era, a time of significant tension between superpowers, particularly the United States and the Soviet Union (USSR). Suspicion and conflict brought the threat of nuclear weapons into play. Government agencies in the United States wanted to know what might happen should a major nuclear strike occur. Lacking a natural research laboratory in which to conduct such an investigation, funding agencies turned to disaster researchers for a similar and related context. How would people react when faced with disruptions to everyday life? Would they panic? Engage in looting to feed their families? Succumb to disaster shock? Furthermore, how would organizations manage threats to normal operations? How would they interact? Would collaborations and communications break down? Would organizations fall into disarray or would they rise to the occasion? Similar questions and disaster efforts would emerge in Canada and France (Quarantelli, 1987*a*) and in other countries through the study of physical hazards and the contributions of geographer Gilbert F. White (Hinshaw, 2006).

In the United States, the National Opinion Research Center at the University of Chicago secured funding (from 1950 to 1954) from the Chemical Corps Medical Laboratories of the Army Chemical Center in Maryland (Quarantelli, 1987*a*). This research developed out of a belief that “one could learn about the probable wartime behavior of a population from studying how they responded to natural and industrial disasters” (Quarantelli, 1987*a*, p. 289). Researchers sought expert accounts by conducting field studies on an earthquake, airplane crashes, and a tornado. The National Academy of Sciences (from 1951 to 1957) and later the National Academies of Science (NAS) Disaster Research Group (from 1957 to 1962) also conducted similar field research, studying a fireworks explosion, tornadoes, floods, and topics from general human behavior to warnings, convergence, social and organizational responses, and the effects of disasters on children (Quarantelli, 1987*a*).

Noted disaster scholars and sociologists E. L. Quarantelli, Russell R. Dynes, and Eugene G. Haas co-founded the Disaster Research Center (DRC), the first of its kind, at The Ohio State University in 1962. From its inception, the DRC has always valued and relied on QDR. With funding from federal agencies, DRC continued to examine

sociobehavioral and organizational response to disasters. The DRC sent both faculty members and graduate students into the field to gather data. One of the first of these students, sociologist Bill Anderson, later went on to work at the National Science Foundation (NSF), the World Bank, and the NAS. Anderson then led efforts to fund qualitative research and link disaster studies to practice. Killian (1956) first wrote on methodological challenges for field studies in disasters for the NAS-National Research Council. He stated that “the method which has been most widely used in disaster field studies is the personal interview” (Killian, 1956, p. 21).

A tradition of producing multiple generations of disaster researchers skilled in qualitative methods continued not only at the DRC but in other institutions as well. DRC alumni and sociologist Dennis Wenger went on to found the Hazards Reduction and Recovery Center at Texas A&M University (now directed by sociologist Walter Peacock). The DRC is now located at the University of Delaware, where it celebrated fifty years of QDR traditions in 2012 (www.udel.edu/DRC).

Gilbert White, a noted champion of mitigation and a geographer, established the Hazards Reduction and Recovery Center at the University of Colorado – Boulder (www.colorado.edu/hazards). White established an annual event called the Natural Hazards Workshop in which practitioners and researchers network and share. One of his successors, sociologist Dennis Mileti, continued White’s tradition and led efforts to inventory social science disaster studies (Mileti, 1999). Mileti’s students have gone on to create their own centers open to QDR. As one example, sociologist Lori Peek (co-director of the Center for Disaster and Risk Analysis at Colorado State University) has written extensively on children’s experiences with disasters, anti-Muslim backlash, parenting, and disabilities (Peek, 2011; Peek & Stough, 2010; Peek & Fothergill, 2008). Other US centers include the Center for Disaster Research and Education founded by sociologist Henry W. Fischer, a graduate of the University of Delaware’s Disaster Research Center (DRC), and the Hazards and Vulnerability Research Institute, directed by geographer Susan Cutter at the University of South Carolina.

Another driver in the creation of a cadre of qualitative disaster researchers came from the NSF, which funded a series of grants that linked established scholars with emerging researchers. The effort emerged through the leadership of Bill Anderson,

who observed that the field of disaster research was “graying,” with fewer new scholars in subsequent cohorts (Reitherman, 2011). Focusing on the “Next Generation for Enabling Hazards Researchers,” Anderson consulted with Dennis Wenger (then at Texas A&M University) to develop a program to mentor new scholars. Assigned mentors assist emerging researchers to enter the field and secure research grants using qualitative and/or quantitative research designs. After Anderson’s departure from NSF, Wenger moved to the NSF to serve as a program officer overseeing disaster research. Clearly, a tightly networked and focused tradition has produced a powerful set of researchers seemingly rare within any specialization. Such an effort has functioned to foster a strong and valued tradition in QDR in the next generation of students and colleagues.

Adopting an Inductive Approach

Scientific inquiry usually proceeds through either inductive or deductive approaches. Deductive reasoning, a standard approach commonly used in quantitative inquiry, begins by examining what we know about a given topic. Literature searches generate evidence on the topic as researchers glean information on the independent and dependent variables they wish to study. Researchers then develop hypotheses that carefully propose a relationship between variables, design appropriate surveys (or similar quantitative methods) to test that relationship, gather and subject data to rigorous statistical testing, and state their findings. Although disaster researchers also use deductive and quantitative research, those who follow qualitative traditions typically craft studies using an inductive approach.

Inductive research typically relies on generating understanding from the data. Although debate exists on the extent to which one should know the literature (which some argue results in potential bias), most researchers usually know thoroughly the literature in their field. Inductive theorists listen carefully to what they are hearing, observe closely what takes place, and investigate vigilantly any document or visual data they uncover. Data then lead researchers to develop working hypotheses to explain what they see. Rather than test static hypotheses and then move on to collect and analyze more data, as is common with deductive approaches, inductive theorists gather more data to refine their working hypotheses. When they reach theoretical saturation, the point at which they can predict what the next interviewee will say, it is time to begin writing up the results into a robust explanation or theory based on

those data (Glaser & Strauss, 1967; Strauss, 1987). Consistent with the scientific method, qualitative disaster researchers then present and publish their work, subjecting it to the scrutiny of their peers. Their work then feeds into the body of knowledge that starts the inductive process all over again with the next study.

Knowing the literature aids a disaster researcher as he or she goes into the field because it provides “theoretical sensitivity” (Glaser, 1978; Strauss, 1987). Students learning qualitative research and disaster studies often find theoretical sensitivity a frustrating and elusive quality as they strive to become researchers. Imagine, though, that you are in the field at a disaster site and focused on the problem of warning response. As you interview appropriate individuals, you listen carefully not only for their answers to the questions, but also to how what they say fits with the extant literature. Are you hearing something that researchers have already reported in the literature? That is good because it confirms the body of knowledge. Or, are you hearing something new, something that scholars have never published previously? Being able to distinguish between what scientists know and what they have not yet published is theoretical sensitivity. It is the ability to know quickly what data confirm or negate the literature and/or how the data (e.g., interviews, observations, documents) deepen or broaden what we know. Familiarity with the literature is a standard approach for qualitative disaster researchers, and time spent reading journals and key works serves novices quite well.

Although qualitative research traditions today enjoy strong support, such an encouraging tradition has not always been the case. Divisions between the “hard” sciences (which tend to use traditional, positivistic, and deductive approaches) and the social sciences have opened schisms and produced critiques of qualitative approaches. In contrast, however, to the notion that social science disaster research is yet another example of a “soft” science, researchers who conduct disaster inquiry might actually be considered as among the most creative and adaptive inductive theorists around.

Qualitative disaster researchers occupy a unique niche, conducting inquiry on the complexities of human behavior and often in a chaotic environment. Their techniques must be academically rigorous and stand up to scientific scrutiny by multidisciplinary reviewers. Furthermore, these researchers must be able to navigate entrée into sometimes dangerous locations, adapt research techniques appropriately

when traditional techniques fail, and collect and preserve data in challenging environments.

Qualitative disaster researchers also occupy a niche between those who expound the value of knowledge for the sake of knowledge (e.g., “basic” research) and for the practical value that it generates (i.e., “applied” research). One way to conceptualize QDR is to place it squarely between pure basic and pure applied research, or, as described by Stokes (1997), a type of “use-inspired basic research” found in “Pasteur’s quadrant.” Such efforts offer both theoretical explanations for something like sociobehavioral response to warnings and insights for emergency managers who try to safeguard the public. Evidence-based findings offer immediately usable applications that can truly make a difference. The journey to such an outcome continues throughout this chapter.

Conducting Disaster Research

This section looks at the special challenges associated with QDR. To begin, we first distinguish between key concepts that may have implications for the scope of the event under inquiry: emergency, disaster, and catastrophe. Following the definition of these important concepts, the section reviews four phases of a disaster that most research falls into. Finally, the section outlines ethical concerns specific to emergency management, homeland security, and QDR.

What Is a Disaster?

Rigorous scientific inquiry begins with defining key concepts. Defining a concept provides boundaries for what researchers do and do not study and communicates important information to other scientists (for an extensive discussion, see Quarantelli [1995; 1998], also Perry & Quarantelli [2005]). For example, most qualitative disaster researchers distinguish between several key concepts. Many researchers view crisis occasions as playing out along a continuum from emergencies to disasters to catastrophes. *Emergencies* occur in fairly limited or bounded areas, such as a car accident. The implications of such boundaries suggest what assets responding agencies need to deploy, which are usually limited to dispatching emergency medical personnel and possibly firefighters and police. First responders usually handle these routine types of events without any additional resources.

Moving further along the continuum, *disasters* require additional assets and personnel. A tornado that strikes a neighborhood will likely require many ambulance, fire, and police units. Should the

disaster be large enough (such as the EF5 tornado that struck Joplin, Missouri, in 2011 or the wildfires that destroyed 350 homes in Colorado Springs in 2012), community disruption will occur including business closures, school cancellations, utility disruption, infrastructure failures, and more. To distinguish between emergencies and disasters, then, means assessing the level of community disruption—in a disaster, normal routines do not occur and social institutions cannot function. Affected communities may also require external assistance from surrounding areas, the state/province, or even the nation (Quarantelli, 1995).

At the far end of the continuum lie *catastrophes*. Hurricane Katrina in the United States, the 2011 Japan tsunami, and 2010 Haiti earthquake qualify as catastrophic events. In these cases, regional agencies and organizations cannot supply the resources needed to help. National responding organizations also experience challenges in responding, even in well-developed nations with normally rich resource bases. International organizations (government and nongovernmental) will be compelled to assist (Quarantelli, 2006).

Most of the time, most studies fall into the category of disasters. Studies have been published, though, that range across the continuum. As discussed elsewhere in this chapter, large events often fuel funding and drive change. The events of September 11, 2001, for example, produced significant organizational changes in numerous nations newly focused on homeland security concerns. Funding also became available to study not only sociobehavioral response to terrorism but also the socio-organizational change that followed (for one collection, see Monday, 2003). Researchers do study more commonly occurring disasters, but usually not in as much breadth or depth. Qualitative disaster researchers are part of a team of researchers generating fuller insights across the continuum, which sheds insight into the disaster most likely to happen, as well as the one that may cause the most disruption and societal change.

The Life Cycle of Disasters

Many nations have organized disaster management into a series of stages with associated tasks (see Figure 26.1). New Zealand, for example, relies on the Four R’s: readiness, response, recovery, and reduction. In the United States, the National Governor’s Association (1979) devised a four-phase model as well, historically organized into preparedness, response, recovery, and mitigation. The four

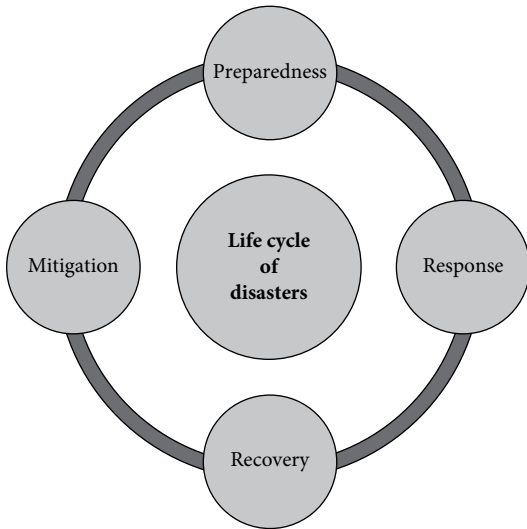


Figure 26.1 The life cycle of disaster research.

phases, although not discrete (Neal, 1997), do provide a point of organization for disaster managers and a rubric under which to describe disaster studies.

RESPONSE

The bulk of disaster studies fall into the response period, understandably the most exciting phase to investigate and the one for which the vast bulk of granting agencies provide funding. Big events tend to drive funding and, not surprisingly, events like the attacks of September 11, 2001; the 2004 Indian Ocean tsunami (Phillips, Neal, Wikle, Subanthore, & Hyrapiet, 2008); hurricane Katrina in 2005 (e.g., see Natural Hazards Center, 2006); and the earthquake that struck Haiti in 2010 represent such examples. Concern exists, though, that such large-scale or catastrophic events can dramatically influence the findings and potential use-value of the research. Most of the time, most communities experience smaller scale events that range from emergencies (when fire, police, and ambulances respond) to disasters (that disrupt community functioning and require interagency coordination; see Quarantelli, 1988). This continuum extends to catastrophic events at the far end of the spectrum. Risk thus exists in applying the findings from catastrophic events to events of smaller scale. Imagine, for example, findings that direct agencies to expend precious, limited staff time and resources on a particular hazard, such as terrorism. Emergency managers have argued that floods—the most common hazard in the United States and usually of smaller scale—have been overlooked. Such a concern also compels researchers to think about their own work.

To produce use-inspired basic research, shouldn't investigators look into a range of events and hazards? A wider array of studies, broadened by examining disasters across varying scales, scopes, and magnitudes, should produce a more robust and useful set of findings.

Another challenge associated with response-time funding is the relatively unexpected nature of disaster events. Although “seasons” exist for cyclones, hurricanes, tornadoes, blizzards, heat waves, droughts, and flooding, disasters can occur outside of anticipated times. Many disaster researchers compensate by designing studies with a specific research question and then waiting for the right moment, sometimes for years. For example, if one is interested in social media and disasters, one could design a way to gather Facebook posts and tweets in order to understand the types of information that people disseminate and the related spikes in user interest (Bennett, 2012).

Some funds exist specifically for quick response efforts. One fund exists at the Natural Hazards Research and Applications Information Center at the University of Colorado – Boulder. These funds can provide for limited travel expenses to get the researcher into the field. Compelling reasons exist to do so, as data may perish before the researcher arrives, such as the correct numbers of people in a shelter or relief camp (Stallings, 2006). In the aftermath of an event, most shelter managers pay attention to serving people rather than capturing accurate data. Few would even think that a researcher might need it collected in specific ways, such as the numbers and types of languages spoken, people with disabilities, and pets; and significant demographic data such as income, education, race, ethnicity, sexuality, and neighborhood of origin. Furthermore, people with critical information (such as search-and-rescue teams) may leave the field as rapidly as they arrived—and researchers will miss their chance to collect data. Researchers have to be in the field to get the data, usually as rapidly as possible. Experienced researchers keep organized backpacks ready to go, complete with research instruments, recording devices, institutional review board (IRB) consent forms, and other necessary field resources (for an early list, see Quarantelli, 2002). Getting to the field quickly enables researchers to identify research questions (such as looting after Katrina) and determine that such myths of human behavior remain largely unfounded (Rodriguez, Trainor, & Quarantelli, 2006).

PREPAREDNESS

After response, preparedness represents the next most studied phase (Mileti, 1999; Tierney, Perry, &

Lindell, 2001). Preparedness can be easier to study because the disaster has not caused people to experience displacement. Indeed, research participants can agree to join a study at their convenience rather than during a time of significant household, organizational, and community disruption. Preparedness typically includes looking at how households, organizations, and communities organize before a disaster. Preparedness includes emergency operations plans (EOPs), standard operations procedures (SOPs), and event-specific planning (e.g., terrorism, hazardous materials accidents). Preparedness can also include looking at public outreach to educate the community, organize neighborhoods, train response teams, and conduct exercises and drills (e.g., see Tierney et al., 2001).

RECOVERY

Recovery can be split into short- and long-term recovery periods. During short-term recovery, researchers look at topics such as utility and infrastructure restoration, debris management, temporary housing, and postdisaster recovery planning. Longer term recovery research, which methodologists call “longitudinal” or taking place over a long period of time, is far less likely to be undertaken (Mileti, 1999). Structural reasons include the need for faculty members to secure tenure and promotions that require publications. Longitudinal research, which may not result in publications for some time, can threaten employment. Investigators also find it challenging to secure funding for longitudinal research.

Furthermore, the manner in which disasters affect people and organizations also influences the capability to carry out research. Displacement means trying to find and follow people who may move 6–10 times before finding permanent housing or rebuilding. Organizations also change and adapt and personnel leave. Santa Cruz, California, affected by the Loma Prieta earthquake in 1989, experienced a near 30 percent turnover rate in city staff (Wilson, 1991). Long-term recovery research does occur but often with significant barriers and challenges. Investigators using qualitative interviews to study long-term recovery after Hurricane Andrew in Florida (which occurred in 1992) discovered that racial and ethnic minorities experienced elongated returns to “normalcy,” with some never accomplishing such a return (Dash, Morrow, Mainster, & Cunningham, 2007).

MITIGATION

Mitigation includes both structural and non-structural measures taken to reduce the risk of

future damage. Engineers are more likely to create and study structural measures such as dams, levees, barricades, blast-resistant buildings, and structural retrofits. Social scientists typically study nonstructural measures, such as building codes and enforcement, insurance, building warning systems, and policy analysis. A recent exception comes from the work of Freudenberg et al. (2009) who used archival and document research to look at the reasons why the City of New Orleans constructed various levee and waterway systems. Finding that economic interests fueled such structural efforts led them to conclude that the story of hurricane Katrina “is not one of nature striking humans. It is the story about humans striking nature—and then enduring the tragic consequence” (Freudenberg et al., 2009). Co-author Shirley Laska received the American Sociological Association’s Public Understanding of Sociology Award for her efforts to bring such concerns to attention and to safeguard the public. As founding director of the Center for Hazards Assessment, Research and Technology at the University of New Orleans, Laska has been at the forefront of generating qualitative insights on communities affected by disaster.

In a perfect research world, multidisciplinary teams would investigate mitigation to assess the full range of mitigation measures. However, because academics tend to concentrate within their own discipline, such creative and needed research remains underexamined. Important exceptions do occur and often through funded projects such as the NSF’s Engineering Research Center program. Funded interdisciplinary work has included understanding earthquake impacts, related policies, and mitigation efforts (<http://www.casa.umass.edu>) and tornado warning systems (<http://www.casa.umass.edu/>).

Ethical Issues in Disaster Research

Disasters produce challenging emotional times, although, interestingly, most people respond well (Norris, Friedman, & Watson, 2002a; Norris et al., 2002b). Concern over the emotional and psychological impacts of disasters form the basis of what IRBs consider after a researcher submits a request to conduct research.

The first rule is, in any type of study, *do no harm*. Do disaster studies harm people? Indirect evidence suggests that the answer is no. Consider that people who come to clinics for psychological support have preexisting conditions; studies in which they participate, based on research, do not seem to cause significant additional harm. Indeed, some authors

suggest that “trauma-related studies report favorable perceptions of the cost-benefit balance” (Newman & Kaloupek, 2004, p. 392). In short, people may experience benefits from talking about their experiences, seeing the contributions they have made, and helping to produce scientific knowledge of value to the broader society (Collogan, Tuma, Dolan-Sewell, Borja, & Fleischman et al., 2004).

A few studies on disasters have looked into ethical concerns. Two events have driven this discussion: the Oklahoma City bombing of 1995 and the events of September 11, 2001. The domestic terrorist attack in Oklahoma City killed 168 people, including 19 children inside the daycare center in the Alfred P. Murrah federal building. Dozens of others sustained serious injuries, and those in surrounding areas felt the explosion. The governor of Oklahoma determined that the University of Oklahoma Health Services should oversee research requests to ensure that survivors would not be retraumatized. The resulting IRB required researchers to conduct “only methodologically acceptable research that either promised direct practical benefits or demonstrated a clearly clinical focus” (North et al., 2002, p. 581). The IRB governed strict requirements for access to children and established a comprehensive registry from which approved researchers could draw samples.

In contrast, similar efforts were not taken after September 11, 2001, perhaps because of the complexity of coordinating access to multiple sites across multiple locations. Researchers converged on affected areas, particularly New York City, and had limited access to the Pentagon in Washington, DC. A conference held in 2003 featured discussion on “Ethical Issues Pertaining to Research in the Aftermath of Disaster” in which several recommendations emerged (Collogan et al., 2004). One recommendation was to develop partnerships with local researchers so that a researcher could become familiar with local culture, as well as with sociopolitical and economic contexts (North et al., 2002; Rosenstein, 2004). A second approach sought to train research teams on how to conduct ethical research, particularly in a situation in which people and researchers may both experience stress (Fleischman & Wood, 2002). A third recommendation was to collect data on the ethics of disaster research so that future investigators and IRBs could offer sage advice.

An emerging ethics matter concerns issues associated with homeland security. Significant amounts of funding have gone into highly secretive research,

ranging from efforts to detect and develop resistance to various terrorist attacks to influenza strains usable as biomedical weapons. The extent to which researchers can publish their findings varies, and this can become quite controversial. An inherent conflict exists between the real need for national security over such research and the scientific method, which directs researchers to subject their work to peer scrutiny or “publish or perish” within the academy. Although most life science-type research remains at some distance from social science disaster research, the reality is that we all must practice responsible conduct for our research. In a 2005 report, the NAS, looking into life science research, noted that experts concurred on the basic rule to “do no harm.” However, “beyond that, it is proving difficult to achieve consensus” (Atlas, 2006). The NAS committee took on the task of examining concerns for homeland security-type of research. Most social scientists would generally agree with the principles the NAS discussion derived, to provide:

- Safety and security, especially in laboratories and with pathogens and related data;
- Education and information of relevant laws affecting scientists;
- Accountability, including notifying authorities of concerns;
- Oversight, including serving as a sound role model for research processes (Atlas, 2006).

Qualitative Disaster Research Methodology

This section lays out some of the key issues important for understanding how social scientists conduct QDR. A number of approaches exist to guide research. Historically, most QDR studies seem consistent with the “naturalistic paradigm.” Discussing the naturalistic paradigm serves as the point of departure in this section as it explains how researchers, including those doing QDR, tend to approach their research settings. A brief overview of feminist disaster research approaches then follows. Finally, a description of sampling procedures ends the section with procedures specifically relevant to QDR.

A Naturalistic Paradigm

Lincoln and Guba (1985) deserve credit for laying out basic ideas for a new paradigm called *naturalism* (see also Erlandson, Harris, Skipper, & Allen, 1992). The basis principles that underlie naturalism are consistent with how many qualitative disaster researchers approach the field. First, research design

is something of a journey that may take unexpected turns. It may be difficult to reach the scene or find the people affected. Researchers must therefore practice a flexible, inductive approach that allows them to think, rethink, and visualize the study in new and sometimes creative ways. Researchers must be attentive to what is theoretically critical as they enter a disaster setting, being sure to record data critical to the context (Erlandson et al., 1992).

As with much of the behavior we see in disaster settings, a naturalistic approach is thus emergent. Several key aspects prove important to consider when planning a QDR study (Erlandson et al., 1992; Lincoln & Guba, 1985). First, one negotiates the entrée process. One has to move past gatekeepers, including the military or police who want to keep people from a scene. Many times, their efforts ensure public safety, including that of the researcher. But it is equally true that the person staffing the barricade may not understand the value or significance of the work—even if you do have a grant from the NSF or Federal Emergency Management Agency (FEMA). Securing advance credentials from funders and agencies can help with this, although this takes advance preparation. With disasters difficult to predict (where and when), negotiating entrée can require diligence, persistence, and patience. A second element is to design a purposive sample selection (a nonprobability strategy; see upcoming section) in which the researcher carefully and mindfully chooses people to interview. The third step dedicates the researcher to planning data collection. Despite the inherently surprising nature of many disasters, it is possible to design general (and sometimes very specific) interview guides, observational checklists, and lists of documents and visual data to collect.

Killian (1956; 2002) realized the need for some degree of flexibility consistent with what would become the naturalistic paradigm. In reporting to the NAS Committee on Disaster Studies, he described how disaster field studies would challenge positivistic, deductively driven approaches and recommended: “data should be collected and analyzed in such a way that new and unexpected relationships can be discerned and the possibility of identifying unforeseen variables is not excluded” (1956, p. 6). Killian further advised collecting descriptive data to reveal the sequence of events and the context in which activities unfolded. A follow-on report in the NAS series (Disaster Research Group, 1961, p. 2) confirmed the emergent approach “in which the behavioral scientist goes to or near the site of

an actual or potential disaster [and] selects some aspect or problem of human behavior in the event.” Each publication admonished researchers to craft their studies diligently, with an eye toward potential exclusion or bias that can result.

Researchers must therefore always plan for study quality, particularly striving to use strategies that enhance the credibility and trustworthiness of the research (see Erlandson et al., 1992; Lincoln & Guba, 1985). Most researchers triangulate their work, which means using multiple methods simultaneously. One also has to consider the “logistical plan” for the study, from quick response aspects of the work (in the field, fast, to gather perishable data) to longitudinal elements that require well-trained teams and funding (Erlandson et al., 1992, p. 69). The flexible, emergent nature of the effort and the dynamic conditions that unfold in a disaster context then require that researchers revisit their research design frequently. One technique (discussed later) is *peer debriefing*, when research teams meet regularly in the field to identify barriers, issues, problems, and movement forward as the study unfolds and conditions change. The starting point for a naturalistic paradigm study is deciding where to go and who to study.

Feminist Disaster Research

Feminist scholars have collaborated to produce and push forward an extensive set of findings related to gendered experiences in disasters. Their work, organized in part at the Gender and Disaster Network website (www.gdnonline.org), has brought a new lens to the field with which to view disaster impacts. Disasters are clearly not equal opportunity events, with people dying in numbers disproportionate to their population. The 2004 tsunami, for example, resulted in more numbers of women and children dying than men. In contrast, flooding from the levee failures in New Orleans took the lives of mostly older, African-American men (Sharkey, 2007).

Feminist researchers start with the understanding that gender stratifies societies and that this stratification has clear implications for life chances. Although social scientists view life chances as the probabilities that one will benefit from what society has to offer, the term carries even stronger implications in a disaster event. Feminist QDR finds that women are more likely to believe and want to respond to warnings, bear an increased burden of relief and recovery activities for their families (including increased caregiving, standing in lines,

child care), and be excluded from recovery activities, planning, and decision making (for a thorough review, see Enarson, 2010). Gender segregation has marked the profession of emergency management as well, with authors pointing out the exclusionary bias that such practices can produce. For example, feminists and advocates for those at risk for domestic violence, sexual assault, harassment, and human trafficking repeatedly uncover instances where safety remains compromised in the postdisaster period (Enarson, 1999; Fisher, 2009). The efforts brought to QDR by feminist methods have revealed significant findings for the disaster body of knowledge and for emergency management practice. Feminist research also emphasizes that traditional studies polarize “researchers and research subjects as a false binary that is inherently flawed, artificial, and ultimately undesirable” (Hesse-Biber & Leavy, 2011, p. 23). To counter the oppositional nature of scientific research and the oppression that can result, feminist disaster scholars encourage a more participatory approach to research, one that liberates marginalized people.

The Gender and Disaster Network established the Mary Fran Myers Award in 2002 to honor work that “recognizes that vulnerability to disasters and mass emergencies is influenced by social, cultural, and economic structures that marginalize women and girls. Research and practice that reduces women’s and girls loss of life, injuries, and property can make a difference. The goal of the Gender and Disaster Network is to promote and encourage such research and practice” (for information visit http://www.gdnonline.org/mfm_award.php).

A need to continue to diversify QDR remains, including an emphasis on and understanding of intersectionality theory. This approach points out that gender alone cannot explain differential impacts, which need to be placed at an intersection of gender, race, income, disability, and development status, for example. Simply put, the effects of disasters are not the same for a woman in Haiti as for a woman in the United States—nor for a woman in the United States who is a janitor compared to one who is a corporate executive (Phillips & Fordham, 2010). Queer theory also remains underused in QDR, with only a few published accounts of how lesbian, gay, transgendered, and bisexual individuals and families experience disasters (D’Ooge, 2008; Eads, 2002; Stukes 2013). To overcome such gaps in the literature, researchers must actively select for participation people from marginalized, overlooked populations and communities.

Sampling

Sampling is the selection of units (i.e., people, organizations, locations, events) for inclusion in a research project. Qualitative disaster research can use either probability sampling or nonprobability sampling.

PROBABILITY SAMPLING

Probability sampling means that every unit (a person, a household, an organization) has the same chance of selection, particularly if random sampling is used. Although probability sampling occurs most frequently within quantitative research, such as surveys, it has been used for QDR. For example, a study that looked at shelters after hurricane Katrina generated an extensive list of nearly 1,000 shelter locations across four states (Texas, Louisiana, Mississippi, and Alabama; see Phillips, Wikle, Head Hakim, & Pike, 2012). Researchers then stratified the sample by type of shelter (officially designated as a Red Cross shelter or as an unofficial, “emergent” shelter) and then enumerated the strata. As is common with research, limited funding coupled with the costs of and time for extensive travel to shelters required a reduction in the total number of shelters visited. To select cases impartially, researchers used a table of random numbers to select shelter sites. Because funding arrived several months after the event (common in QDR), researchers had to substitute closed shelters with those that remained open. To do so, they used a spatial zone sampling technique (discussed shortly). One can easily imagine the additional challenges associated with probability sampling in a rapid-onset event, such as where such events occur, if they meet the parameters for inclusion, and how one knows when the sampling frame has been completed so that one can begin selection.

NONPROBABILITY SAMPLING

Nonprobability sampling suits QDR well for several reasons. First, it allows the researcher to determine who best to study. Probability sampling could easily miss the most critical persons needed in the sample, such as the emergency manager, key leaders in a recovery committee, or the person tasked with debris removal (Stallings, 2006). Intentional selection therefore increases the chance that researchers will gather deep, rich, and meaningful data useful in generating inductive theory. Second, nonprobability sampling allows the researcher to refine and adapt the sample as one enters the study. Starting a research project in a new location with new organizations likely means that you do not know exactly

who you should ask to participate. Organizations do vary in structural configurations, as well as in where they place key personnel. Disasters, and especially catastrophes, also prompt emergent behavior in both people and organizations. Emergence, defined as newly appearing behavior or social structures, often occurs when disasters or catastrophes generate unmet needs. In Haiti after the earthquake, for example, military units external to the nation restored the main airport so that relief personnel and supplies could arrive. After hurricane Katrina in the United States, numerous long-term recovery committees developed across the region. Local leaders, government representatives, and faith-based organizations (FBOs) provided time on the committees, which varied considerably in structural arrangements, assigned tasks, and procedures. People moved in and out of the committees and in and out of the affected areas. Simply put, researchers cannot always anticipate who might take on disaster tasks or how and where they might secure interviews. The adaptability of nonprobability sampling therefore allows the researcher a higher chance of securing needed data from informative participants. The adaptability afforded by naturalistic qualitative research approaches means that researchers can gather data appropriately.

Several techniques can be used in nonprobability sampling. *Snowball sampling* occurs probably most frequently, in which a researcher asks an interviewee who they should talk to next. Imagine, for example, doing a study of how people who are deaf experienced a disaster. One would need to tap into the deaf community and those institutions (schools, worship locations, community centers) used by people who are deaf—and referrals help tremendously. Researchers, though, must ensure that they do not tap into an established social network without investigating the breadth of responding organizations within the deaf community. Beyond people, we may use *spatial zone sampling* to identify potential sites (Killian, 2002). The impact zone is one such area of interest, although access can be challenging to secure. Entrée into Haiti was very problematic for responding organizations, not to mention researchers. Safety can also be a problem; for example, after the Japan tsunami, radiation created concerns for anyone in the area. A second area is called the *filter zone*, where people and supplies move into and out of the impact zone. Researchers might find it useful to station themselves at such as places where medical teams assess individuals with injuries or medical conditions. It is also possible to conduct

QDR outside of these areas, such as in communities that host displaced evacuees (Bell, 2008; Stough & Sharp, 2008). In the shelter study for Hurricane Katrina, a number of shelters had closed by the time that researchers secured funding and arrived to use their probability sample. To replace closed shelters, researchers selected the next geographically closest shelter of the same type (e.g., a formally designed Red Cross shelter; see Phillips et al., 2012).

Researchers would also be interested in sampling the type of event that occurs (Schatzman & Strauss, 1973). Routine events (like ambulance runs) occur on a regular basis; special events tend to occur at irregular times, such as fire drills; untoward events, the kind that occur unexpectedly, such as earthquakes, interest disaster researchers the most. As noted earlier, the researcher must take care to consider the scope and magnitude of an event, particularly when comparing one's findings to the extant literature.

A final type of sampling includes time (Killian, 2002). Although emergency operations centers (EOCs) operate 24/7, the numbers of personnel and activities may vary as the event transitions from response to recovery (Neal, 1997). Public shelters also vary their staff and procedures so that residents can sleep, eat, work, and play (Nigg, Barnshaw, & Torres, 2005; Pike, Phillips, & Reeves, 2006; Yelvington, 1997). People also experience varying lengths of time away from their homes following a disaster, a particular problem for socially vulnerable populations (Dash et al., 2007).

Qualitative Disaster Research Methods

A method is defined as the toolset used to gather data (Hesse-Biber & Leavy, 2011), and QDR methods are no different from the methods in any discipline. However, the context in which researchers conduct QDR work presents particular challenges worth noting here (Stallings, 2006). Typically, disaster researchers rely on four main qualitative methods: interviewing, observation, documents, and visual research (Quarantelli, 2002).

Interviewing

Qualitative interviewers value how they establish a relationship with a respondent, a process particularly important to feminist scholars (Hesse-Biber & Leavy, 2006). Researchers must develop a sense of rapport, or meaningful connection, with their respondents in order to elicit productive data. Researchers view participants as active players in jointly producing knowledge. Making that

connection to a participant may be far easier in the preparedness, mitigation, and recovery phases when more time exists to connect with a participant, ensure their comfort with the interview process, and move into a “guided conversation” designed to put participants at ease (Gorden, 1992; Rubin & Rubin, 2011; Weiss, 1994).

Disasters make for challenging contexts in which to accomplish such an interpersonal connection, especially during response time. First, finding a quiet location in which to conduct an interview may be impossible. Helicopters flying overhead create distractions and obscure recorded conversations needed for later transcription. Radios, cell phones, texting, and other communications interrupt the process, too, often with imperative requests that participants must answer. One must practice patience in finding a time and space in which a research participant can think clearly to produce useful content. On top of these challenges, a participant may be completely exhausted after having worked without rest, faced considerable stress, or been unable to sleep in an unfamiliar or noisy location. Despite these challenges, most researchers seem to have fairly good luck in securing key interviews during the response time. One common technique is to make contact, introduce yourself, and obtain a brief overview of what is going on. Many researchers then return later to follow-up with the interviewee (Quarantelli, 2002; Stallings, 2006).

A related challenge stems from being able to find that person at a later date. In a large-scale disaster, personnel move into and out of the area on various deployments or rotations. Search-and-rescue efforts, for example, occur fairly rapidly after an event. Getting to a location can be very challenging, as witnessed following the massive 1992 Guadelajara explosion (Aguirre et al., 1995). For scholars interested in rescue techniques, then, the time period within which interviews can be conducted occurs within a tight frame—or one must conduct interviews in a location completely separate from the disaster, such as the Urban Search and Rescue (USAR) team’s home area. Teams like USAR groups may be comprised of people from various departments or units spread out across a wide geographic area. Thus, getting to the site of action means access to the rescuers in one location (when they are available) or finding funding or a means (e.g., Skype) to conduct interviews later.

Another challenge of disaster research stems from culture and language. Most nondisaster researchers enjoy being able to conduct interviews

in their own language or location, but disasters can strike anywhere. The 2004 Indian Ocean tsunami, for example, impacted at least thirteen nations, with the majority of the deaths and devastation occurring in India, Sri Lanka, Thailand, and Indonesia. Each nation embodied diverse cultures and multiple languages. Navigating into the areas required a research team trained in cultural sensitivity with native speakers. Because interviewing demands the creation of meaningful relationships, one must know and be able to interact appropriately with local culture including eye contact, touching (e.g., a handshake), and personal space. Knowing what to say at a time of disaster matters as well. Studying Hindus or Muslims, for example, requires sensitivity on how to state a question and how to interpret the response, a circumstance that occurs in other faith-based traditions as well (Dynes & Yutzy, 1965; Smith, Kenneth, Pargament, & Oliver, 2000; Wilson & Moran, 1998).

Our research team conducted cultural immersion training prior to studying rescue techniques and mass fatality management in India. We did so by developing a research team with Indian graduate students trained in cultural geography, including one who spoke the languages used in the affected areas (Hyrapiet, 2005; Phillips et al., 2008). Investigating the Haiti earthquake went far better for researchers who knew Creole, French, and sometimes Spanish, as well as English. The Japanese tsunami, earthquake, and nuclear accident have proven equally difficult to research.

Overcoming these challenges should result in securing an interview with depth, breadth, and meaning. Interviews need to go well beyond the standard nominal “yes” or “no” level of responses secured on quantitative surveys. Deep understanding sheds light on the individual and his or her lived experience, which enables qualitative disaster researchers to secure use-inspired data that improve policy and practice. Imagine, for example, asking someone in New Orleans what it was like to re-enter the city after it had been dewatered. A quantitative survey might ask how many miles the respondent drove, the amount of time it took to get there, and his feelings rated along a Likert scale from 1 to 5. An interviewer verbally goes with the participant to create a narratively driven, visual picture for users. Surveys fail to capture the sickening feeling in one’s stomach while driving through once-familiar neighborhoods now devoid of traffic lights, street signs, and familiar landmarks. Neighbors are gone, the streets are silent, and—once night falls—frighteningly dangerous.

Walking inside the house, mildew can be seen spread across the walls, and none of the furniture is where it used to be. Family photographs can no longer be salvaged, beloved books used to teach classes are beyond repair, and mementoes from a past Mardi Gras are covered with dried muck. There's no working bathroom, no power (for months), and no place to find food or water in a city where hundreds of thousands used to live. Good qualitative interviews capture that silent pain sensitively in a manner that produces rigorous, subjective insights into life after Katrina. Such an understanding can assist trauma counselors, neighborhood associations, recovery committees, and others as they lay out a timeline and plan to restore and rebuild. The insider or "emic" view is indeed the hallmark of any qualitative research effort, including QDR (Baca Zinn, 1979). Thus, disaster scholars try to capture what people who face disasters think, feel, and do. This subjective, lived experience serves as the hallmark of all qualitative studies, including those on disasters.

Observation

Observation is one method that offers a range of implementation strategies. Researchers can choose from observing an event while remaining completely separate from the action or as a participant. Certainly, the choice one makes along such a continuum can influence how one perceives social interactions and social structures. Being an insider can enable you to be closer to what Goffman (1959) called "backstage" behavior, where people become more likely to do and say what they truly think and feel. Conversely, the potential also exists for participant observers to influence what happens, especially if they enter influential social positions. Managing one's role, then, is key to successful observation.

Let's consider several examples. In the aftermath of the September 11 terrorist attacks, faculty and students from the DRC moved into New York City. Their observations ranged from spontaneous donations arrivals (Kendra & Wachtendorf, 2001) to the reconstruction of an EOC (see Kendra & Wachtendorf, 2006). For the latter, researchers observed for hundreds of hours as staff (who had fled the collapse of the World Trade Center and suffered the loss of colleagues and friend) reestablished a working EOC. The study documented the process of organizational improvisation under catastrophic conditions, finding several forms of improvisation including reproductive (rebuild the EOC) and adaptive (newly appearing or emergent waterborne evacuation, see Kendra & Wachtendorf, 2006).

Moving along the continuum from complete observer to participant observer, Lois (2003) spent time becoming a member of a volunteer search-and-rescue team. She found it necessary to negotiate entrée, establish rapport, and negotiate her roles within the group. Lois moved through the process of becoming a member of the group, slowly learning the ropes, and determining her research problem. She ultimately settled on researching how search-and-rescue teams managed their emotions in difficult and trying circumstances.

Similarly, Desmond (2009) already worked on a wildland firefighting crew. In need of thesis data, he began to gather observations and interviews about the crew and its experiences. Akin to Lois, he used a naturalistic paradigm that allowed the subject to emerge over time. While documenting the ways in which wildland firefighter crews lived, trained, worked, and managed personal lives, he focused in on line-of-duty deaths. His personal level of participation led him to understand that crews and crew leaders did not always follow key rules and sometimes for good reason. Called the "Ten and Eighteen," two sets of rules establish what crews and crew leaders should do to remain safe. Fire behavior and fire conditions do not always fit consistently with those rules, sometimes requiring teams to alter adherence. Yet, when firefighter deaths occurred, investigators typically noted that rule violations resulted in the death.

Each of these examples demonstrates that access represents the most significant challenge in QDR. First responders, particularly firefighters, talk about the "brotherhood" in which they live, work, and rely on each other for their lives. Outsiders do not gain access easily inside these groups. One researcher who did find that she could hang out at the fire station to add context to her longitudinal interview design (Chetkovich, 1997). Chetkovich negotiated access within the fire administration. Increasingly, with barricades erected around disaster sites and facilities hardened to protect staff, gaining such entrée can be very difficult. It may be necessary to first establish relationships with people and organizations in areas likely to experience disasters or likely to be part of the response and recovery. Or, a researcher could study mitigation and preparedness phases where access may not be as difficult. Many communities, for example, conduct mitigation planning that must be open to public participation. Similarly, planning departments and city councils must contend with building codes, especially after a disaster. These more open sites also reveal opportunities for useful

insights. A team approach may facilitate entrée, with scholars and practitioners working together to conduct the research. Richard Rotanz (2006, p. 475), an emergency manager in New York, writes “let’s start here, in our university offices and from our emergency operations centers, and ‘pick up the phone.’ A simple introduction of each other will lead to a long-term relationship that will enhance our emergency planning and preparedness initiatives.”

Beyond access, observers must work to develop and maintain a rapport with those they observe. Doing so can be challenging again because of the potential for impacting people’s behaviors. Watching other people can make them nervous or uncomfortable. Desmond found this with his study of wildland firefighters. Several firefighters asked him if he was writing down what they said (he was). Ultimately, though, his prereseach relationship with the crew, coupled with time spent in the setting, enabled Desmond to render a realistic portrait of wildland crews. These strategies, prolonged engagement and persistent observation over time, will be discussed further at the end of this chapter.

Once in the setting, taking notes and knowing what to record emerge as the next biggest issues. Recording technologies can vary from taking mental notes to jotting on small notepads to using digital recorders as small as a pen. Accuracy naturally varies, with mental notes as the least reliable to digital recorders usually the most accurate. Still, one may need to choose one or the other. Sometimes, it just does not feel socially sensitive or ethically responsible to take notes or record people when they are experiencing painful losses from the deaths of loved ones or when talking about sensitive personnel matters. Furthermore, some research participants will be more forthcoming without the presence of note taking “props” that tend to demarcate each other’s roles into awkward dualities. At many disaster sites, prospective participants may associate such props with reporters. If participants assume that a researcher is a reporter, they may distance themselves, offer rehearsed statements, or completely block the researcher from further data gathering. Observation and the meaningful depth needed in qualitative research often result in draining tasks: one must gather useful, reliable data, and one must also build a meaningful relationship with the participant to produce that data. Sensitivity to the context of note taking and to how people appear to feel matters.

Beyond note taking, the research question should always drive note taking but with

theoretical sensitivity and an ear open to new research directions. Lois (2003) wanted to know how search-and-rescue teams operated but, over time, became interested in the emotional aspects of rescue work. The DRC tradition has always been to go to an affected area with a research question in mind but to be open to new avenues of inquiry. By using this approach, the DRC has led QDR into many new areas useful to both social science and emergency management. From its early days, DRC teams would go to a disaster site to gain an overall picture of what was happening. One observational checklist directed researchers to gather data that would yield a chronology of involvement, interorganizational links, and resources used by those organizations. In a study of emergency medical services, observations included the number of triage sites, the number of organizations at each site, any observable coordination across sites, transportation and communication between locations, numbers of those treated, routing to hospitals, and lifespan of the triage site (Quarantelli, 2002).

Documents

Documents abound in a variety of places, including public and private locations, and can encompass accounts or materials that shed light into social aspects of people and organizations (Hill, 1993; Killian, 1956; Plummer, 1983; Webb et al., 1999). Public locations that draw researchers include libraries, official archives, and museums. These public sites may include public materials such as reports but also private records including letters, journals, and other individual items. Searching for just the right document can be an interesting journey, with the researcher never knowing where it might turn up or if it will appear at all. My own recent research concerns the role of FBOs operating in a disaster context. In reviewing documents found in relevant archives, I repeatedly found mention of a set of promotional materials potentially useful in explaining FBO origins. I spent nearly a year searching for a relevant slide set and film made in the mid to late 1950s by Mennonite Disaster Service (MDS), possibly the first ever such visual materials made by any FBO operating in a disaster context.

I began my search at the Mennonite Central Committee (MCC) headquarters in Akron, Pennsylvania. The MCC helped to promote MDS in the mid to late 1950s, spreading its mission across the United States and Canada and firmly establishing the organization as a strong embodiment of Anabaptist faith principles. The slides and

film were nowhere to be found in Pennsylvania, including at the fairly new MDS headquarters building a few blocks away. My journey then took me to the Mennonite Library and Archives in Newton, Kansas. This archive contained several boxes of recently donated MDS materials that had yet to be organized or archived systematically. I left, assuming that the slides and film had been lost over the years. This seemed to be a sad turn of events because Kansas represented the 1950 birthplace of MDS and the location where organizers made their film, in El Dorado. Nearly a year later, I made my way to Goshen, Indiana, home of the Mennonite Church USA archives. After several days of searching through extensive and beautifully archived documents, a script for the slides surfaced. A professional archivist joined in the search, finding the slides and securing a special device to view them. Delighted, I went to lunch to celebrate and returned to the archive—where a film canister labeled “El Dorado” awaited. The archivist set up an impromptu viewing area, and we projected the film onto a cardboard box. Images came vividly to life as Mennonites moved amid the debris, sorting through the remnants of people’s lives and restoring order and hope to a stricken community.

More excited than ever, I returned a year later to dig into the remaining files. Letters, minutes, and reports produced visible and strong ties between the use of the film and slide set to spread the message of MDS across the United States and Canada. The materials proved pivotal in explaining the purpose of the organization that emerged in 1950 and the strategies used for launching one of the first and oldest disaster organizations operating out of a religious context (Phillips 2014). Scanlon (2002) had a similar experience when searching for materials on Prince’s Halifax study. Imagine the challenge: the explosion occurred in 1917, and nearly anyone he would want to interview had died. He hunted down materials related to Prince’s biography and his study of Halifax by doggedly pursuing every lead imaginable. To do so, he not only visited archives, libraries, and museums both directly and tangentially related to the event but also newspaper archives (called *morgues*; for a disaster example, see also Musson, 1986). He searched in Canada and in other nations. He found information in the National Archives in Washington DC on the first American ship that helped. In England, he found documents from a British officer who conducted damage assessment. Scanlon also told everyone about his research, from the hotel maid to his colleagues and neighbors, the

friends and family of Prince, and even golfing partners, and found himself surprised at where a suggestion might lead. He told other journalists about his research, which led “the police officer who found his mother’s scribbled account of what happened” to contact him (Scanlon, 2002, p. 293).

Sometimes the journey to find documents is not as cumbersome or as expensive. Several locations exist dedicated specifically to events of interest to researchers. For example, the Oklahoma City National Memorial and Museum maintains extensive records, documents, items, and memorabilia associated with the bombing that occurred there in 1995. Researchers can examine written records from various responding organizations, listen to 9-1-1 tapes, sort through boxes of items left at the memorial, investigate information related to the criminal trials, analyze materials from the competition to design the museum, and more. Wagner-Pacifici and Schwartz (1991) analyzed mementoes left at the Vietnam Veterans Wall in Washington DC from a comparable repository, unearthing categories of meaning created by family and friends to honor loved ones. The official archives require researchers to register for permission to work in the Reading Room, where archivists oversee use of the collection.

Perhaps one of the most extensive collections worldwide on a single event, the Oklahoma City Memorial Archives

“collects and preserves images, audio recordings, video recordings, documents, books, newspapers, articles, textiles, art and artifacts. Among these invaluable items are over 30,000 artifacts in the permanent collection, over 300,000 photographs, 2000 video recordings and 68,000 items in the Fence Collection. A recording studio was also constructed to capture oral histories, aid in updating exhibit videos and as a place to interview dignitaries and others. The collection already includes more than 500 oral histories from survivors, rescue workers and family members of the victims.” [for more, visit <http://www.oklahomacitynationalmemorial.org/>]

Smaller sets of materials can be unearthed in fire museums, historic sites, government agencies, libraries, and other locations. Documents abound outside of official archives as well, although it may take some creativity to find them and certainly a strong ethical stance to determine appropriate use.

For example, emergency management agencies generate situation and after-action reports, record data, and write plans. Such documents represent extremely important sets of materials. An emergency

operations plan, for example, not only shows procedures but interorganizational relationships. By examining plans over time, it is possible to discern how those relationships change, how policies influence planning, and how decisions are supposed to be made. Emergency operations center logs (increasingly available through electronic recording such as Virtual EOC) represent a potentially massive amount of information. In a consulting project that I did on post-hurricane sheltering along the U.S. Gulf Coast, one agency provided their electronic data on shelters during a massive evacuation. By digging into these virtual documents, it was possible to see how, where, and when shelters opened, peaked in terms of population numbers, and closed. The pattern of leaving and returning to the coast became easily visible, with consequent recommendations for future evacuations and shelter efforts. I also collected and examined emergency operations plans for a wide set of agencies. What became quite clear was that a “boilerplate” had been used across a wide region, even excerpting the same language from one jurisdiction to another—despite differences in demography, geographic location, and decision-making procedures.

Although documents clearly represent valuable information, researchers must also be careful. A starting point is typically to identify the context of the document (Killian, 1956; Webb et al., 1999). Who created the document, when, and for what purpose? Were they able to capture the data objectively or without distraction? Shelter data, for example, tend to be ballpark estimates. Many shelters take a count at midnight when sleeping evacuees are less likely to move about. However, shelter size and staffing can influence the accuracy of the count as well. Mega-shelters, like those seen after Hurricane Katrina (or relief camps, such as those established after the 2010 Haiti earthquake) remain difficult to count with precision. Shelter staff members may also need to focus on other, more immediate matters than securing an accurate count, such as psychological services, first aid, feeding people, amusing children, or other critical tasks.

Other steps help the researcher to establish the viability and usefulness of the document. For example, is it an original document, or did someone write it down later? Is it a first- or second-hand account of an event? Was it altered in any way? Was the author biased? Similar concerns exist when considering whether *selective deposit* may have occurred (Killian, 1956; Plummer, 1983; Webb et al., 1981). Did everything related to the event get deposited in the file or archive? Did something get left out?

Determining the presence or absence of key documents can be challenging, although ordering materials chronologically may help. In addition, following the thread of a conversation may help. In short, researchers looking at documents may need to contend with missing materials, either lost through the passage of time or through intent.

In general, though, documents represent a type of unobtrusive measure that presumably does not cause any significant impact on the person who created it, especially if it is in an official archive. Generally, we believe that documents represent minimal reactions for human subjects. Documents can also increase the subjective understanding of an event, organization, or individual’s experience. Given the caveats presented earlier, disaster researchers should rely on an established procedure to deal with these concerns: *triangulation*. With documents, searching for additional materials that may shed light, support, or possibly confound the researcher’s insights is simply part of the process.

Visual Research

Present-day technologies offer incredible opportunities to capture or download a range of visual images from a variety of sources (Hockings, 1995). In the past, researchers often had to offer a camera or videocam to a research participant to capture data (Blinn 1987; Albrecht, 1985; Becker, 1975; 1978; 1981; Curry & Clarke, 1977; Wagner, 1979; Worth & Adair, 1972). Today, the widespread availability of cell phones means that participants already have the tools they need. Visual research historically has included photographs, film, and videos from individual, organizational, and archival sources. The DRC began to collect photographs early on. Researchers did so with a particular intent: “we are not interested in photos of physical damage or destruction...[but in]...photos that either illustrate some social or, even better, sociological aspects of the disaster situation...convergence on disaster sites...search-and-rescue teams...officials working in a very crowded EOC” (Quarantelli, 2002, p. 114). Currently, it is possible to view how people and organizations choose to represent themselves on web sites, in social media, and on other venues like YouTube (e.g., see www.youtube.com/FEMA). Videos that “go viral” suggest that we are a visually oriented culture, and today’s disaster researcher may actually feel overwhelmed by the amount of potential data.

Visual research methods have been regaining interest in the social sciences, although

disaster researchers have not tapped into this exciting and available data source. Indeed, just examining the photos posted by a Humane Society on their Facebook page following a disaster can reveal the process of animal rescue, owner reunions, and adoptions. Four main techniques allow researchers to gather data that reveal social structure and social relationships. The first technique is native photography, in which the researcher asks participants to record visual images (Collier & Collier, 1986; Worth & Adair, 1972). Researchers usually direct participants to record images on a particular theme, such as a typical day or the process of moving back into a rebuilt home. Pike et al. (2006) did this in shelters established after Hurricane Katrina. They found that survivors revealed both the pain they had endured from the event and the meaningful relationships they established in the shelter.

Pike et al. (2006) supplemented the native photography with a second technique called *photo-elicitation* (Blinn & Harrist, 1991). In this technique, participants view photographs as researchers conduct interviews. The interviewee and interviewer work together to discern the meaning of the images. In the shelter study, interviewers asked participants to give titles to the photographs they took. Findings produced insights useful to shelter managers (e.g., rooftop survivors needed to “hoard” food). Theoretically, the data also generated information on the creation of surrogate families in the shelters, particularly among people distanced from their kin. Photo-elicitation has been demonstrated to produce data considered more precise and “even encyclopedic” than just an interview alone (Harper, 2002, p. 14).

A third technique is called *photo-documentation*, and it guides the researcher to gather images widely and generously on a given research topic. Shooting photos or videos of debris removal over time, for example, would likely discern a process by which this happens. Furthermore, one could photo-document the process from the perspective of a homeowner, a contractor, or the government agency that oversees debris operations. The downside of photo-documentation is the sheer amount of images that must be organized, coded, and analyzed. However, clear benefits can result from the depth and breadth of the data.

The last technique, called *phototherapeutic intervention* (Blinn & Harrist, 1991), might be of interest as well. In this technique, researchers and/or counselors use images in therapy settings. For those who remember the traumatizing images associated with

September 11 or the Oklahoma City bombing, the technique might make sense. One could also extend the technique beyond a therapeutic environment to conduct inquiry into the functioning (or dysfunctioning) of an organization operating in a disaster context. Such inquiries usually occur as debriefings held in emergency management agencies but could also allow a consultant or researcher to diagnose organizational behavior and recommend needed alterations.

Data Analysis

At the start of a project, the bulk of any research effort focuses primarily on data gathering. By the end of the project, data analysis consumes the bulk of one’s activity, although data gathering continues, usually for confirmation purposes (Lofland & Lofland, 1995; Lofland, Snow, Anderson, & Lofland, 2006). This section provides a general overview of influential aspects of data analysis approaches, coupled with specific techniques to reduce the data into usable materials (a number of good volumes can guide data analysis; e.g., see Miles & Huberman, 1994). Although much of the excitement of QDR occurs in the field, the value of such an effort emerges through rigorous analysis of the data (Phillips, 1997).

As a symbolic interactionist and qualitative researcher, sociologist Herbert Blumer came from the 1920s (University of) Chicago School tradition that sent students into the field to gather and analyze data close-up (Hesse-Biber & Leavy, 2011; Fine 1995; Deegan, 1990). Faculty members believed that robust explanations of human behavior had to come from a close-up and connected exposure to people—and the “data” they would produce. From the 1920s to the 1960s, though, qualitative methodology and the field tradition peaked and waned until a new generation of social scientists embraced the methodology again. E. L. Quarantelli graduated from the University of Chicago where he studied with Blumer. The Chicago School influence would ultimately permeate the DRC and its students.

The DRC, established in 1962, began disaster studies at the same time that social scientists renewed their enthusiasm about qualitative methods. Their excitement stemmed in part from the publication of key works by Glaser and Strauss (1965; 1967). Dubbed “grounded theory,” the Glaser and Strauss technique (at the time, see debates from Corbin & Strauss 1990; Glaser, 1992; Strauss and Corbin 1990) emphasized a data reduction process that resulted in systematically comparing bits of data

(such as a sentence or paragraph of an interview) in order to assign a code to the data. Coded data then served as a basis for memo-writing to explain the data further. Additional techniques moved the researcher into an increasingly focused interrogation of the data to glean insights. Under the guidance of Quarantelli (and co-directors), the DRC latched onto grounded theory, teaching it to those students who took classes from associated faculty and to those who served as graduate research assistants. Others who came to disaster studies also learned the grounded theory technique (among others). To this day, the basic principles of grounded theory appear to underlie much of the data analysis conducted, implicitly to explicitly, in QDR.

Grounded theory shares some techniques in common with other data analysis strategies. First, the researcher must reduce significant amounts of data into manageable chunks. A second commonality is to determine a coding scheme to consistently reduce the data. Grounded theorists look for codes that emanate from the data. These can be analytical terms or “native” terms used by people from the setting (Spradley, 1979) or found within the visual data (Ball & Smith, 1992) but should be well grounded in the data. Most analysts generate a long list of potential codes from reading through their data, then winnow the list as they determine how well the data fit with the codes. Developing a list of codes along with definitive parameters helps in this process. A third similarity is that data analysis always begins at the inception of the research project and continues until the end of the effort.

Sjoberg et al. (2006) used grounded theory to examine leadership during rescue operations. From data collected on multiple events, they transcribed interviews and initiated “open coding” by looking at bits of data, then assigning a brief code. As one example, the interview statement “I became mentally blocked, I didn’t see any solutions” fell into the code of “stress reactions” (Sjoberg et al., 2006 p. 579). Continuing, the researchers then sorted codes into categories, with stress reactions falling into the category of “during the operation.” Their efforts led to a tentative model of leadership operations conducted by these authors associated with the Swedish National Defence College. Various disciplines offer other strategies to reduce data.

Anthropologist James P. Spradley (1979) created a twelve-step process to move the researcher along from general observations to increasingly focused inquiry. His technique starts with domain analysis, in which the researcher identifies a cultural domain

relevant to the setting. The next step is to discern one of nine possible semantic relationships that link the cover term of the cultural domain to the included terms, as seen in this table:

Cover Term	Shelter	Emergency Operations Center
<i>(Semantic Relationship)</i>	<i>(strict inclusion) Kinds, types....</i>	<i>(location for action) Is a place for doing....</i>
Included Term	General Population	Interorganizational coordination
Included Term	Functional Needs	Resource distribution management
Included Term	Pet	Decision making

A number of QDR publications have been produced simply from domain analysis, although additional steps can add even more depth and comparison. Examples of domain analysis include an effort from Hurricane Andrew that struck southern Florida in 1992 (Phillips, Garza, & Neal, 1994). Researchers found six barriers (included term) to service delivery (cover term), such as language, communications, and lack of familiarity with the terrain.

Researchers can use preexisting coding schemes, although such an effort may artificially impose codes upon the data. In addition, the effort may force data into marginally relevant codes or even miss revelatory insights from data that fall outside the codes. Researchers understand that sometimes preexisting codes are necessary to satisfy a funder’s requirements. A federal agency, for example, may want to know how people who are deaf respond to warning messages. Researchers will need to code specifically for the answers. Most scholars, though, also know that qualitative data usually produce more data than actually needed. The DRC knew this and always retained additional data for later use. Coding schemes can be found in a variety of disciplines, and many of them can be applied to disaster data.

For example, educational researchers Bogdan and Biklen (2006) identify several kinds of codes that can be used for QDR. These codes include perspectives held by subjects and codes about activities, strategies, events, and methods. In the aftermath of disaster, people work at how they should perceive and respond to events. The outcry that

accompanied failures to respond adequately after Hurricane Katrina ranged from perspectives demonstrating public outrage to political assessments described as a “failure of initiative” (US Congress, 2006; White House, 2006). Analysts can also focus in on the activities that occur in a given location, such as a relief camp, a search-and-rescue team (Aguirre et al., 1995), recovery assistance (Bell, 2008), case management (Stough & Sharp, 2008), or social vulnerability planning (Metz, Hewett, Tanzman, & Muzzarelli, 2002). Coding for strategies also proves insightful for QDR. In the aftermath of Katrina, for example, it became clear that neither public nor privately opened shelters had adequately anticipated the needs of culturally, linguistically, and physically diverse communities (Morrow, 2009; Santos-Hernandez and Morrow 2013; National Council on Disability, 2009; National Organization on Disability, 2005). Often operating in an emergent fashion, creative shelter managers found ways to bring in native speakers, connect with schools for the deaf, and assist children separated from their families (Peek, 2008; Peek & Richardson, 2010). Events also prove worthwhile to analyze from large-scale (massive evacuations and airlifts) to small-scale personal interactions, such as using prayer as a coping mechanism (Lawson & Thomas, 2007; Spence, Lachlan, & Burke, 2007).

One tradition from the DRC has centered on creating a case study using categories to summarize the data and tell the story. Known as the “C” model, the number of *C* elements has varied over time but typically include a *career/chronology* of the event, the *conditions* that influenced what happened, the *characteristics* of organizations and those involved, and the *consequences* that followed. Similar to other data reduction strategies, the C model is simply an attempt to organize massive amounts of information into manageable chunks (an extensive list of reports and publications that incorporate the C model to varying degrees can be found at www.udel.edu/DRC; see also Quarantelli, 1987*b*).

How coders conduct their work varies, as does the techniques that they use. Some prefer traditional methods of laying out transcripts and then reading them over and over to “hear” the data speak to them. As they review transcripts, they write codes in the margins, then systematically return to review and compare the coded sections for similarities and differences. Similarly coded chunks of data (words, sentences, even paragraphs) then get reordered to start the formation of an explanation. This kind of effort makes sense. Because researchers

want to develop rapport, they usually guide the participant to tell the story. The very nature of interviews (especially in a chaotic response environment) means that people may wander from topic to topic and then return to the real focus. Identifying the most relevant portions of the interview or observation requires dedicated concentration to glean—or code—the data into a robust explanation.

Software programs can help with managing massive amounts of data although few disaster studies report using them. Procedures follow the traditions established by grounded theorists and others who direct researchers to code data systematically to produce an explanation. To visualize the process, imagine importing an interview transcript into a software program. After adding correct labels so that the transcript links back to the data throughout the analytical process, you begin to read the transcription. As you read through the transcript, phrases that resonate with the research question stand out. The software enables you to highlight those phrases, assign a relevant code into a pop-up box, and then move on. The software lets you move through the transcription fairly quickly as you read, code, and move on. At various points, it is advisable to stop and examine those data—to engage in the constant comparative method (Glaser & Strauss, 1967)—and ascertain that you have consistently applied codes to similar phrases.

I used the software program NVivo in my research on the Mennonite Disaster Service, which rebuilds homes for low-income families through volunteer labor. As an exercise, consider how you might code these excerpts from interviews:

The quality is “good, only minor repairs, the kind you would have with any construction, probably less. The quality is better than what could have been done by any professional. They should survive another hurricane. We have nothing to complain about.”

“Those people were built beautiful houses. I was real worried. I had seen houses that other organizations built and they did not meet code. So I went every day to check and did daily inspections. The MDS houses met or exceeded all ICC, city, and state codes. I had a lot of problems with contractors taking shortcuts and complaining when I did not approve them. I never, ever found any code violations with the Mennonites.”

I coded these comments as “reputation,” which ultimately built into an explanation of how the organization built effective relationships with local

communities. (Phillips 2014) As the research continued, comments from clients included these statements—what code would you assign?

“It was like they were family. They invited me out to eat dinner, participate in the singing, the food was good. We cooked for the Mennonites here and had them over. We had a really good relationship with all of them.”

“The Mennonites are such personal people, they give all they can give. It was like one big family. They have big hearts.”

“They were great workers, very friendly. They became like family to us in just a week. We love them to death!”

The familial connection felt by clients comes through clearly in these comments. Ultimately, the research has built into a more robust description of how a disaster organization fostered a therapeutic community effect beyond physical construction (Barton 1970; Phillips 2014). The research benefited from additional data gathering. For example, in-home observations revealed photographs of volunteers in the homes of clients, even years after the home repairs or reconstructions had been completed. Clients also spoke of continuing communications with volunteers who worked on their homes and brought out photographs, letters, e-mails, and other documents. Confirmation, through multiple sources, represents an important base on which the quality, credibility, and trustworthiness of QDR will rest.

Quality of Qualitative Disaster Research

Qualitative disaster research relies on the same techniques used by other specializations to ensure trustworthiness and credibility. This section presents those techniques briefly and with necessarily select examples from researchers who have employed the technique. Lincoln and Guba (1985) direct qualitative researchers to these approaches:

- *Triangulation.* Triangulation means to collect data using multiple methods and look at the research question from multiple data points. Imagine, for example, that you want to study emergency operations plans (EOP). Your data might include obtaining a document (the EOP) and reading through it to see how the agency expects to conduct disaster operations. A disaster then occurs and your team moves in to conduct observations, followed by interviews at a later

date. You secure a chronology of events from your observations which you then compare to the EOP. Finding some variations from the expected protocol, you design an interview guide to look at why this happened and how the agency (and related organizations) managed the variation. If you had looked only at the EOP, you would have only a partial picture of the link between planning and operations. By triangulating your study, you now have a richer, deeper, and far more realistic understanding of what transpired. Bell's (2008) study of displaced Katrina evacuees used observation, interviews, focus groups, and documents. She described and explained not only the displacement experience, but the process of meeting the needs of people unfamiliar with local context and culture—and of case managers who needed to learn about their clients' lost networks and backgrounds as well.

- *Prolonged engagement.* Staying in the field increases the potential to secure trustworthy data. Because you are there over time, you are more likely to become known and accepted in the setting and to be able to see essential “backstage behavior.” Desmond (2009) did this in his study of wildland firefighters by staying in the setting for multiple seasons. His ability to do so enabled him to capture an “emic,” native, or insider perspective. Consistent with recommended protocol, Desmond also used a triangulated strategy incorporating all four of the qualitative methods mentioned in this chapter.

- *Focused and persistent observation.* Spradley (1979) recommends that researchers increasingly focus their attention on a topic. Lois (2003) did this when she began her study of search-and-rescue teams, ultimately focusing on the emotional aspect of their work. Spradley directs researchers to focus through his Developmental Research Sequence involving a twelve-step process. Briefly, the researcher uses various techniques (domain, taxonomic, and componential analyses in particular) to hone in and narrow the study. Efforts by Kendra and Wachtendorf (2006) demonstrate the necessity of persistent observation. By being and staying in place, they captured a previously undocumented and heroic effort to rebuild emergency management capabilities after the attacks of September 11, 2001.

- *Research teams.* Most disaster research centers and, increasingly, many funded research projects, rely on a team approach. Teams can deploy across an affected area to conduct multiple methods and

investigate various aspects of the research question. The DRC has used this technique successfully since 1962 (Quarantelli, 2002), often sending out gender-diversified teams (for an extensive list of DRC publications, visit www.udel.edu/DRC). Because of the inherently multidisciplinary nature of disaster response, teams increasingly incorporate researchers with expertise relevant to the study. Henry W. Fischer, who founded the Center for Disaster Research and Education at Millersville, led efforts to study mass fatalities after the 2004 Indian Ocean tsunami. He put together teams that went to India, Sri Lanka, Thailand, and Indonesia, thus enabling research efforts across a significant geographic area and on a topic rarely investigated by social scientists.

- *Peer debriefers.* The peer debriefing technique often takes place in the field, when teams meet regularly to discuss what they think they are finding. This technique is especially useful when team members uncover conflicting information, and it enables them to investigate further. In-the-field debriefing also enhances the possibility that researchers will focus in as Spradley (1979) recommends. For nonresponse or nonfield times, one's colleagues may serve as peer debriefers. Simply sitting and talking about one's research often reveals holes, lack of understanding, and areas in which the study needs to develop further.

- *Member checks.* A member check happens when the researcher sends reports or drafts of papers back to the interviewees. This participatory element of qualitative research can be especially valuable in disaster studies, especially for novices unfamiliar with the acronym soup that characterizes the field. For qualitative research, terminology is particularly important because actors can reveal varying definitions, uses, and understanding of the term. Researchers can enhance the credibility of their work by asking for feedback from interviewees to ensure they captured the data accurately and analyzed it in a manner that makes sense to the affected community. Researchers who study disabilities and disasters understand this. Terms used in the past include special needs, at-risk populations, vulnerable populations, and similar terms that have included (at times) people with disabilities. Such terms connote a dependency rather than a capacity in disaster contexts. Hurricane Katrina, in particular, prompted movement away from such terms and from a medical approach (creating shelters for medical needs and sometimes disabilities)

to a more functional approach. Today, general population shelters must meet the functional needs of people with disabilities (e.g., communication) rather than segregate them into separate facilities.

- *Referential adequacy.* This technique takes place during data analysis, which begins from the start of data collection. The researcher identifies several cases (documents, interviews, observation notes, images) and sets them aside until the end of the study. When the researcher believes that an explanation has emerged, it is time to use the set-asides as a reference check to assess the adequacy of the explanation. Consistency with the findings strengthens confidence in the overall explanation. Deviations suggest that additional data collection or a reinvestigation of the original codes and categories must take place.

- *Negative case analysis.* This technique also aids the researcher in ensuring that the analysis is adequate. Negative case analysis occurs when the researcher actively looks for evidence that the explanation does not fit with the data. As it is unlikely that all the data will fit consistently within a derived explanation, negative examples are likely to emerge. The researcher must honestly describe those variations, discuss how extensively they appear, and consider their impact on the explanation. However, most social science research (and QDR) creates an explanation that supports most of the data.

- *Audit Trails.* It seems that many researchers use the idea of an audit trail informally. This technique is akin to a set of materials that accountants use to demonstrate the flow of money into and out of accounts. Instead of money, data appear in the audit books and the researcher demonstrates (through a carefully crafted trail) how he or she moved from raw data through initial memoranda to fully developed explanations or theories. Software programs aid in this considerably, as the researcher can code interview data (for example) and have the software move it into separate files (imagine a page of codes on "volunteer management strategies") that link back to the original interview(s). The researcher can then compare all the coded data from all the interviews on the single code of volunteer management strategies to ensure consistency in coding. That consistency convinces the auditor (perhaps a dissertation supervisor) that the researcher completed the coding and analysis appropriately and consistently and generated a robust explanation

worthy of a PhD (or of publication in a respected journal).

Writing Up Qualitative Disaster Research

Qualitative disaster researchers write in several kinds of journals, including discipline-specific (e.g., *American Sociological Review*), multidisciplinary (e.g., *Journal of Disability Policy Studies*), or disaster-specific. Most researchers publish in the last type. In 1978, an initial effort to launch a journal called *Mass Emergencies* began. After several years of publishing, efforts among disaster researchers turned toward establishing an interdisciplinary social science journal titled the *International Journal of Mass Emergencies and Disasters (IJMED)*. Academics who launched the journal in 1982 did so under the auspices of the International Research Committee on Disasters, which is Research Committee No. 39 of the International Sociological Association. The journal continues today (www.ijmed.org), publishing three times a year and it:

addresses issues of theory, research, planning, and policy. The central purpose is publication of results of scientific research, theoretical and policy studies, and scholarly accounts of such events as floods and earthquakes, explosions and massive fires, disorderly crowds and riots, energy cut-offs and power blackouts, toxic chemical poisonings and nuclear radiation exposures, and similar types of crisis-generating situations. Its audience includes specialists within various areas of research and teaching plus people working in the field who are responsible for mitigative, preparedness, response, or recovery actions. (www.ijmed.org, accessed July 16, 2012)

The *IJMED* has been exceptionally friendly to and inviting of QDR. Over its thirty-year history, the *IJMED* has published works that have contributed substantially to the body of knowledge for disaster studies and emergency management practice.

Other exceptional journals have joined the *IJMED*, publishing QDR in a range from exclusively academic to practitioner-friendly. The majority of these journals host editors and reviewers open to rigorous and accountable QDR, although some prefer quantitative studies. As space limits listing all, some of the journals with a history of accepting qualitative work include *Natural Hazards Review*, *Environmental Hazards*, *Disaster Prevention and Management*, *Natural Hazards*, and the *Journal*

of Emergency Management. Qualitative disaster research-type journal submissions do not typically vary in format from other manuscripts. An exception may occur in the concluding section. The inherently use-inspired nature of the findings suggests that researchers should address the significance of the findings and remaining questions not only for research, but also for policy and practice.

Conclusion

Thomas E. Drabek (an alumni of the DRC at Ohio State University) once reflected on his lifelong journey as a disaster researcher: “new theory must be created and old notions tested and revised. But insights for practitioners also must be produced as we join other disciplines in accelerating the professionalization of emergency management. In this way we fulfill the real promise of disaster research—to prevent or ameliorate human suffering” (Drabek, 2002, p. 153). Qualitative disaster research represents an important use-inspired hybrid demanding rigor and precision. Studies created by disaster scholars do not simply sit on a shelf collecting dust; they can and are used in degree programs for emergency managers, in pushing forward social science theory, and as a means to improve policy. Qualitative disaster research makes a difference, and the quality in which researchers accomplish their work matters.

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Conducting Mixed Methods Research: Using Dialectical Pluralism and Social Psychological Strategies

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Abstract

The philosophy/theory of dialectical pluralism (DP) is first summarized. Ontologically, DP views reality as plural and changing. Epistemologically, DP follows a dialectical, dialogical, hermeneutical approach which includes listening, interacting, and learning from “the other.” Theoretically, DP is inspired by and integrates multiple theoretical concepts especially from the works of John Rawls, John Dewey, and Jürgen Habermas (Johnson, 2012, 2011a). From empirical research, the authors of this chapter draw on concepts and findings from social psychological literatures such as conflict management, negotiation, small group psychology, group counseling, group dynamics, political diplomacy, deliberative democracy, and workplace justice. DP recommends purposeful construction of teams that include multiple/different values and perspectives and stakeholders from the most disadvantaged affected groups. The group process operates from the position of equal power, the use of social psychological strategies (discussed in depth in this chapter), and working toward win-win solutions.

Key Words: Dialectical, pluralism, procedural justice, heterogeneous groups, group dynamics, conflict negotiation, win-win solution, paradigm, metaparadigm, process philosophy

In many ways, the paradigm debates (e.g., qualitative vs. quantitative research, constructivism vs. positivism) are about knowledge, and, since ancient times, we have been debating the issue of knowledge: What is it? Who has it? How does one get it? Is it ephemeral or eternal? In the West, one can “see” proto-qualitative, proto-quantitative, and proto-mixed thinking at least as far back as ancient times, and those arguments continue to present times (Johnson & Gray, 2010). In ancient times, *ontological relativism* (proto-qualitative) is seen in the sophists, such as Protagoras’s claim that “man [sic] is the measure of all things.” *Ontological realism* (proto-quantitative) is seen in Platonic philosophy of the forms and the search for absolute, eternal truth and in Euclid’s geometry.

A few examples of “mixtures” or proto-mixed research ideas are seen in Aristotle, such as his

theories of ethics/psychology, multiple causation, his use of multiple logics (e.g., deduction, induction, abduction, dialectics), and his principle of balance (of excess and deficiency). It is also seen in Heraclitus’s statements that “you cannot step twice into the same river; for other waters are ever flowing on to you” and “opposition brings concord. Out of discord comes the fairest harmony.” It is also seen in Abelard’s merging of realism and nominalism into “conceptualism” and in his claim of “sic et non” (i.e., “yes and no”). The mixed way of thinking also is seen in Montesquieu’s “checks and balances,” Vico’s listening to ancients and moderns, Comte’s statics and dynamics, Dilthey’s and Weber’s natural and human sciences, and various versions of dialectical and dialogical philosophy seen in history; putting an “and” between many or most dichotomy

pairs (that we all argue about) helps produce the “mixed viewpoint” (Johnson, 2008, 2011b). The mixed viewpoint/attitude is partially summarized like this: both or some of both are often better than one (e.g., poles, perspectives, approaches, methods, paradigms). “Mixed thinking” shows an engagement with difference, a rejection of monisms or singular/reductionist solutions, and an explicit attempt to incorporate ideas and goods that are important but in dynamic tension. This idea, today, is at the core of *mixed methods research* (hereafter called *mixed research* because mixed methods research is about more than just methods).

In this chapter, we provide a process philosophy and provide social psychological strategies to enable the conduct of the strongest form of mixed research, known as “equal-status” or “fully interactive” mixed methods research. This kind of research occurs when multiple stakeholders or perspectives interact and learn from one another, producing a dynamic perspective that is instantiated in each research project. We first explain the process philosophy, called *dialectical pluralism 2.0* (or more simply, DP; Johnson, 2011a). Dialectical pluralism provides a process for engaging with difference. Second, we provide multiple social psychological principles and strategies that enable effective collaboration across paradigms and learning from difference. Dialectical pluralism allows us to engage multiple sets of social psychological principles interactively. Third, we provide implementation and dissemination frameworks for conducting mixed research using DP. Fourth, we make mixed methods research a little more complex by reminding the reader that it is not just one thing; it includes at a minimum three major kinds of research (qualitatively driven, quantitatively driven, and equal-status or fully interactive). We conclude with a brief vision for mixed research driven by the philosophy of DP that we hope will be attractive to all researchers.

Dialectical Pluralism

Dialectical pluralism was originally explained in Johnson (2011a) and more recently applied in Johnson and Stefurak (2013), and we describe its key features here. First, dialectical pluralism is ontologically pluralist and assumes that there are multiple realities and many possible ways to construe reality. For example, one might construe different parts of reality as subjective, intersubjective, and objective. Likewise, different disciplines and different paradigms provide different sorts of reality

for consideration. Second, the “dialectical” in DP suggests that the epistemology is dialectical (which allows learning from different and even contradictory ideas), dialogical (communication and dialogue is required for many considerations of difference and similarity), and hermeneutical (what we do is ultimately interpretive and will be continually reinterpreted and reconstructed in the future).

Dialectical pluralism is a process philosophy and meta-paradigm for dialoging with multiple perspectives. Dialectical pluralism is needed because there are many important philosophical and methodological paradigms and worldviews that deserve a great deal of respect (Johnson, 2011b). A few of the “big” ones include constructivism, postpositivism, critical theories, realism, pragmatism, participatory, postmodernism, and poststructuralism. Dialectical pluralism starts with several assumptions: (a) one should consider multiple perspectives and sources of evidence which might be conflicting and divergent; (b) no single perspective is perfect or exhaustive in its understanding or importance; (c) each perspective provides a set of goods for consideration, as well as a vision of its most important good, end, or summum bonum; (d) different perspectives drive creativity, change, and provide new, more useful, wholes; and (e) one should engage with multiple perspectives dialectically, dialogically, hermeneutically, and empirically.

Dialectical pluralism is a process philosophy for several reasons. Philosophically, DP recognizes that much reality is plural and dynamic (rather than singular and static). You can see this in your own life as it continually changes, over the long term, as well as day to day and even moment-to-moment. More generally, the notion is that much of reality is complex, dynamic, and interactive. Process philosophy views reality holistically rather than as always propositionally true or false. Process philosophy rejects most if not all dualisms because each pole might be important, especially when they interact and take into consideration contexts and shifting dynamics. Here, one can see that a dialectical-dialogical-hermeneutical approach is process oriented. Another sense of process in DP is seen in a more Rawlsian (2001) meaning, as a dynamic for producing political justification for action based on deliberative democracy and procedural justice. The research purpose is jointly constructed by the “citizens” making up the research team, and the empirical outcome (regardless of what it is) becomes justified because of a deliberative democratic or other agreed upon procedural logic.

Dialectical pluralism attempts to follow and requires attention to Rawls's (2001) first principle of procedural justice (equal basic rights) and his second principle (which includes careful, additional attention to helping those with the least power in society).¹ When working with teams (e.g., collaborative research teams and communities of practice [CoP]), one attempts to make this happen at the team or CoP level.

Dialectical pluralism is helpful for interactive mixed research because it is not set in any single paradigm. Relying on more than one paradigm, DP also is a communication theory in that it requires the respectful listening to two or more (typically multiple) "paradigms" or arguments for "best" design, analysis, and interpretation in a mixed study. Dialectical pluralism asks us to purposefully, dialectically, and dialogically engage with difference, including different paradigms, methods, disciplines, values, stakeholders, and citizen perspectives. This process should help produce socially and scientifically justified knowledge that is "thick" (value laden), provisionally true, useful, and widely accepted. Heloise (1101–1164) expressed the philosophical component well when she said "I preferred the weapons of dialectic to all the other teachings of philosophy, and armed with these I chose the conflicts of disputation instead of the trophies of war."

As a metaparadigm and process for mixed research, the use of DP requires "dialectically listening" to multiple perspectives, explicitly stating and "packing" the approach with stakeholders' and researchers' epistemological and social/political values to guide the mixed research study (including valued-means and valued-ends), and combining important ideas from competing paradigms, methodologies, and competing values into a new socially agreed upon whole for each research study. The exact instantiation of DP use will vary from research study to research study depending on the research questions, the situation, research participants, and any other important contingencies that you identify. To use DP, you are required to engage in listening, dialoging with difference, embracing tensions, understanding "the Other," learning from and valuing the Other, and acting (e.g., Buber, 1923/2000).

Following Rawls's theory of justice as fairness (2001) and his "Law of Peoples" (1999), DP emphasizes procedural or process justice through a deliberative democratic process. Early studies on procedural justice revealed that having a "voice"

or being heard during a process increases perceptions of outcome fairness even when outcomes were not favorable. People's feelings of fairness were also affected by their perceptions of control of the process and input into procedures for resolving a dispute or negotiating (Thibaut & Walker, 1975).

Dialectical pluralism advocates a holistic approach to research team/group composition that includes key stakeholders and representatives for all important standpoints. When dialoging with qualitative and quantitative perspectives, the mixed research expert serves both as a content expert and as a facilitator; this requires careful and strategic attention by this person to perform two separate and often conflicting roles. A mixed researcher supports process justice primarily by practicing inclusive team construction and equal power-oriented discourse. Specific strategies include (a) encouraging and reinforcing open-mindedness of members; (b) working toward shared development and understanding of team/group goals for a research study; (c) obtaining agreement on process; (d) making the process transparent and fair; (e) encouraging all members to listen actively and participate, including engaging in constructive cognitive conflict; (f) making sure everyone expresses her or his views and reasons for those views; (g) examining alternatives; and (h) ensuring that the group articulates clear rationales for positions and decisions. Conflict monitoring theory argues that detection of conflict should lead to increased cognitive control and that prior experience with conflict is facilitative (Botvinick et al 2001; Botvinick et al., 2004; Mansouri et al., 2009).

In addition to supporting process justice based on Rawlsian principles, DP can address multiple specific outcome-oriented justices through appropriate team construction and/or value emphasis, including *social justice* (to allocate power and outcomes within and across groups; van den Bos, 2003; Wainryb, Smetana, & Turiel, 2008); *distributive justice* (to allocate valued resources; Folger & Konovsky, 1989); *retributive* or, better, *restorative* and *compensatory justice* (to compensate for violations of rights of humans in the form of, e.g., inequality of opportunity for jobs and education; Bies & Tripp, 2001; Folger, Cropanzano, & Goldman, 2005); and *global justice* (to allocate power within and across nations and indigenous groups across the world; Shapiro & Brillmayer, 1999; Silbey, 1997).

The point is that DP is designed to produce process justice (because the team members must agree to the deliberative democratic process for decision

making), *and* the specific goals and values content of DP can be adjusted to the needs and values of the particular research team (as long as Rawls's first and second principles of justice are respected). When the specific process and holistic core values are negotiated across stakeholders to be representative of all relevant and important values, the process and outcomes become more justified. For example, core values might include transformation (Mertens, 2012), gender equality and feminism (Hesse-Biber, 2012), indigenous values (Chilisa, 2012), and racial equality (Delgado & Stefancic, 2012). But the effort of "packing" the team or larger group or set of decision makers holistically is not necessarily sufficient to ensure that the diverse voices of its members are heard nor to ward off group process failures such as groupthink, social loafing and social inhibition, unequal power, confirmation bias, premature closure, sunk-cost bias, and various excesses and deficiencies (a la Aristotle). One must strategically attempt to prevent these group breakdowns.

Early on, practicing DP might produce multiple kinds of immediate outcomes, including continued confusion, informed conflict, tense compromise, tolerance, compromise, general agreement that maintains key aspects from different positions, a happy balance of differences, or (best) a win-win solution. Stated differently, the path to DP with fidelity might not be easy—we recommend stakeholders invest in collaboration that reveals the values behind the research questions generated, as well as the data collection and analysis protocols used, in order to work toward the ultimate goal—a win-win solution based on deliberative democratic discussion, constructive conflict of positions, interactive process cooperation, and justified outcomes because of agreed-upon process assumptions and procedures.

We have provided an overview of DP as a process philosophy. Next, we show that DP can be used both as an intellectual process within an individual (where one internally dialogues with ideas, values, concepts, and other differences) and as a group process (where one, working in a heterogeneous group, strives to produce win-win results).

Dialectical Pluralism as Intrapersonal Dialogue

As an intellectual process, DP requires that you pay deep attention to multiple disciplines and multiple perspectives of issues. This is internal dialectical thought or intrapersonal dialogue (Johnson, 2011a). An individual using DP should strive for continual "dialectical integration," which is a

dynamic and continual process of reconstruction. The individual needs to interrelate intellectual, conceptual, and practical differences and tradeoffs, including, for example, multiple methods and sources of information and evidence, multiple values, and multiple epistemological issues and stakeholder standpoints. It is sometimes argued in the qualitative literature that one cannot hold two paradigms (i.e., strong incommensurability). We disagree with the strong form of Kuhn's incommensurability. Furthermore, rather than accept the proposition that mixed research needs to accept one and only one paradigm, two or more standpoints can coexist as a dynamic whole. Strategies of movement back and forth, code switching, and Gestalt switching can be learned, although it is difficult and requires much practice. In 1936, F. Scott Fitzgerald recognized this issue when he was quoted as saying "The test of a first-rate intelligence is the ability to hold two opposite ideas in mind at the same time and still retain the ability to function." In short, we recommend that each individual hoping to conduct strong mixed research develop skills in locating, seeing, understanding, and moving back and forth between different perspectives and positions.

The ability to differentiate the distinctions and contradictions found among a set of elements and, at the same time, recognize linkages among these elements shapes *integrative complexity*. Those with higher integrative complexity have a multidimensional view and show more flexibility and openness to inconsistency in opinions and behaviors (Tetlock, Peterson, & Berry, 1993). Integrative complexity can be enhanced through the use of paradoxical frames, which are "mental templates individuals use to embrace seemingly contradictory statements or dimensions of a task or situation. When embracing the paradox, individuals recognize the contradiction...yet understand their potential relationship as complementary or reinforcing" (Miron-Spektor, Gino, & Argote, 2011, p. 229). These contradictions create positive intrapersonal conflict, which motivates the individual to move away from existing assumptions to explore novel relationships among contradictions (Huang & Galinsky, 2011; Miron-Spektor et al., 2011). Research demonstrates that when individuals are primed with paradoxical frames, they experience more conflict, their integrative complexity increases, and they are more creative than are those primed with creativity or efficiency frames (Miron-Spektor et al., 2011). Facilitators in a mixed research setting can provide paradoxical frames to enhance intrapersonal dialogue.

Not all values are easy to articulate. Polanyi (1958) claimed that absolute objectivity is a false ideal because all knowledge claims rely on personal judgments. A knower does not stand apart from the universe but rather participates personally within it. Intellectual skills are driven by passionate commitments, which motivate discovery and validation. Polanyi (1966) distinguished among the phenomenological, instrumental, semantic, and ontological aspects of tacit knowing and advocated several levels of reality. Intentionality is an example of a higher level reality functioning as a downward causal force. Our pursuit of self-set ideals such as truth and justice enriches our awareness of the world. The reductionist attempt to reduce higher level realities into lower level realities generates what Polanyi describes as a *moral inversion*, in which the higher is rejected in favor of the lower. This inversion is pursued with moral passion. Polanyi identified it as a pathology of the modern mind, and traced its origins to a false conception of knowledge, one that, although relatively harmless in the formal sciences, generated nihilism in the humanities.

Intrapersonal dialogue takes tremendous strength of self and is facilitated by practiced reflection. Critical to this process is being able to articulate one's own values regarding research and an ability to frame this reflection within complex systems. Revealing the values that ground your research is a reflexive process, requiring constant questioning of your core assumptions about paradigmatic foundations, procedures, personal motivations, needs and skills, contexts and conditions, logistics, collaboration, and desired outcomes. Marion (1994, pp. 50–51) conceived of three tiers of values: moral values which operate at the base tier as “bedrock,” “unconditionally forming the life philosophy of the individual”; second tier values which are “consciously and deliberately” taken to form our personal code of honor, such as truth telling and respect for others; the third tier and highest level of values reflects the professional's constantly evolving community standards of practice (e.g., including research ethics, social justice). Level 3 is especially important for collaborative values. To achieve the goals of DP one needs to work up to level 3 values through some form of authentic interpersonal dialogue, which leads us to the section that follows.

Dialectical Pluralism as Interperson Dialogue: Getting Beyond Herding Cats Syndrome

There are many challenges to conducting holistic intrapersonal dialogue in the strong sense just

mentioned. However, equal-status or fully interactive mixed research can also be implemented through the use of DP as an interpersonal or group process. This is important because mixed research is often conducted in research teams and larger groups (e.g., publics, CoPs, research literatures). In the context of these groups, although tangible values can be articulated, honoring the often “stray cats” of divergent and tacit values can prove more elusive and tension generating.

In this section, three perspectives of DP interpersonal dialog are examined: constructive conflict, collaborative creativity, and collaborative transformation. Finally, a framework for implementing DP is presented, along with a brief case.

Today, we see a proliferation of global research communities using cooperative frameworks to conduct research with the concerted purpose of increasing intellectual and social capital—in fact, many define themselves via the intangible values they produce and their capacity to lower barriers to collaboration (e.g., Cambia's BIOS, SocialPhysics, and multiorganizational research networks such as opin, Eli Lilly's open platform of global idea sharing). Although enticing, the prospect of global dialog can be daunting if we want to practice what we preach. Verna Allee's (1997) Knowledge Evolution was among the first to launch a worldwide conversation about the tangible and intangible values of intellectual capital. She developed the concept of value network analysis (VNA) to a global audience using a ValueNet Works methodology to study the web of relationships that generate economic or social value through complex dynamic exchanges of both tangible and intangible benefits (2003).

Collaboration takes time—this is not always part of a person's regular work, and extraorganizational sanctioned time is rarely committed. Hence, organizational support is helpful. Although research CoPs are seen as conceptually valuable and anecdotal success stories abound, tangible and intangible values for research CoPs at the individual, group, and formal organizational levels should be identified. In their work on CoPs, Wenger, McDermott, and Snyder (2002) described five stages of CoPs, ranging from potential to coalescence to mature to sustain to transform. If we want DP communities to be able to reach transformative goals, this requires resources and time.

Regardless of how a DP research team or community forms and no matter its goals, it must be nurtured and organized for relationships to emerge. Preconditions for the success of effective

collaborations often include presence of a top-level sponsor, a skilled network manager, logistical supports, and access to “knowledgeable others” who do not need to be regular participants but serve as critical sources of timely knowledge and skills. Anklam (2007) refers to two essential roles: “orchestrator” and “choreographer”; those who stay with the show, interpreting the nuances of a production based on the attributes and skills of the performing musicians and dancers. In addition to the predictable needs of management, goal setting (and resetting), and trust building, these facilitators must be skilled in a variety of more tacit skills, such as cultural diligence and responsiveness to all layers of the organization(s).

It is important that someone in each group act first and foremost as the group process facilitator and ombudsman. This process leader should be trained in DP (and procedural justice) and skillfully perform the roles just listed. Equitable and fair process makes teams successful in the long run and produces a sense of justice. Goals and ends are always changing, but an effective and just process enables the continual reconstruction of newer and better knowledge. DP facilitators often serve in multiple roles, and the fluidity of roles depends on the formality of the CoP’s governance structure. Successful facilitators can mediate essential tensions and resolve questions about

- balancing network and individual values to maintain commitment of all members;
- balancing horizontal and vertical structures to ensure efficient organization and resourcing and to support genuine dialogue;
- mediating entry and exit of members and new ideas with qualitative criteria such as diversity in knowledge domains, values, and operational styles, and commonality and collaborative potential;
- negotiating clarity of purpose: tangible outcome production versus idea exchange versus discovery based on equitable evidence-based needs assessment.

We have discussed that mixed research frequently is conducted in teams. Therefore, here we provide a procedure for team development provided by Harvard negotiation experts Dyer, Dyer, and Dyer (2007). These scholars/practitioners organize their thinking into their “four Cs of team development.” The first point is to analyze and affect the team *context*. An effective team context or environment will emphasize the need for teamwork, value collaboration, reward teamwork, allow failures in route to

successes, and more generally constitute a culture and social structure that supports teamwork.

The second point is to know and affect the team *composition*. The team should not be too large, it should include people with different knowledge/skills, and it should include people who are able and motivated to engage in constructive conflict. In the case of mixed research, you should make sure you get the right combination of people for your research project. An effective mixed research team will typically include experts in qualitative, quantitative, and mixed approaches and methods, as well as experts in pedagogical, content, and organizational issues. DP also asks that you include members who are key stakeholders but are frequently excluded because of their lack of power and/or minority status; providing inclusion is required for a just process.

Third, successful collaborative teams require development of *competencies* and skills that will enable strong performance. Some competencies exist in separate individuals, others will be shared across all members, and some capacity building may be required across the members to build sustainability and perhaps scaling to larger venues. Successful teams set shared and superordinate goals, build trust among members and commitment to goals, and they include indicators of competence. A competent team effectively communicates, manages conflicts, and makes agreed-upon decisions.

The fourth “C” is *change*. A successful team will monitor its performance toward its goals and make changes as needed. Change management strategies are incorporated, team strengths and weaknesses are understood, “bottlenecks” are identified and reduced or eliminated, and, more generally, the team adapts to new conditions and focuses on improvement.

Dialectical Pluralism Interpersonal Perspective 1: Approaching Conflict as a Positive

Rather than avoidance, practicing DP requires skills to deal with conflict. Functional conflict is part of effective team process. Much of this work evolved in the late 1960s and 1970s. The interactionist philosophy of conflict proposed by Robbins (1978) argues that conflict is necessary for high-performance groups and encourages the stimulation of functional “conflict [as] the catalyst for change” (p. 69). He shows that conflict stimulates change, which brings about adaptation. The seeds of conflict that induce change include “opposition to others’ ideas, dissatisfaction with the status quo,

concern about doing things better and the desire to improve inadequacies” (Robbins, 1978, p. 69). Conflict is functional to the extent that it facilitates the parties’ productivity and adaptability (Pondy, 1967). Robbins concludes that “functional levels of conflict are conducive to innovation and higher quality decisions” (p. 69). This is demonstrated in numerous studies (e.g., Beersma & De Dreu, 2005; De Dreu & Nijstad, 2008; Janis, 1972; Lawrence & Lorsch, 1986; Pruitt & Rubin, 1986).

Pondy identified three types of conflict: (a) *bargaining conflict*, which occurs among groups competing for scarce resources; (b) *bureaucratic conflict*, which occurs among levels of the organizational hierarchy; and (c) *systems or lateral conflict*, which occurs among individuals at the same level and is concerned with “problems of coordination” (Pondy, 1967, p. 317). The third type, lateral conflict, is the type most likely to emerge in mixed research groups. However, bargaining conflict and bureaucratic conflict are likely to occur when the outcomes of the mixed research process determine resource distribution or administrative decisions and when the membership consists of different disciplines and traditionally unequal “power” partners.

Approaches to conflict resolution historically have been viewed according to two dimensions: specifically, the desire to satisfy one’s own interests and the desire to satisfy the interests of the other party. The approach demonstrated in a party’s desire to win at the expense of the other party is *competing*, whereas the approach focusing on satisfying the needs of the other party without fulfilling one’s own interests is *accommodating*. An approach between these two that offers incomplete satisfaction to both parties’ interests is *compromising*. When compromise is used, both parties give up some of their interests and keep some of their interests. The ideal solution is found between the preferred outcomes for each party. Compromise is a lose-lose solution because neither side has its interests completely fulfilled. Compromise “represents an alternative which yields some, but incomplete satisfaction to both parties” (Thomas, 1976, p. 898). In DP, the sought-after negotiation outcome is a win-win solution.

Collaboration is an approach to conflict that is especially conducive to mixed research. Collaboration seeks to integrate the interests of all parties and to satisfy fully their interests and needs. When collaborating, the parties use problem-solving to search for a “mutually beneficial agreement,” and they do not engage in tactics to win at the other party’s expense (Thomas, 1976, p. 901). *Integrative*

bargaining, a collaborative negotiation strategy, is a subprocess of bargaining that focuses on joint problem solving by identifying solutions that maximize the joint gains of both parties (Walton & McKersie, 1965). A key to collaborative solutions is open communication and a willingness to share information regarding each party’s interests and needs. Research shows that integrative solutions are more likely to strengthen the parties’ relationships and yield more stable agreements (Lewicki, Weiss, & Lewin, 1992).

Pinkley and Northcraft’s (1994) research shows that individuals cognitively frame conflict situations along three separate dimensions: (1) the cooperative versus competing approach, (2) a focus on relationship versus task, and (3) a focus on intellectual aspects versus emotional aspects of the conflict. Their study revealed that when dyads had large differences on the intellectual and emotional, and the cooperative versus competing dimensions, the frames of parties converged during negotiations, whereas, parties’ frames did not converge when one side focused on relationship and the other side focused on the task. Furthermore, individuals who framed the conflict in terms of relationships and intellectually, rather than emotionally, reported greater satisfaction with negotiation outcomes and reported more positive effects for the ongoing relationship. Individuals who framed the conflict in terms of the task and cooperation reported significantly better monetary outcomes. The researchers concluded that communication and collecting additional information are critical for helping disputants understand the differences in their framing and to enhance their ability to resolve the dispute to the mutual benefit of both parties. These findings are consistent with the philosophy of DP and equal-status mixed research.

Although collecting additional information seems to be an important element for resolving conflict among members of a group and making better decisions, research shows that groups with homogeneous views are less likely to search for disconfirming information regarding the group’s decision and are more likely to engage in groupthink than are heterogeneous groups (Schulz-Hardt, Jochims, & Frey, 2002). Therefore, using heterogeneous group composition to ground equal-status mixed research should lead to the availability of more information and, subsequently, improved decision making.

We should recognize that diversity, which is necessary to DP-driven mixed research, may in some ways inhibit the group’s ability to reach

consensus and resolve conflicts. Group members' willingness to share information and engage in crafting integrative solutions can be inhibited if *faultlines* develop among subgroups in a research team or CoP (Thatcher & Patel, 2012). "Faultlines are hypothetical dividing lines that split a group into two or more subgroups based on the alignment of one or more individual attributes" (Thatcher & Patel, 2012, p. 969). Faultlines may be created by differences in demographic characteristics such as age, gender, or race; information characteristics such as occupation, education, or functional expertise; personality traits or types; geographic location; or theoretical assumptions or core beliefs. Research on the effects of faultlines shows that stronger faultlines increase intragroup conflict and, subsequently, reduce group cohesion (Thatcher, Jehn, & Zanutto, 2003). Faultlines also inhibit information elaboration (Meyer, Shemla, & Schermuly, 2011) and creativity (Pearsall, Ellis, & Evans, 2008).

When your team has conflict, the process facilitator can use conflict management and negotiation strategies. Fisher and Ury (2011; Harvard Negotiation Project) have found a four-part process to work well: (a) separate the people from the problem; (b) focus on the personal interests and reasons behind the positions, and if progress is difficult then to start with small gains/wins and work toward larger gains; (c) focus on generating options that provide mutual gain; and (d) base team choices and decisions on "objective" or socially agreed-upon criteria. If (d) occurs and the process is viewed as just, there should be buy-in to the solutions and products produced by the team. One additional strategy to help minimize the effect of faultlines is called *fractionation* (Dues, 2010), in which you get the disputants to agree on smaller issues or parts of the larger disagreement, and then attempt, again, to move to a larger agreement.

Dialectical Pluralism Interpersonal Perspective 2: Collaborative Creativity

We especially like Keith Sawyer's (2008) concept of *collaborative creativity* in teams and groups rather than the popular myth of creativity coming only from isolated individuals. Sawyer argues that research shows that creativity and most innovations are collaborative in that they occur in an intellectual environment that is collaborative in a broad sense (e.g., we collaborate with those who come before us, books we have read, and discussions we have

had with others, as well as when explicitly working with others). Sawyer uses the metaphor of jazz improv performances, in which each person plays a part and a whole is produced, but the whole is literally created (creatively) anew in each performance. Sawyer builds on his dissertation advisor's (Mihály Csíkszentmihályi's) concept of group flow and summarizes this collaborative creative process as follows:

Group flow happens when many tensions are in perfect balance: the tension between convention and novelty; between structure and improvisation; between the critical, analytic mind and the freewheeling outside-the-box mind; between listening to the rest of the group and speaking out in individual voices. The paradox of improvisation is that it can happen only when there are rules and the players share tacit understandings, but with too many rules or too much cohesion, the potential for innovation is lost. The key question facing groups that have to innovate is finding just the right amount of structure to support improvisation, but not so much structure that it smothers creativity. Jazz and improv theater have important messages for all groups because they're unique in how successfully they balance all of these tensions. These types of ensemble art forms embrace the tensions that drive group genius. (p. 56)

The "space" in which collaboration occurs is critical. We see benefits to Soja's *third space* as a place to leverage collaborative creativity yet remain sensitive to social justice values. Soja (1996, p. 57) developed a theory of third space in which "everything comes together...subjectivity and objectivity, the abstract and the concrete, the real and the imagined, the knowable and the unimaginable, the repetitive and the differential, structure and agency, mind and body, consciousness and the unconscious, the disciplined and the transdisciplinary, everyday life and unending history." As he explains, "I define Third space as an-Other way of understanding and acting to change the spatiality of human life, a distinct mode of critical spatial awareness that is appropriate to the new scope and significance being brought about in the rebalanced trialectics of spatiality-historicity-sociality."

Soja constructs third space from Lefebvre's (1974/1991) spatial trialectics and Foucault's (1986) concept of heterotopia. Where utopias are characterized by unusual, consoling, coherent spaces that do not exist, Foucault's heterotopic spaces are real, disquieting spaces that deliberately play with spatial ambiguities by intersecting real and imaginary spaces

for our purposes. Lefebvre suggested distinguishing among isotopias, utopias, and heterotopias to explain the emergence of the “differential space” of the urban, denoting a space that brings together and is produced by contrasts and contradictions, a “pure form” that unites an ensemble of differences, encounters, and simultaneity (Lefebvre, 1974/1991, 1970/2003). Whereas *isotopia* refers to “everything that makes a place the same place” and *utopia* to that which does not have a place, *heterotopia* accounts for what makes a place “a different place,” “an other place,” through relating it to other spatial trajectories. Third space is a transformational concept that includes “an-Other”; it facilitates the questioning of assumptions and the renegotiation of boundaries, meaning, and cultural identity. These spatial concepts empower our understanding and use of the organizational space in which we strive to support collective creativity—space intervenes in the quality of learning and multiplicity of values shared and where dissonant and concordant trajectories converge. In other words, third space connotes a space where multiplicity can be engaged and shared; meanings negotiated, and learnings from difference honored; to practice DP, we need intentionally to seek and create these spaces and leave our more typical silos.

Dialectical Pluralism Interpersonal Perspective 3: Transformational Change Is Messy, Nonlinear, and Slow

Not content with simple change notions, DP helps us to generate expectations for high-order change such as transformation. Here, we highlight four methods for facilitating transformative-oriented DP work: future search, open space, world café, and appreciative inquiry. Dialogue is core. Although the origin of these dialogical circle techniques are murky, linkages to Freire’s (1970) use of communal praxis can be seen. These methods have been developed as part of the organizational development field as “whole system” change strategies that include all participants in a network, leverage diversity and breadth of a network, and focus dialogue toward the common intent of building a shared vision.

First, *future search* (Weisbord & Janoff, 2000) (futuresearch.net) brings all stakeholders together, maps the path of interactions, establishes common ground using jointly negotiated themes, and then searches for innovative strategies to build a common commitment to a shared future. People follow a generic agenda, regardless of topic. It consists of four or five half-day sessions on the

Past, the Present, the Future, Common Ground, and Action Planning. The techniques used—time lines, a mind map, creative future scenarios, common ground dialogue—are all managed to support the principles of a collaborative process to search for shared futures. People need no special training, orientation, vocabulary, or background to participate. They work in small groups, make reports to the whole, and join in whole-group dialogues on what they are learning. Future search managers practice a “hands-off” approach to facilitation. Although the literature documenting the success of this approach is extensive, this model tends to fail when (a) participants do not perceive a need to work together, (b) many participants do not want really to act, (c) key stakeholders are missing, (d) insufficient time is allowed for the size of the task, and (e) overactive/controlling facilitation is present. Given the potential complexity inherent in DP-driven mixed research, these are important considerations to anticipate.

Second, *open space* (openspaceworld.com) asks participants to identify topics they want to talk about and then provides breakout rooms for anyone interested to gather, explore, and develop commitments (Owen, 1997). Open space technologies (OST) have been used in meetings of five to more than 2,000 people. An example of open space occurs when the sponsor (the official or acknowledged leader of the group) introduces the purpose; a single facilitator then explains the “self-organizing” process called “open space.” Then, the group creates the working agenda as individuals post their issues and opportunities in bulletin-board style. Chairs are arranged in a circle; a “marketplace” with many breakout spaces emerges; participants move freely among the spaces, learning and contributing as they “shop” for information and ideas; a “breathing” pattern of flow occurs between plenary and small-group breakout sessions. The approach is most distinctive for its initial absence of an agenda, which sets the stage for the meeting’s participants to create the agenda for themselves. Each individual “convener” of a breakout session takes responsibility for naming the issue, posting it on the bulletin board, assigning it a space and time to meet, and then later showing up at that space and time, kicking off the conversation, and taking notes. These notes are usually compiled into a proceedings document that is distributed physically or electronically to all participants. The ideal facilitator is described as being “fully present and totally

invisible” (see Owen, 1997, User’s Guide), “holding a space” for participants to self-organize rather than managing or directing the conversation.

Third, *world café* (theworldcafe.com) is an orchestrated series of small-group dialogues focused on a common question that elicits member ideas and innovations. Five components comprise the basic model:

- *Setting*: Create a “special” environment, most often modeled after a café (i.e., with small round tables, butcher block paper, colored pens, and an optional “talking stick” item). There should be four chairs at each table.

- *Welcome and introduction*: The host begins with a welcome and an introduction to the World Café process to set the context, sharing norms of the Café Etiquette.

- *Small group rounds*: Begin with the first of three or more 20-minute rounds of conversation for the small group seated around a table. At the end of the 20 minutes, each member of the group moves to a different new table. They may or may not choose to leave one person as the “table host” for the next round; this person will welcome the next group and briefly fill them in on what happened in the previous round.

- *Questions*: Each round is prefaced with a question designed for the specific context and desired purpose of the session. The same questions can be used for more than one round, or questions can be built on each other to focus the conversation or guide its direction.

- *Harvest*: After the small groups, individuals are invited to share insights or other results from their conversations with the rest of the large group. These results are reflected visually in a variety of ways.

Fourth, *appreciative inquiry* (appreciativeinquiry.case.edu) is a process for all stakeholders to discover positive core values expressed by stories of the past and then use these values to envision a desired future state and co-construct an action plan to achieve it. Cooperrider, Whitney, and Stavros (2008, p. 433) define appreciative inquiry (AI) as “the “cooperative search for the best in people, their organization, and the world around them. It involves the systematic discovery of what gives a system ‘life’ when the system is most effective and capable in economic, ecological, and human terms.” Appreciative inquiry looks for the positive characteristics and ideas in different people and groups in order creatively to improve groups and organizations (Boyd & Bright, 2007; Dunlap, 2008; Moore, 2008). At the heart of the process is

its opening with an “affirmative topic.” The process then engages in a “4D cycle.” The four parts of the cycle are (1) discovery (appreciating the best of “what is” and looking at the strengths of the people in the context); (2) dreaming (where one imagines and considers what might be possible and visioning); (3) designing (where one determines “what should be”), and (4) delivery or destiny (where one empowers, adjusts, and creates “what will be”) (Cooperrider et al. (2008, pp. 69, 101). This approach focuses on identifying the positive and creating a positive future. In the context of mixed research, this involves identifying the many positives of postpositivism, constructivism, critical theory, and other paradigms and the many benefits of qualitative and quantitative methods.

Strategies to Support Dialectical Pluralism

The group process toward the path of DP strives to create a values mélange of trust, conflict, cooperation, diversity, reflection, reflexivity, negotiation of multiple realities, and dialogue. This chapter along with Johnson’s (2011a) recent work identify social psychological strategies from multiple literatures, including peace-and-conflict studies, conflict management, negotiation, innovation, counseling, and small-group research. The following list includes several key research-based support conditions and related strategies:

- *Trust*: Continually develop trust and empathy.
- *Conflict*: Engage in constructive conflict; practice constructive criticism.

- *Cooperation*: Practice reciprocity; peace comes with balanced or equal power.

- *Diversity*: Try to understand the interests and reasons and benefits behind divergent perspectives; view difference and conflict as normal and good—they drive change; identify truth value and insights in different perspectives; realize that many differences are complementary (differential consensus); continually use differences to drive creativity and innovation.

- *Personal*: Continually practice self-reflection; focus on learning rather than influencing.

- *Negotiation of realities*: Identify and invoke common interests, goals, and desired outcomes; develop multifaceted and holistic truths; wholes can incorporate similarity and difference, divergence and convergence; focus on creating the desired future(s); group development/change is not linear (punctuated equilibrium).

Table 27.1 Implementation framework for dialectical pluralism in mixed research and evaluation

Purposes	Design	Phases of DP in MMR/E and Enabling Strategies	
		Grow	Perform Strategies
1. Develop DP individual attitudes and capacity 2. Group development and DP process building 3. Optimize DP values in individuals/ networks and systems	Design for DP: Identify stakeholders; build relationships; Clarify purpose; Identify contexts (e.g., uncertainty, paradoxes, ambiguity)	Grow capacity of DP structural, human and relational capital: Systems theory, complexity, local and general evidence-based	Perform DP with enabling strategies: Maintain momentum and interaction, manage problems, respond to changes, move toward transformation/whole systems change, and strive for win/win solutions
Strategies for Purpose 1: Individual Capacity Building by Phases			
Question personal and DP assumptions: – Philosophy/values – Members – Collaboration – Systems theory – Values (means and ends) – Outcomes of MMR/E	– Conduct individual needs assessment – Identify paradigms present as baseline – Identify DP indicators and desired outcomes	Polarity mapping ²² : – Top-down vs. emergent – Outcome vs. discovery – Closed vs. open – Routine vs. innovative vs. transformative MMR/E models	– Facilitate collaborative logic modeling (CLM) ⁵ to frame discussions about individual and group assumptions – Identify preconditions and assumptions with ladder of inference ¹⁹ – TIP to manage complexity, uncertainty, ambiguity, paradox ²¹
Understand strengths of contexts and people in MMR/E network	– Clear membership criteria – Diverse Knowledge/ expertise honored	– Build tolerance for ambiguity and diversity in knowledge, perspectives, styles	– Assess needs with Appreciative inquiry ¹
Identify individual capacity to play key roles: – Sponsors, leaders, steering, core members – Clarify contributing roles: knowledgeable others	– Potentially support collaborative – Seek areas of Commonality – Develop plan to expand roles of participants over time	– Support collaborative actions – Build internal and external facilitator capacity – Establish roles for individuals to broker connections – Negotiate membership boundaries	– Identify and support distributed leadership ² – Identify and support influencers and “boundary crossers” across networks via thirdspace strategies ³

Table 27.1 (Continued)

Purposes	Phases of DP in MMR/E and Enabling Strategies		
	Design	Grow	Perform Strategies
Strategies for Purpose 2: Group Development and DP Process Building by Phases			
Understand interactions in MMR/E network: structure and governance model, norms, valuing relationship, group development/change is not linear (punctuated equilibrium)	<ul style="list-style-type: none"> – Context setting – Supports dialogue and inquiry, which enhances trust, reciprocity, and emergence 	<ul style="list-style-type: none"> – Facilitate discussions – Plan and update tech infrastructure – Transition to hybrid structures – Event logics increase as goal becomes more abstract – Monitor decision making 	<ul style="list-style-type: none"> – VNA⁴, CLM⁵ interpreted in context and balances transactional and knowledge-based interactions – Practice reciprocity⁸ – Members self-monitor CoP functioning with processes and rubrics such as: conflict management process²⁴, 4 C's of team development²⁶, Debriefing interviews²⁷, and CoPCAR¹⁶
Build support for group conditions/trust	<ul style="list-style-type: none"> – Individual respected inclusion in visioning and goal setting foster alternative viewpoints 	<ul style="list-style-type: none"> – Culturally responsive framework – Core values negotiated – Create healthy competition balanced with cooperation 	<ul style="list-style-type: none"> – Future search⁷ – Open space⁸ rules: Whoever comes are the right people – Radical collegiality¹⁰
Interact both formally and informally	<ul style="list-style-type: none"> – Engage with events – Respond to challenging questions – Orientation sessions – Time to explore; anecdote collection, self-organizing 	<ul style="list-style-type: none"> – Networked learning opportunities – Disseminate info ahead of events – Create open-ended agendas – Sanction time to dialogue deeply 	<ul style="list-style-type: none"> – Data retreats¹¹ – Collaborative creativity¹² – World café¹³ – Scenario mapping¹⁴ – Dialogue or study circle¹⁵ – Members self-monitor CoP functioning with rubrics such as CoPCAR¹⁶ – Open space⁸
Define deliverables as a group: <i>Every ending is a new starting point</i>	<ul style="list-style-type: none"> – Articulated 	<ul style="list-style-type: none"> – Seek overt and tacit values – Iterative 	<ul style="list-style-type: none"> – Co-design/co-deliver CLM⁵ – Negotiate SNA tools¹⁷

Strategies for Purpose 3: Optimizing DP Values Systemically by Stage

Optimize value of collaborating and networking: <i>Focus on learning rather than influencing</i>	– Develop multifaceted and holistic truths	– Commit to collaboration – Track path of win-wins and other mixtures	– Snowden's complexity-based sense making ¹⁸ – VNA ⁴
Appreciate interests and reasons behind divergent perspectives; identify problems/tensions: Realize many differences are complementary (differential consensus)	– Collect anecdotes and metaphors – Collaborate with other networks on a task – Develop multifaceted and holistic truths	– Engage in constructive conflict – Problem-solving heuristics – Monitor change triggers: planned, discovered, dynamic, asymmetric – Peace comes with balanced or equal power	– Appreciative inquiry ¹ – Critical friend protocol ²³ – Delphi ²⁴ – Recycle/revisit CLM ⁵ – Conflict management ¹¹ – Ecological Systems model of MMR ²⁸
Increase value of tangible and intangible: Focus on learning rather than influencing	– Enable discovery – Add communication channels – Increase diversity by creating ties with other networks		
Increase value for network/CoP	– Leverage diversity with reward structures		

1. Appreciative inquiry (Cooperrider & Srivastva, 1987)
2. Distributed leadership (Spillane, 2012)
3. Third space strategies http://www.academia.edu/380186/Navigating_Third_Space_and_the_Construction_of_Hybridity_Narratives;
4. Value Network Analysis (VNA) (Allee, 1997)
5. CLM (Kaplan & Garrett, 2005)
6. Reciprocity (Cialdini, 2009)
7. Future search (Weisbord & Janoff, 2000)
8. Open space (Owen, 1997)
9. Conflict management: (Lewicki et al., 1992)
10. Radical collegiality (Fielding, 2004)
11. Data retreats: (Sargent, 2003)
12. Collaborative creativity (Sawyer, 2008)
13. World café (theworldcafe.com)
14. Scenario mapping (www.uxforthemasses.com/scenario-mapping)
15. Dialogue or study circle (Abdullah, C. M., & McCormack, 2008; Freire, 1970; Lewis, 2004)
16. Community of Practice: Collaborative Assessment Rubric (CoPCAR) rubric (Gajda & Koliba, 2007): https://confluence.umassonline.net/download/attachments/26771985/am+jour+of+eval+2007_001.pdf
17. Social network analysis (SNA) tools: http://en.wikipedia.org/wiki/Social_network_analysis_software
18. Snowden's (2005) complexity-based sense making
19. Ladder of inference model to reveal assumptions (Argyris et al., 1985)
20. Tools for dealing with uncertainty, ambiguity, and paradox: reflective methods for group development (Murray, Ross, & Ingles, 2008)
21. TIP to support comprehensive decision making at the meta level that complexity seems to demand (<http://www.global-arina.org/researchprojects/TIP.html>)
22. Polarity mapping by Barry Johnson (1992), www.polaritymanagement.com
23. Critical friend protocol: www.nsrharmony.org
24. Delphi: Rowe & Wright, 2001
25. Four part conflict management process developed by Fisher & Ury (2011)
26. Four C's of team development by Dyer et al 2007
27. Debriefing interviews (Collins, K. M. T., Onwuegbuzie, A. J., Johnson, R. B., & Frels, R. K. (2013).
28. Ecological systems model of mixed methods research process (Onwuegbuzie, A. J., Collins, K. M. T., & Frels, R. K. (2013).

- *Dialogue*: Thoughtful-empathetic discussion is progress; every ending is a new starting point; keep coming back to agreed-upon goals; use social agreement to produce socially valued change.

A Framework for Implementing Dialectical Pluralism

Here, we present a new framework for applying our *mélange* of DP strategies in order to work toward a constructive combination or synthesis of the conditions mentioned earlier in this chapter:

- Multiple realities, justices, and perspectives (including those with the least power, based on Rawls's second principle of justice)
 - Dialectical integration with reflexive dialogue
 - Dynamic, interactive, and holistic combinations/syntheses
 - Collaboration for win-win solutions by revealing and reframing tangible and intangible values
 - Integrative complexity developed through paradox frames
 - Positive outcomes facilitated by purposive team composition, team development, and change management

Table 27.1 summarizes our Dialectical Pluralism Implementation Framework built around three purposes:

- *Building the capacity of individual team members* to articulate their philosophy, visions, values, and research goals
- *Facilitating group interactions* to build the necessary conditions for intentional group goals and values-sharing dialog and trust
- *Systemically optimizing values* that can support DP conditions and communities of practice

These three purposes move across three phases: (1) a *design phase* to match strategies with team members and group needs; to (2) a *capacity-building phase* to grow structural, human, and relational capital; culminating in (3) a *DP performance phase* to harness implementation strategies as well as achieve fidelity and sustainability.

Dialectical Pluralism in Mixed Research as a Diffusion Process

Rogers's (2003) theory of *diffusion of innovations* provides another metaphor (i.e., diffusion of innovations) and framework for heterogeneous team development and action to support the DP process. According to Rogers's theory, diffusion is the process by which an innovation is communicated, or

diffused, via specific modes over time among the members of a sociocultural system. As such, diffusion is a form of social change, that is, the process by which "alteration occurs in the structure and function of the social system" (Rogers, 2003, p. 6). This theory can be mapped onto the mixed research team process, wherein the mixed research study serves as the innovation and the members of the sociocultural system are represented by the mixed research team. (Note the importance of constructing an inclusive and representative team.) The theory of diffusion of innovations is particularly applicable for large and diverse mixed research teams. This theory can be used by the mixed research process facilitator(s) to help optimize team synergy.

Rogers (2003) theorized that four main elements influence the dissemination of an innovation: the innovation itself, communication channels, time, and a social system. In terms of the mixed research team process, the four elements are the mixed research study, the manner in which the mixed research team members communicate, time, and the mixed research team itself. With regard to the research study, one or more members of a mixed research team initially might not have formed favorable or unfavorable attitudes toward the study. Attitudes toward the study might be expressed in terms of the extent and level of enthusiasm proffered by each team member. Among the process questions investigated by the process facilitator(s) during the conduct of the study are (a) how the *early adopters* (i.e., mixed research team members who initially embrace all qualitative, quantitative, and mixed components and phases of the research study regardless of research orientation [i.e., quantitative research orientation, qualitative research orientation, or mixed research orientation]) differ from the *later adopters* (i.e., mixed research team members who do not initially embrace all qualitative, quantitative, and mixed components and phases of the research study); and (b) how the perceptions of a team member about the role her or his research tradition are situated in the overall mixed research study.

With respect to communication channels, communication refers to the process by which the mixed research team members effectively communicate with each other to create synergy. This indicates that a team-based mixed research study is a social process. Rogers (2003) described the concept of *homophily*, which is "the degree to which two or more individuals who interact are similar in certain attributes" (p. 19) and *heterophily*, the opposite of homophily, which is the "the degree to which two or more individuals who

interact are different in certain attributes” (p. 19). An example of *homophilous researchers* are researchers in a mixed research team who represent the same research tradition or who represent the same field or discipline, whereas *heterophilous researchers* are researchers in a mixed research team who represent different research tradition or who represent different fields or disciplines regardless of research orientation. Thus, the more heterophilous the mixed research team is, the greater the challenge for the process facilitator(s) to promote team synergy. As noted by Rogers (2003), “one of the most distinctive problems in the diffusion of innovations is that the participants are usually quite heterophilous” (p. 19).

Time, the third element of the mixed research diffusion process, is represented by (a) the innovation-decision process, by which the research team member passes from knowledge of the research study to its full adoption/embrace or rejection; (b) the relative earliness/lateness with which the research study is fully adopted/embraced by a team member relative to the other members of the team; and (c) the rate of full adoption/embrace of the research study, as measured by the number of researchers in the team who fully adopt/embrace the research study in a given time period.

In the context of mixed research, the innovation-decision process is the mental process through which an individual member of the mixed research team passes from knowledge of the research study to forming an attitude toward the study, to a decision fully to adopt or to reject one or more elements of the research study, to confirmation of this decision. As such, there are five steps in the mixed research diffusion process: (a) knowledge, (b) persuasion, (c) decision, (d) implementation, and (e) confirmation. The process facilitator(s) should obtain information from every team member at various stages of the innovation-decision process to minimize uncertainty about the mixed research team’s expected outcomes.

According to Rogers (2003), five adopter categories represent the innovativeness or, in our case, represent a propensity to embrace fully all components of the mixed research study including its process and its outcomes. First are the *innovators*. They will be the first researchers to embrace fully every component of the research study, and they are typically the mixed researchers in the team. Second are the *early adopters*. They are the second fastest group of researchers (broadly defined) who embrace fully every component of the research study; they have the highest degree of *opinion leadership* among the other

adopter categories inasmuch as they are able to influence informally other researchers’ attitudes toward the entire research study. Third is the *early majority*. This is the group of researchers who embrace fully every component of the research study after varying degrees of time. Fourth is the *late majority*. This is the group of researchers who embrace fully every component of the research study after the majority of the team members; they approach the mixed research study with a high degree of skepticism about one or more elements of the research study (e.g., the qualitative components) and show little opinion leadership. Last are the *laggards*. These are the last researchers on the team to embrace fully every component of the research study, if at all, and they often show little or no opinion leadership. Although we focused on a relatively large research team here, note that the diffusion metaphor applies equally well to the acceptance and use of the empirical research findings and recommendations. For an additional theory of use of research and evaluation results, see Johnson (1998).

Promoting Intraperson and Interperson Dialogue in Dialectical Pluralism Mixed Research

Another effective way of promoting both intraperson- and interperson dialogue (e.g., Dyer et al.’s [2007] four Cs of team development; Roger’s [2003] diffusion of innovation; Fisher & Ury’s [2011] four-part conflict management process; Sawyer’s [2008] collaborative creativity; Onwuegbuzie, Collins, & Frels’s [2013] ecological systems model of the [mixed] research process; Cooperrider et al.’s [2008] appreciative inquiry) in DP mixed research uses a strategy conceptualized by Collins, Onwuegbuzie, Johnson, and Frels (2013) that they referred to as *debriefing interviews*, which are conducted throughout the research process systematically to discuss and reflect on the on-going research process (broadly viewed). For instance, interviews, which are conducted by a designated process facilitator with every member of the mixed research team (broadly viewed), could be used to identify innovators, early adopters, early majority, late majority, and laggards. As another example, interviews also could be used to identify conflict among team members and their reasons to help engage in Fisher and Ury’s (2011) four-part conflict management process. Collins et al. provide examples of interview questions to document (throughout the research process, not just at the end) the degree that researchers are meeting quality criteria such as philosophical

clarity (i.e., the researcher's epistemological and values stance, and the degree that this stance influences the researcher's research decisions) and incorporation of multiple standpoints. The ultimate goal of these interviews is to identify and avoid group process failures (e.g., groupthink, social loafing, social inhibition, confirmation bias, foreclosure).

Dialectical Pluralism Dialogues with Philosophical and Methodological Issues

We contend that DP and the related strategies presented here are especially important for conducting equal-status or interactive mixed research. The conduct of strong mixed research (i.e., equal-status or interactive designs) will require a single researcher or, much more frequently, an inclusive and heterogeneous and representative team of researchers to determine the research purpose and questions, and also it will require dialogue, with many differences and tensions present in this form of research. We now examine DP in relation to ontology, epistemology, axiology, and methodology. Dialectical pluralism allows us to interact dialectically with multiple ontologies, epistemologies, values, and methods or methodologies.

Ontologically speaking, DP is committed to ontological pluralism (i.e., the existence of many important kinds of reality). For example, DP recognizes subjective, intersubjective, and objective reality. Subjective reality is personal. Intersubjective reality is social, cultural, and language based. Objective reality is seen in material, physical, and process realities; for example, the material and process thing we call the earth will exist tomorrow if all humans ceased to exist. Objective reality also is seen in causation where, for example, an event or process at one level of reality affects another at that level (car hitting human produces death) or something at one level produces something at another level; for example, human beliefs affect culture and culture reciprocally affects human beliefs. Another sort of pluralist reality is seen in disciplinary and paradigmatic realities. Going beyond basic pluralism, however, DP also provides and emphasizes a process of "dialectical listening" to multiple ontologies such as idealism, ontological relativism, ontological realism, process metaphysics, and emergence. *Dialectical pluralism takes the existence of multiple realities and ontologies as a strength to be embraced, not as a weakness to shut down conversation.*

Epistemologically speaking, DP is committed to a dialectical, dialogical, and hermeneutical process for knowledge discovery, construction, and reconstruction. It also provides a process to engage with

multiple epistemologies, among them empiricism, rationalism, constructivism, scientific realism, epistemological relativism, pragmatism, contextualism, interpretivism and hermeneutics, evolutionary epistemology, and communicative rationality. The process of DP helps us to produce new wholes (that are concurrently homogeneous and heterogeneous) that value multiple standpoints and result in solutions that work in theory and practice. Dialectical pluralism relies on the process of "epistemological listening" in each team and each research project, resulting in many complex instantiations over time. Dialectical pluralism produces "thick" (i.e., value embedded) knowledge. It produces provisional truths or lower case "t" truths, not final, universal, and timeless capital "T" Truth. *Dialectical pluralism takes the existence of multiple epistemologies as a strength to be embraced, not as a weakness to shut down conversation.*

Ethically or axiologically speaking, DP is committed to the importance of multiple values and multiple goods. For example, political science is sometimes said to study the tension between freedom and equality. Dialectical pluralism uses the strategy of recognizing the importance of both, rather than emphasizing that one is always more important than the other. Intellectually speaking, DP provides a way to interact with multiple ethical theories, including ethical relativism, ethical realism, utilitarianism, deontology, ethics of democracy, social justice, discourse ethics, and standpoint ethics. Also, there are many social values and epistemic values/virtues to consider dialectically. Our own perspective is that mixed research should not simply attempt to construct knowledge, but should also promote procedural, social, and global justice. We also suggested that DP attempt to achieve Rawls's (2001) justice as fairness and to listen carefully to Rawls' (1999) Law of Peoples. Ultimately, however, each research team is responsible to decide what specific values to emphasize in each study in its local context. (Recall that team members should be representative of methodological and stakeholder groups, and always include voices from those who are disadvantaged.) In mixed research, DP provides a way, on a project-by-project basis, to interact with the situation, stakeholders, and research needs creatively to combine multiple ethical concepts and values into a justifiable whole. *Dialectical pluralism takes the existence of multiple needs, values, and ethical theories/principles as a strength to be embraced, not as a weakness to shut down conversation.*

Methodologically speaking, DP is committed to the importance of multiple methodologies and methods, with the combination to be determined

on a project-by-project basis; these methodologies include qualitative, quantitative, and mixed research; multimethod research; feminist methods; scientific naturalism; methodological humanism; queer theory; confirmation theory; and inference to best explanation. Dialectical pluralism provides a process to dialogue with multiple methods and methodological issues, and we suggest that it should privilege the mixing of methods that are very different. We also do not believe that each research study has to focus on a single purpose for mixing. Rather, a single project might give attention to two or more purposes. Furthermore, one project might have larger superordinate purposes, as well as more specific purposes. Greene (2007) provides the most popular list of purposes for mixing, which include triangulation complementarity, development, initiation, and expansion. One should not be bound to any list or typology, however; using DP, you can creatively construct new purposes with emergent properties. Not surprisingly, *DP takes the existence of multiple methodologies and methods as a strength to be embraced, not as a weakness to shut down conversation.*

A Case Example of Walking the Talk: A Story About One Path Toward Dialectical Pluralism Implementation

The path to DP is a nonlinear, reflexive journey in which it is more an iterative process than a product. The example here is a composite of three recent DP-infused mixed research, large-scale education partnership projects implementing DP by project staff, researchers, and evaluators. Although all three projects are a good “fit” for DP, this composite synthesis was designed to highlight some of the challenges that can emerge and that can become opportunities for program strengths if collaboratively resolved. Many enabling strategies mentioned in Table 27.1 were used.

CONTEXT

The following illustrative example involves the development of mixed research in the context of a 5-year federally funded university–school educational partnership between several universities and 20 school districts.² As part of the application, the funders required a logic map be submitted that aligned project values, resources, activities, goals, and outcomes. Another expectation was an articulated plan leveraging the work of a diverse team of researchers and external evaluators. The external evaluator facilitated a collaborative logic modeling process (see Table 27.1

note #5 for more information about CLM), which brought project management, developers, researchers, and evaluators into the broiling atmosphere of deadline-driven grant writing. DP provided a collective space for dialoguing across divergent philosophical and methodological paradigms when it was recognized no one perspective would be acceptable to all stakeholders and that the team needed to develop a more actionable common vision for both the early implementation and to build sustainability over time. Mixed methods plans were negotiated for both research and evaluation. In that crystal ball phase, before the team knew if they were funded, they needed to have a process logic that supported the program logic and negotiate core values over the course of a 5-year funding cycle. As a team, the grant designers integrated DP exploration as part of its interperson dialogue. Valued means and valued ends were elicited using a ladder of inference approach (see Table 27.1, note #19) and woven into a comprehensive logic model, which the team was challenged to review and refine annually.

DIALECTICAL PLURALISM ENABLING STRATEGIES

During the first two years, the evaluators and researchers formed a Community of Practice (Table 27.1 note #16), along with representatives of the management team and developers to ground the work holistically. Reflection was facilitated by having quarterly data retreats (Table 27.1 note #11) at which researchers worked to align their questions and designs with project core assumptions (e.g., K20 partnering, distributed leadership of low- and high-status participants, translating Mertens’s (2012) transformative values into action, continuous improvement, and justice strategies to diminish the achievement gap). Appreciative inquiry (Table 27.1 note #1) was helpful in the first year of the grant but did not support improvisation in the second year. Collaborative creativity and group flow (Table 27.1 note #12) were applied to two of the three research questions, which also were a better fit with DP-infused mixed research methodology.

WHEN SHIFT HAPPENS TO TEAM COMPOSITION AND COMPETENCIES

The third year of operation has been crucial for shifting from exploratory to confirmatory research. DP-infused processes have been useful in collaboratively refining mixed research questions at three levels: individual, CoP (between representatives

from program implementation, researchers, and evaluator), and networking across multiple districts and universities aided by value network analysis (Table 27.1 note #4) and social network analysis tools (Table 27.1 note #17). Although the number of districts expanded each year, and some stakeholders and researchers have changed, the need for teaming escalates as pressure for sustainability builds—including leveraging diversity of project stakeholders and participants to implement sustainable professional development and mixed methods processes and products across research and evaluation. With the advent of significant changes in the research team in the middle of the third year, DP team composition was re-examined for changes on leveraged skill sets, motivation to engage in constructive conflict, trust, and team competency. New resulting tensions caused by composition changes included:

- *Axiological distancing*: In this case, axiology is concerned with the principles and values in the practice of educational research and evaluation. Given the diversity of principles and values expressed in the practices of each of these fields, it is no wonder that tensions can arise. With the change in researchers came a change in research perspective, aims, methods, expertise, and reflexivity. Rising tensions emerged about core assumptions of the value of each field, methods which are compatible with “transformative” values of the original proposal, implementation of data collection, and data sharing.

- *Ontological and epistemological tensions*: Multiple realities are increasingly articulated, but tensions about postpositivist QUAN-dominant researcher privilege about acceptable or actionable reality are currently juxtaposed against a program improvement evaluation approach, which has been implementing an equal-status transformative mixed research approach. These tensions are being mediated via data retreats facilitated by a group process expert.

- *Methodological expediency pressures*: Collaboration and partnership are values at the core of the project. The inclusion of dialogic methods were in the original proposal and practiced by the original researchers but are valued differently by their replacements along with new tensions to publish and develop internal funding in a post-downturn economy.

In the third year, data retreats have been increased to monthly meetings, and include a

professional external facilitator to free up the project management to be able to participate in a more equal status way with researchers, staff, and evaluators. Data retreats have become a trusting third space for data sharing and collaborative reflection across all critical stakeholders—genuine collegiality is emerging. Rather than facilitating conflict management, there is a strong community of practice emerging which has shifted to 1) more collaborative creativity; 2) heightened opportunities for reciprocity and trust at individual/CoP and system levels, and 3) DP capacity and commitment to use mixed methods collaboratively which promises more cost-effectiveness and sustainability.

Three Major Kinds of Mixed Research

We have spent a lot of time explaining the process of DP and related research-based strategies. We believe DP makes equal-status mixed designs possible. It also can be used in other major types of mixed designs (Johnson, Onwuegbuzie, & Turner, 2007). Because some readers will not be familiar with the three major types of mixed designs, we briefly explain them here. It is important to dispel the myth that there is only one kind of mixed research or only one style of design. In fact, the three types presented here are only a few of *many* ways of construing mixed designs, and many additional and more specific approaches will continue to appear in future literature.

The first of the major forms of mixed methods research is qualitatively driven mixed research or QUAL + quan (Morse, 1990). Qualitatively driven mixed research occurs when the mixed researcher aims to address one or more research questions using any lens associated with the qualitative research paradigm (e.g., constructivist, critical theorist) while simultaneously believing that adding quantitative research approaches (particularly the use of quantitative data and analysis) can help to address the research question(s) to a greater extent (Johnson et al., 2007). At the analysis stage of mixed research process, qualitatively driven mixed research implies combining quantitative and qualitative analytical techniques at various levels wherein the qualitative analyses are privileged (Onwuegbuzie, Leech, & Collins, 2011). A detailed chapter on *mixed analysis* is forthcoming by Onwuegbuzie and Hitchcock (in press).

The second of the major forms of mixed methods research is quantitatively driven mixed

research or QUAN + qual (Morse, 1990). Quantitatively driven mixed research occurs when the mixed researcher aims to address one or more research questions using a postpositivist (quantitative) lens while simultaneously believing that inclusion of qualitative research approaches (particularly the use of qualitative data and analysis) helps to address the research question(s) to a greater extent (Johnson et al., 2007). At the analysis stage of mixed research process, quantitatively driven mixed research implies combining quantitative and qualitative analytical techniques at various levels wherein the quantitative analyses are privileged.

The third of the major forms of mixed methods research is equal-status or “fully” interactive mixed research; popularly designated as QUAL + QUAN. Equal-status mixed research occurs when the mixed researcher aims to address one or more research questions using simultaneously and approximately equally a lens that is associated with the qualitative research paradigm (e.g., constructivist, critical theorist) and a lens that is associated with the quantitative research paradigm (i.e., postpositivism) (Johnson et al., 2007). Because these paradigms are in tension and sometimes contradictory, they can only be “merged” dynamically and dialectically using DP. At the analysis stage of mixed research process, equal-status mixed research implies combining quantitative and qualitative analytical techniques at various levels wherein the qualitative analyses and quantitative analyses are given approximately equal weight. That is, neither tradition is privileged.

Within the mixed research community, note that there seems to be two different versions of equal-status designs: weak and strong. The weak version requires only an approximately equal mixture of qualitative and quantitative data. Although this is a popular and practical approach, it ignores the deeper philosophical assumptions and issues about which the “paradigm war” and related dialogues are based. It is a bit simplistic to assume that avoidance will pacify qualitative and quantitative related paradigms. The strong version relies on DP dialectically and dynamically to mix/combine philosophies and paradigms and it tends to be more explicitly concerned with misuse of power and resolving longstanding differences in the academy about knowledge. Generally speaking, we prefer the strong version. We hope we have demonstrated in this chapter that dialectical pluralism is especially important for the strong version and that DP

provides a way to listen to and creatively to merge divergent perspectives.

Conclusion and the Future

We have provided a rationale, philosophy, and strategies for the conduct of mixed research. If one is curious about what mixed research rejects, here are our suggestions: Mixed research based on dialectical pluralism rejects the following:

- Dogmatisms, reductionisms, monisms, and one-way-isms;
- Essentialist definitions that are resistant to change or improvement over time;
- Most universalisms (e.g., other than universal human rights and a few moral principles);
- Scientism; and
- Nihilism as an end goal.

We contend that researchers coming from different philosophies, politics, and methodologies need to be open minded, listen to the other, and continually construct new, creative, and better research together. This is why we have emphasized the importance of research teams in this chapter.

One might, last, ask What are some attitudinal dispositions that lend themselves to high-quality mixed research? We suggest that, if you hope to conduct an equal-status or interactive mixed research study, you use DP to interact dialectically with the poles and multiple dimensions of concept pairs and sets of family-related concepts such as similarity *and* difference; quantity *and* quality; induction, deduction, abduction, dialectic, hermeneutics, *and* criticism; natural science *and* human science; objectivity *and* subjectivity; etic *and* emic perspectives; structure *and* agency; explanation *and* understanding; reason, emotion *and* faith; facts *and* values; nomological *and* idiographic causation and knowledge; and knowledge *and* wisdom. Our version of mixed research emphasizes ontological pluralism (there are many important kinds of reality); respecting the values of others, with an emphasis on improving the situations of those with the least power in research and society; and continual epistemological and methodological dialogue using the process philosophy of DP. You will need to examine multiple sides of issues; learn about your own weaknesses; never fail to hear, listen to, and understand “the Other”; thrive on differences, and never accept any proclamation that mixed research is not possible. Mixed research is very much possible, and we hope that you will team with others who are different from you to make it happen in new and better ways.

Notes

1. These two principles from Rawls are viewed as useful to include in a dialogue about the tension between freedom and equality. However, dialectical pluralism is not based on any one person's work. That would contradict the fundamental concept of DP which is about continual dialogue, process, combination, learning from the other, action, change, and development.

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PART 6

Analysis, Interpretation,
Representation, and
Evaluation



Coding and Analysis Strategies

Johnny Saldaña

Abstract

This chapter provides an overview of selected qualitative data analytic strategies with a particular focus on codes and coding. Preparatory strategies for a qualitative research study and data management are first outlined. Six coding methods are then profiled using comparable interview data: process coding, in vivo coding, descriptive coding, values coding, dramaturgical coding, and versus coding. Strategies for constructing themes and assertions from the data follow. Analytic memo writing is woven throughout the preceding as a method for generating additional analytic insight. Next, display and arts-based strategies are provided, followed by recommended qualitative data analytic software programs and a discussion on verifying the researcher's analytic findings.

Key Words: analysis, analytic memo, analytic strategies, assertions, codes, coding, data analysis, interpretation, qualitative data analysis, qualitative research

Coding and Analysis Strategies

Anthropologist Clifford Geertz (1983) charmingly mused, “Life is just a bowl of strategies” (p. 25). *Strategy*, as I use it here, refers to a carefully considered plan or method to achieve a particular goal. The goal in this case is to develop a write-up of your analytic work with the qualitative data you have been given and collected as part of a study. The plans and methods you might employ to achieve that goal are what this article profiles.

Some may perceive *strategy* as an inappropriate if not colonizing word, suggesting formulaic or regimented approaches to inquiry. I assure you that that is not my intent. My use of strategy is actually dramaturgical in nature: strategies are actions that characters in plays take to overcome obstacles to achieve their objectives. Actors portraying these characters rely on action verbs to generate belief within themselves and to motivate them as they interpret the lines and move appropriately on stage. So what I offer is a qualitative researcher's array of actions

from which to draw to overcome the obstacles to thinking to achieve an analysis of your data. But unlike the pre-scripted text of a play in which the obstacles, strategies, and outcomes have been predetermined by the playwright, your work must be improvisational—acting, reacting, and interacting with data on a moment-by-moment basis to determine what obstacles stand in your way, and thus what strategies you should take to reach your goals.

Another intriguing quote to keep in mind comes from research methodologist Robert E. Stake (1995) who posits, “Good research is not about good methods as much as it is about good thinking” (p. 19). In other words, strategies can take you only so far. You can have a box full of tools, but if you do not know how to use them well or use them creatively, the collection seems rather purposeless. One of the best ways we learn is by *doing*. So pick up one or more of these strategies (in the form of verbs) and take analytic action with your data. Also keep in mind that these are discussed in the order in

which they may typically occur, although humans think cyclically, iteratively, and reverberatively, and each particular research project has its own unique contexts and needs. So be prepared for your mind to jump purposefully and/or idiosyncratically from one strategy to another throughout the study.

QDA (Qualitative Data Analysis)

Strategy: To Foresee

To foresee in QDA is to reflect beforehand on what forms of data you will most likely need and collect, which thus informs what types of data analytic strategies you anticipate using.

Analysis, in a way, begins even before you collect data. As you design your research study in your mind and on a word processor page, one strategy is to consider what types of data you may need to help inform and answer your central and related research questions. Interview transcripts, participant observation field notes, documents, artifacts, photographs, video recordings, and so on are not only forms of data but foundations for how you may plan to analyze them. A participant interview, for example, suggests that you will transcribe all or relevant portions of the recording, and use both the transcription and the recording itself as sources for data analysis. Any analytic memos (discussed later) or journal entries you make about your impressions of the interview also become data to analyze. Even the computing software you plan to employ will be relevant to data analysis as it may help or hinder your efforts.

As your research design formulates, compose one to two paragraphs that outline how your QDA may proceed. This will necessitate that you have some background knowledge of the vast array of methods available to you. Thus surveying the literature is vital preparatory work.

QDA Strategy: To Survey

To survey in QDA is to look for and consider the applicability of the QDA literature in your field that may provide useful guidance for your forthcoming data analytic work.

General sources in QDA will provide a good starting point for acquainting you with the data analytic strategies available for the variety of genres in qualitative inquiry (e.g., ethnography, phenomenology, case study, arts-based research, mixed methods). One of the most accessible is Graham R. Gibbs' (2007) *Analysing Qualitative Data*, and one of the most richly detailed is Frederick J. Wertz et al.'s (2011) *Five Ways of Doing Qualitative Analysis*. The author's core texts for this article came

from *The Coding Manual for Qualitative Researchers* (Saldaña, 2009, 2013) and *Fundamentals of Qualitative Research* (Saldaña, 2011).

If your study's methodology or approach is grounded theory, for example, then a survey of methods works by such authors as Barney G. Glaser, Anselm L. Strauss, Juliet Corbin and, in particular, the prolific Kathy Charmaz (2006) may be expected. But there has been a recent outpouring of additional book publications in grounded theory by Birks & Mills (2011), Bryant & Charmaz (2007), Stern & Porr (2011), plus the legacy of thousands of articles and chapters across many disciplines that have addressed grounded theory in their studies.

Particular fields such as education, psychology, social work, health care, and others also have their own QDA methods literature in the form of texts and journals, plus international conferences and workshops for members of the profession. Most important is to have had some university coursework and/or mentorship in qualitative research to suitably prepare you for the intricacies of QDA. Also acknowledge that the emergent nature of qualitative inquiry may require you to adopt different analytic strategies from what you originally planned.

QDA Strategy: To Collect

To collect in QDA is to receive the data given to you by participants and those data you actively gather to inform your study.

QDA is concurrent with data collection and management. As interviews are transcribed, field notes are fleshed out, and documents are filed, the researcher uses the opportunity to carefully read the corpus and make preliminary notations directly on the data documents by highlighting, bolding, italicizing, or noting in some way any particularly interesting or salient portions. As these data are initially reviewed, the researcher also composes supplemental analytic memos that include first impressions, reminders for follow-up, preliminary connections, and other thinking matters about the phenomena at work.

Some of the most common fieldwork tools you might use to collect data are notepads, pens and pencils, file folders for documents, a laptop or desktop with word processing software (Microsoft Word and Excel are most useful) and internet access, a digital camera, and a voice recorder. Some fieldworkers may even employ a digital video camera to record social action, as long as participant permissions have been secured. But everything originates from the researcher himself or herself. Your senses are immersed in the cultural milieu you study, taking

in and holding on to relevant details or “significant trivia,” as I call them. You become a human camera, zooming out to capture the broad landscape of your field site one day, then zooming in on a particularly interesting individual or phenomenon the next. Your analysis is only as good as the data you collect.

Fieldwork can be an overwhelming experience because so many details of social life are happening in front of you. Take a holistic approach to your entree, but as you become more familiar with the setting and participants, actively focus on things that relate to your research topic and questions. Of course, keep yourself open to the intriguing, surprising, and disturbing (Sunstein & Chiseri-Strater, 2012, p. 115), for these facets enrich your study by making you aware of the unexpected.

QDA Strategy: To Feel

To feel in QDA is to gain deep emotional insight into the social worlds you study and what it means to be human.

Virtually everything we do has an accompanying emotion(s), and feelings are both reactions and stimuli for action. Others’ emotions clue you to their motives, attitudes, values, beliefs, worldviews, identities, and other subjective perceptions and interpretations. Acknowledge that emotional detachment is not possible in field research. Attunement to the emotional experiences of your participants plus sympathetic and empathetic responses to the actions around you are necessary in qualitative endeavors. Your own emotional responses during fieldwork are also data because they document the tacit and visceral. It is important during such analytic reflection to assess why your emotional reactions were as they were. But it is equally important not to let emotions alone steer the course of your study. A proper balance must be found between feelings and facts.

QDA Strategy: To Organize

To organize in QDA is to maintain an orderly repository of data for easy access and analysis.

Even in the smallest of qualitative studies, a large amount of data will be collected across time. Prepare both a hard drive and hard copy folders for digital data and paperwork, and back up all materials for security from loss. I recommend that each data “chunk” (e.g., one interview transcript, one document, one day’s worth of field notes) get its own file, with subfolders specifying the data forms and research study logistics (e.g., interviews, field notes, documents, Institutional Review Board correspondence, calendar).

For small-scale qualitative studies, I have found it quite useful to maintain one large master file with all participant and field site data copied and combined with the literature review and accompanying researcher analytic memos. This master file is used to cut and paste related passages together, deleting what seems unnecessary as the study proceeds, and eventually transforming the document into the final report itself. Cosmetic devices such as font style, font size, rich text (italicizing, bolding, underlining, etc.), and color can help you distinguish between different data forms and highlight significant passages. For example, descriptive, narrative passages of field notes are logged in regular font. **“Quotations, things spoken by participants, are logged in bold font.”** *Observer’s comments, such as the researcher’s subjective impressions or analytic jottings, are set in italics.*

QDA Strategy: To Jot

To jot in QDA is to write occasional, brief notes about your thinking or reminders for follow up.

A jot is a phrase or brief sentence that will literally fit on a standard size “sticky note.” As data are brought and documented together, take some initial time to review their contents and to jot some notes about preliminary patterns, participant quotes that seem quite vivid, anomalies in the data, and so forth.

As you work on a project, keep something to write with or to voice record with you at all times to capture your fleeting thoughts. You will most likely find yourself thinking about your research when you’re not working exclusively on the project, and a “mental jot” may occur to you as you ruminate on logistical or analytic matters. Get the thought documented in some way for later retrieval and elaboration as an analytic memo.

QDA Strategy: To Prioritize

To prioritize in QDA is to determine which data are most significant in your corpus and which tasks are most necessary.

During fieldwork, massive amounts of data in various forms may be collected, and your mind can get easily overwhelmed from the magnitude of the quantity, its richness, and its management. Decisions will need to be made about the most pertinent of them because they help answer your research questions or emerge as salient pieces of evidence. As a sweeping generalization, approximately one half to two thirds of what you collect may become unnecessary as you proceed toward the more formal stages of QDA.

To prioritize in QDA is to also determine what matters most in your assembly of codes, categories,

themes, assertions, and concepts. Return back to your research purpose and questions to keep you framed for what the focus should be.

QDA Strategy: To Analyze

To analyze in QDA is to observe and discern patterns within data and to construct meanings that seem to capture their essences and essentials.

Just as there are a variety of genres, elements, and styles of qualitative research, so too are there a variety of methods available for QDA. Analytic choices are most often based on what methods will harmonize with your genre selection and conceptual framework, what will generate the most sufficient answers to your research questions, and what will best represent and present the project's findings.

Analysis can range from the factual to the conceptual to the interpretive. Analysis can also range from a straightforward descriptive account to an emergently constructed grounded theory to an evocatively composed short story. A qualitative research project's outcomes may range from rigorously achieved, insightful answers to open-ended, evocative questions; from rich descriptive detail to a bullet-pointed list of themes; and from third-person, objective reportage to first-person, emotion-laden poetry. Just as there are multiple destinations in qualitative research, there are multiple pathways and journeys along the way.

Analysis is accelerated as you take cognitive ownership of your data. By reading and rereading the corpus, you gain intimate familiarity with its contents and begin to notice significant details as well as make new insights about their meanings. Patterns, categories, and their interrelationships become more evident the more you know the subtleties of the database.

Since qualitative research's design, fieldwork, and data collection are most often provisional, emergent, and evolutionary processes, you reflect on and analyze the data *as* you gather them and proceed through the project. If preplanned methods are not working, you change them to secure the data you need. There is generally a post-fieldwork period when continued reflection and more systematic data analysis occur, concurrent with or followed by additional data collection, if needed, and the more formal write-up of the study, which is in itself an analytic act. Through field note writing, interview transcribing, analytic memo writing, and other documentation processes, you gain cognitive ownership of your data; and the intuitive, tacit, synthesizing capabilities of your brain begin sensing patterns, making connections, and seeing the bigger picture. The purpose and outcome of data analysis

is to reveal to others through fresh insights what we have observed and discovered about the human condition. And fortunately, there are heuristics for reorganizing and reflecting on your qualitative data to help you achieve that goal.

QDA Strategy: To Pattern

To pattern in QDA is to detect similarities within and regularities among the data you have collected.

The natural world is filled with patterns because we, as humans, have constructed them as such. Stars in the night sky are not just a random assembly; our ancestors pieced them together to form constellations like the Big Dipper. A collection of flowers growing wild in a field has a pattern, as does an individual flower's patterns of leaves and petals. Look at the physical objects humans have created and notice how pattern oriented we are in our construction, organization, and decoration. Look around you in your environment and notice how many patterns are evident on your clothing, in a room, and on most objects themselves. Even our sometimes mundane daily and long-term human actions are reproduced patterns in the form of roles, relationships, rules, routines, and rituals.

This human propensity for pattern making follows us into QDA. From the vast array of interview transcripts, field notes, documents, and other forms of data, there is this instinctive, hardwired need to bring order to the collection—not just to reorganize it but to look for and construct patterns out of it. The discernment of patterns is one of the first steps in the data analytic process, and the methods described next are recommended ways to construct them.

QDA Strategy: To Code

To code in QDA is to assign a truncated, symbolic meaning to each datum for purposes of qualitative analysis.

Coding is a heuristic—a method of discovery—to the meanings of individual sections of data. These codes function as a way of patterning, classifying, and later reorganizing them into emergent categories for further analysis. Different types of codes exist for different types of research genres and qualitative data analytic approaches, but this article will focus on only a few selected methods. First, a definition of a code:

A code in qualitative data analysis is most often a word or short phrase that symbolically assigns a summative, salient, essence-capturing, and/or evocative attribute for a portion of language-based or visual data. The data can consist of interview transcripts, participant observation fieldnotes, journals, documents, literature, artifacts, photographs,

video, websites, e-mail correspondence, and so on. The portion of data to be coded can... range in magnitude from a single word to a full sentence to an entire page of text to a stream of moving images... Just as a title represents and captures a book or film or poem's primary content and essence, so does a code represent and capture a datum's primary content and essence. [Saldaña, 2009, p. 3]

One helpful pre-coding task is to divide long selections of field note or interview transcript data into shorter *stanzas*. Stanza division "chunks" the corpus into more manageable paragraph-like units for coding assignments and analysis. The transcript sample that follows illustrates one possible way of inserting line breaks in-between self-standing passages of interview text for easier readability.

Process Coding

As a first coding example, the following interview excerpt about an employed, single, lower-middle-class

P: ¹ When I go to the grocery store, I can't believe how much the price of meat has skyrocketed. The other day I was at the meat section, and there was a horde of people clustered around the chicken. It was because the store was offering two-for-one. Buy one package of chicken, get the second one free. Now *that* was a bargain. And I got some.

I: What other consumer habits of yours have been changed by the economy?

P: Sometimes it's the little things.² Like, at work, do I really want to pay \$1.50 for one bottle of Diet Coke from a vending machine? I can practically get a two-liter bottle for that same price at the grocery store. So I³ think twice before I put my dollar and coins in a machine.

⁴ I've been going to all-you-can-eat places a lot lately, because it's both cheap and filling. I go to Peter Piper's or Sweet Tomatoes or Golden Corral or some cheap Chinese buffet and I stock up on lunch so I can skip dinner. Or I skip lunch so I can stock up on dinner. With Sweet Tomatoes I get those coupons for a few bucks off for lunch, so that really helps.

I: What about purchases of non-food items? How have your spending habits changed these days?

P: ⁵ I still have my bad habits I refuse to give up: books and cigarettes, it's always gonna be that.⁶ I look at clothes some, but when I see

adult male's spending habits during the difficult economic times in the U.S. during 2008–2012 is coded in the right-hand margin in capital letters. The superscript numbers match the datum unit with its corresponding code. This particular method is called process coding, which uses gerunds ("ing" words) exclusively to represent action suggested by the data. Processes can consist of observable human actions (e.g., BUYING BARGAINS), mental processes (e.g., THINKING TWICE), and more conceptual ideas (e.g., APPRECIATING WHAT YOU'VE GOT). Notice that the interviewer's (I) portions are not coded, just the participant's (P). A code is applied each time the subtopic of the interview shifts—even within a stanza—and the same codes can (and should) be used more than once if the subtopics are similar. The central research question driving this qualitative study is, "In what ways are middle-class Americans influenced and affected by the current [2008–2012] economic recession?"

¹ BUYING BARGAINS

² QUESTIONING A PURCHASE

³ THINKING TWICE

⁴ STOCKING UP

⁵ REFUSING SACRIFICE

⁶ THINKING TWICE

the prices I think I don't really need them, what I've got is fine.⁷ I've got my cats to take care of, so they get priority with special foods, meds, vets.

⁸ I don't go to movies anymore. I rent DVDs from Netflix or Redbox or watch movies online—so much cheaper than paying over ten or twelve bucks for a movie ticket.

⁹ In a way, I've always lived kind of cheap. I'm not a big spender, really, so I haven't changed my habits all that much, but¹⁰ I do notice I'm not putting as much into savings as I used to, so that's a sign that I'm spending more because the price of stuff has gone up.¹¹ I heard that peanut butter's gonna go up because of some bad crop, so that's another ding in my wallet.

I: You said you have cats to take care of.

P: Yeah, three of them.

I: What about their expenses?

P: ¹² Man, they are *so* high maintenance. All three are on some of type of meds of one kind or another. One's diabetic so he has to have insulin shots twice a day, another's got some kind of thyroid condition so he gets ear gel twice a day, and the third one gets his ear gel for urinary infections on an as-needed basis. Two of them need special food, there's lots of trips to the vet's for check-ups.

¹³ I just had to have dental work recently, almost \$1,000 to fix up my teeth because I hadn't been taking care of them as good as I should have. And that was just round one, there's two more procedures I have to go through, and that'll be another couple of thousand.¹⁴ And my dental insurance is just worthless on this so I have to pick up the tab myself.

I: Sounds like it's just one thing after another.

P: Yeah, yeah, and it all adds up.¹⁵ I'm surprised I've made it this far. I'm not as bad off as others are, so I thank God for that. But, man—scary times.

Different researchers analyzing this same piece of data may develop completely different codes, depending on their lenses and filters. The previous codes are only one person's interpretation of what is happening in the data, not the definitive list. The process codes have transformed the raw data units

⁷ PRIORITIZING

⁸ FINDING ALTERNATIVES

⁹ LIVING CHEAPLY

¹⁰ NOTICING CHANGES

¹¹ STAYING INFORMED

¹² MAINTAINING HEALTH

¹³ MAINTAINING HEALTH

¹⁴ PICKING UP THE TAB

¹⁵ APPRECIATING WHAT YOU'VE GOT

into new representations for analysis. A listing of them applied to this interview transcript, in the order they appear, reads:

BUYING BARGAINS
QUESTIONING A PURCHASE

THINKING TWICE
 STOCKING UP
 REFUSING SACRIFICE
 THINKING TWICE
 PRIORITIZING
 FINDING ALTERNATIVES
 LIVING CHEAPLY
 NOTICING CHANGES
 STAYING INFORMED
 MAINTAINING HEALTH
 MAINTAINING HEALTH
 PICKING UP THE TAB
 APPRECIATING WHAT YOU'VE GOT

Coding the data is the first step in this particular approach to QDA, and categorization is just one of the next possible steps.

QDA Strategy: To Categorize

To categorize in QDA is to cluster similar or comparable codes into groups for pattern construction and further analysis.

Humans categorize things in innumerable ways. Think of an average apartment or house's layout. The rooms of a dwelling have been constructed or categorized by their builders and occupants according to function. A kitchen is designated as an area to store and prepare food and the cooking and dining materials such as pots, pans, and utensils. A bedroom is designated for sleeping, a closet for clothing storage, a bathroom for bodily functions and hygiene, and so on. Each room is like a *category* in which related and relevant *patterns of human action* occur. Of course, there are exceptions now and then, such as eating breakfast in bed rather than in a dining area or living in a small studio apartment in which most possessions are contained within one large room (but nonetheless are most often organized and clustered into sub-categories according to function and optimal use of space).

The point here is that the patterns of social action we designate into particular categories during QDA are not perfectly bounded. Category construction is our best attempt to cluster the most seemingly alike things into the most seemingly appropriate groups. Categorizing is reorganizing and reordering the vast array of data from a study because it is from these smaller, larger, and meaning-rich units that we can better grasp the particular features of each one and the categories' possible interrelationships with one another.

One analytic strategy with a list of codes is to classify them into similar clusters. Obviously, the

same codes share the same category, but it is also possible that a single code can merit its own group if you feel it is unique enough. After the codes have been classified, a category label is applied to each grouping. Sometimes a code can also double as a category name if you feel it best summarizes the totality of the cluster. Like coding, categorizing is an interpretive act, for there can be different ways of separating and collecting codes that seem to belong together. The cut-and-paste functions of a word processor are most useful for exploring which codes share something in common.

Below is my categorization of the fifteen codes generated from the interview transcript presented earlier. Like the gerunds for process codes, the categories have also been labeled as “-ing” words to connote action. And there was no particular reason why fifteen codes resulted in three categories—there could have been less or even more, but this is how the array came together after my reflections on which codes seemed to belong together. The category labels are ways of answering “why” they belong together. For at-a-glance differentiation, I place codes in CAPITAL LETTERS and categories in upper and lower case **Bold Font**:

Category 1: Thinking Strategically

CODES:
 STAYING INFORMED
 NOTICING CHANGES
 QUESTIONING A PURCHASE
 THINKING TWICE
 THINKING TWICE

Category 2: Spending Strategically

CODES:
 PICKING UP THE TAB
 BUYING BARGAINS
 STOCKING UP

Category 3: Living Strategically

CODES:
 MAINTAINING HEALTH
 MAINTAINING HEALTH
 REFUSING SACRIFICE
 PRIORITIZING
 FINDING ALTERNATIVES
 LIVING CHEAPLY
 APPRECIATING WHAT YOU'VE GOT

Notice that the three category labels share a common word: “strategically.” Where did this word come from? It came from analytic reflection

on the original data, the codes, and the process of categorizing the codes and generating their category labels. It was the analyst's choice based on the interpretation of what primary action was happening. Your categories generated from your coded data do not need to share a common word or phrase, but I find that this technique, when appropriate, helps build a sense of unity to the initial analytic scheme.

The three categories—**Thinking Strategically**, **Spending Strategically**, and **Living Strategically**—are then reflected upon for how they might interact and interplay. This is where the next major facet of data analysis, analytic memos, enters the scheme. But a necessary section on the basic principles of interrelationship and analytic reasoning must precede that discussion.

QDA Strategy: To Interrelate

To interrelate in QDA is to propose connections within, between, and among the constituent elements of analyzed data.

One task of QDA is to explore the ways our patterns and categories interact and interplay. I use these terms to suggest the qualitative equivalent of statistical correlation, but interaction and interplay are much more than a simple relationship. They imply *interrelationship*. Interaction refers to reverberative connections—for example, how one or more categories might influence and affect the others, how categories operate concurrently, or whether there is some kind of “domino” effect to them. Interplay refers to the structural and processual nature of categories—for example, whether some type of sequential order, hierarchy, or taxonomy exists; whether any overlaps occur; whether there is superordinate and subordinate arrangement; and what types of organizational frameworks or networks might exist among them. The positivist construct of “cause and effect” becomes *influences and affects* in QDA.

There can even be patterns of patterns and categories of categories if your mind thinks conceptually and abstractly enough. Our minds can intricately connect multiple phenomena but only if the data and their analyses support the constructions. We can speculate about interaction and interplay all we want, but it is only through a more systematic investigation of the data—in other words, good thinking—that we can plausibly establish any possible interrelationships.

QDA Strategy: To Reason

To reason in QDA is to think in ways that lead to causal probabilities, summative findings, and evaluative conclusions.

Unlike quantitative research, with its statistical formulas and established hypothesis-testing protocols, qualitative research has no standardized methods of data analysis. Rest assured, there are recommended guidelines from the field's scholars and a legacy of analytic strategies from which to draw. But the primary heuristics (or methods of discovery) you apply during a study are *deductive*, *inductive*, *abductive*, and *retroductive* reasoning. Deduction is what we generally draw and conclude from established facts and evidence. Induction is what we experientially explore and infer to be transferable from the particular to the general, based on an examination of the evidence and an accumulation of knowledge. Abduction is surmising from the evidence that which is most likely, those explanatory hunches based on clues. “Whereas deductive inferences are certain (so long as their premises are true) and inductive inferences are probable, abductive inferences are merely plausible” (Shank, 2008, p. 1). Retroduction is historic reconstruction, working backwards to figure out how the current conditions came to exist.

It is not always necessary to know the names of these four ways of reasoning as you proceed through analysis. In fact, you will more than likely reverberate quickly from one to another depending on the task at hand. But what is important to remember about reasoning is:

- to base your conclusions primarily on the participants' experiences, not just your own
- not to take the obvious for granted, as sometimes the expected won't always happen. Your hunches can be quite right and, at other times, quite wrong
- to examine the evidence carefully and make reasonable inferences
- to logically yet imaginatively think about what is going on and how it all comes together.

Futurists and inventors propose three questions when they think about creating new visions for the world: What is possible (induction)? What is plausible (abduction)? What is preferable (deduction)? These same three questions might be posed as you proceed through QDA and particularly through analytic memo writing, which is retroductive reflection on your analytic work thus far.

QDA Strategy: To Memo

To *memo* in QDA is to reflect in writing on the nuances, inferences, meanings, and transfer of coded and categorized data plus your analytic processes.

Like field note writing, perspectives vary among practitioners as to the methods for documenting the researcher's analytic insights and subjective experiences. Some advise that such reflections should be included in field notes as relevant to the data. Others advise that a separate researcher's journal should be maintained for recording these impressions. And still others advise that these thoughts be documented as separate analytic memos. I prescribe the latter as a method because it is generated by and directly connected to the data themselves.

An analytic memo is a "think piece" of reflexive free writing, a narrative that sets in words your interpretations of the data. Coding and categorizing are heuristics to detect some of the possible patterns and interrelationships at work within the corpus, and an analytic memo further articulates your deductive, inductive, abductive, and retroductive thinking processes on what things may mean. Though the metaphor is a bit flawed and limiting, think of codes and their consequent categories as separate jigsaw puzzle pieces, and their integration into an analytic memo as the trial assembly of the complete picture.

What follows is an example of an analytic memo based on the earlier process coded and categorized interview transcript. It is not intended as the final write-up for a publication but as an open-ended reflection on the phenomena and processes suggested by the data and their analysis thus far. As the study proceeds, however, initial and substantive analytic memos can be revisited and revised for eventual integration into the final report. Note how the memo is dated and given a title for future and further categorization, how participant quotes are occasionally included for evidentiary support, and how the category names are bolded and the codes kept in capital letters to show how they integrate or weave into the thinking:

March 18, 2012

EMERGENT CATEGORIES: A STRATEGIC AMALGAM

There's a popular saying now: "Smart is the new rich." This participant is **Thinking Strategically** about his spending through such tactics as THINKING TWICE and QUESTIONING A PURCHASE before he decides to invest in a product. There's a heightened awareness of both immediate trends and forthcoming economic bad news that positively

affects his **Spending Strategically**. However, he seems unaware that there are even more ways of LIVING CHEAPLY by FINDING ALTERNATIVES.

He dines at all-you-can-eat restaurants as a way of STOCKING UP on meals, but doesn't state that he could bring lunch from home to work, possibly saving even more money. One of his "bad habits" is cigarettes, which he refuses to give up; but he doesn't seem to realize that by quitting smoking he could save even more money, not to mention possible health care costs. He balks at the idea of paying \$1.50 for a soft drink, but doesn't mind paying \$6.00–\$7.00 for a pack of cigarettes. Penny-wise and pound-foolish. Addictions skew priorities.

Living Strategically, for this participant during "scary times," appears to be a combination of PRIORITIZING those things which cannot be helped, such as pet care and personal dental care; REFUSING SACRIFICE for maintaining personal creature-comforts; and FINDING ALTERNATIVES to high costs and excessive spending. **Living Strategically** is an amalgam of thinking and action-oriented strategies.

There are several recommended topics for analytic memo writing throughout the qualitative study. Memos are opportunities to reflect on and write about:

- how you personally relate to the participants and/or the phenomenon
- your study's research questions
- your code choices and their operational definitions
 - the emergent patterns, categories, themes, assertions, and concepts
 - the possible networks (links, connections, overlaps, flows) among the codes, patterns, categories, themes, assertions, and concepts
 - an emergent or related existent theory
 - any problems with the study
 - any personal or ethical dilemmas with the study
 - future directions for the study
 - the analytic memos generated thus far [labeled "metamemos"]
 - the final report for the study [adapted from Saldaña, 2013, p. 49]

Since writing *is* analysis, analytic memos expand on the inferential meanings of the truncated codes and categories as a transitional stage into a more coherent narrative with hopefully rich social insight.

QDA Strategy: To Code—A Different Way

The first example of coding illustrated process coding, a way of exploring general social action among humans. But sometimes a researcher works with an individual case study whose language is unique, or with someone the researcher wishes to honor by maintaining the authenticity of his or her speech in the analysis. These reasons suggest that a more participant-centered form of coding may be more appropriate.

In Vivo Coding

A second frequently applied method of coding is called *in vivo* coding. The root meaning of “*in vivo*” is “in that which is alive” and refers to a code based on the actual language used by the participant

(Strauss, 1987). What words or phrases in the data record you select as codes are those that seem to stand out as significant or summative of what is being said.

Using the same transcript of the male participant living in difficult economic times, *in vivo* codes are listed in the right-hand column. I recommend that *in vivo* codes be placed in quotation marks as a way of designating that the code is extracted directly from the data record. Note that instead of fifteen codes generated from process coding, the total number of *in vivo* codes is thirty. This is not to suggest that there should be specific numbers or ranges of codes used for particular methods. *In vivo* codes, though, tend to be applied more frequently to data. Again, the interviewer’s questions and prompts are not coded, just the participant’s responses:

P: When I go to the grocery store, I can’t believe how much the price of meat has¹ skyrocketed. The other day I was at the meat section, and there was a² horde of people clustered around the chicken. It was because the store was offering³ two-for-one. Buy one package of chicken, get the second one⁴ free. Now *that* was a⁵ bargain. And I got some.

I: What other consumer habits of yours have been changed by the economy?

P: Sometimes it’s⁶ the little things. Like, at work, do I really want to pay \$1.50 for one bottle of Diet Coke from a vending machine? I can practically get a two-liter bottle for that same price at the grocery store. So I⁷ think twice before I put my dollar and coins in a machine.

I’ve been going to⁸ all-you-can-eat places a lot lately, because it’s both⁹ cheap and filling. I go to Peter Piper’s or Sweet Tomatoes or Golden Corral or some¹⁰ cheap Chinese buffet and I¹¹ stock up on lunch so I can skip dinner. Or I¹² skip lunch so I can stock up on dinner. With Sweet Tomatoes I get those¹³ coupons for a few bucks off for lunch, so that really helps.

I: What about purchases of non-food items? How have your spending habits changed these days?

P: I still have my¹⁴ bad habits I refuse to give up: books and cigarettes, it’s always gonna be that. I look at clothes some, but when I see the prices I think I¹⁵ don’t really need them, what I’ve got is fine. I’ve got my cats to

¹ “SKYROCKETED”

² “HORDE”

³ “TWO-FOR-ONE”

⁴ “FREE”

⁵ “BARGAIN”

⁶ “THE LITTLE THINGS”

⁷ “THINK TWICE”

⁸ “ALL-YOU-CAN-EAT”

⁹ “CHEAP AND FILLING”

¹⁰ “CHEAP”

¹¹ “STOCK UP”

¹² “SKIP”

¹³ “COUPONS”

¹⁴ “BAD HABITS”

¹⁵ “DON’T REALLY NEED”

take care of, so they get¹⁶ priority with special foods, meds, vets.

I don't go to movies anymore. I rent DVDs from Netflix or Redbox or watch movies online—so much¹⁷ cheaper than paying over ten or twelve bucks for a movie ticket.

In a way, I've always¹⁸ lived kind of cheap. I'm¹⁹ not a big spender, really, so I²⁰ haven't changed my habits all that much, but I do notice I'm²¹ not putting as much into savings as I used to, so that's a sign that I'm²² spending more because the price of stuff has gone up. I heard that peanut butter's gonna go up because of some bad crop so that's²³ another ding in my wallet.

I: You said you have cats to take care of.

P: Yeah, three of them.

I: What about their expenses?

P: Man, they are *so*²⁴ high maintenance. All three are on some of type of meds of one kind or another. One's diabetic so he has to have insulin shots twice a day, another's got some kind of thyroid condition so he gets ear gel twice a day, and the third one gets his ear gel for urinary infections on an as-needed basis. Two of them need special food, there's lots of trips to the vet's for check-ups.

I just had to have dental work recently, almost \$1,000 to fix up my teeth because I hadn't been taking care of them as good as I should have. And that was just round one, there's two more procedures I have to go through, and that'll be another²⁵ couple of thousand. And my dental²⁶ insurance is just worthless on this so I have to²⁷ pick up the tab myself.

I: Sounds like it's just one thing after another.

P: Yeah, yeah, and²⁸ it all adds up. I'm surprised I've made it this far. I'm²⁹ not as bad off as others are, so I thank God for that. But, man—³⁰ scary times.

The thirty in vivo codes are then extracted from the transcript and listed in the order they appear to prepare them for analytic action and reflection:

“SKYROCKETED”

“HORDE”

¹⁶ “PRIORITY”

¹⁷ “CHEAPER”

¹⁸ “LIVED KIND OF
CHEAP”

¹⁹ “NOT A BIG SPENDER”

²⁰ “HAVEN'T CHANGED
MY HABITS”

²¹ “NOT PUTTING AS
MUCH INTO
SAVINGS”

²² “SPENDING MORE”

²³ “ANOTHER DING IN
MY WALLET”

²⁴ “HIGH MAINTENANCE”

²⁵ “COUPLE OF
THOUSAND”

²⁶ “INSURANCE IS JUST
WORTHLESS”

²⁷ “PICK UP THE TAB”

²⁸ “IT ALL ADDS UP”

²⁹ “NOT AS BAD OFF”

³⁰ “SCARY TIMES”

“TWO-FOR-ONE”

“FREE”

“BARGAIN”

“THE LITTLE THINGS”

“THINK TWICE”

“ALL-YOU-CAN-EAT”
 “CHEAP AND FILLING”
 “CHEAP”
 “STOCK UP”
 “SKIP”
 “COUPONS”
 “BAD HABITS”
 “DON’T REALLY NEED”
 “PRIORITY”
 “CHEAPER”
 “LIVED KIND OF CHEAP”
 “NOT A BIG SPENDER”
 “HAVEN’T CHANGED MY HABITS”
 “NOT PUTTING AS MUCH INTO SAVINGS”
 “SPENDING MORE”
 “ANOTHER DING IN MY WALLET”
 “HIGH MAINTENANCE”
 “COUPLE OF THOUSAND”
 “INSURANCE IS JUST WORTHLESS”
 “PICK UP THE TAB”
 “IT ALL ADDS UP”
 “NOT AS BAD OFF”
 “SCARY TIMES”

Even though no systematic reorganization or categorization has been conducted with the codes thus far, an analytic memo of first impressions can still be composed:

March 19, 2012
 CODE CHOICES: THE EVERYDAY
 LANGUAGE OF ECONOMICS

After eyeballing the *in vivo* codes list, I noticed that variants of “CHEAP” appear most often. I recall a running joke between me and a friend of mine when we were shopping for sales. We’d say, “We’re not ‘cheap,’ we’re *frugal*.” There’s no formal economic or business language in this transcript—no terms such as “recession” or “downsizing”—just the everyday language of one person trying to cope during “SCARY TIMES” with “ANOTHER DING IN MY WALLET.”

The participant notes that he’s always “LIVED KIND OF CHEAP” and is “NOT A BIG SPENDER” and, due to his employment, “NOT AS BAD OFF” as others in the country. Yet even with his middle class status, he’s still feeling the monetary pinch, dining at inexpensive “ALL-YOU-CAN-EAT” restaurants and worried about the rising price of peanut butter, observing that he’s “NOT PUTTING AS MUCH INTO SAVINGS” as he used to.

Of all the codes, “ANOTHER DING IN MY WALLET” stands out to me, particularly because on

the audio recording he sounded bitter and frustrated. It seems that he’s so concerned about “THE LITTLE THINGS” because of high veterinary and dental charges. The only way to cope with a “COUPLE OF THOUSAND” dollars worth of medical expenses is to find ways of trimming the excess in everyday facets of living: “IT ALL ADDS UP.”

Like process coding, *in vivo* codes could be clustered into similar categories, but another simple data analytic strategy is also possible.

QDA Strategy: To Outline

To *outline* in QDA is to hierarchically, processually, and/or temporally assemble such things as codes, categories, themes, assertions, and concepts into a coherent, text-based display.

Traditional outlining formats and content provide not only templates for writing a report but templates for analytic organization. This principle can be found in several CAQDAS (Computer Assisted Qualitative Data Analysis Software) programs through their use of such functions as “hierarchies,” “trees,” and “nodes,” for example. Basic outlining is simply a way of arranging primary, secondary, and sub-secondary items into a patterned display. For example, an organized listing of things in a home might consist of:

- I. Kitchen
 - A. Large appliances
 1. Refrigerator
 2. Stove-top oven
 3. Microwave oven
 - B. Small appliances
 1. Toaster
 2. Coffee maker
 3. Can opener
- II. Dining room
 - A. Furniture
 1. Table
 2. Chairs
- III. Etc.

In QDA, outlining may include descriptive nouns or topics but, depending on the study, it may also involve processes or phenomena in extended passages, such as *in vivo* codes or themes.

The complexity of what we learn in the field can be overwhelming, and outlining is a way of organizing and ordering that complexity so that it does not become complicated. The cut-and-paste and tab functions of a word processor page enable you to arrange and rearrange the salient items from your preliminary

coded analytic work into a more streamlined flow. By no means do I suggest that the intricate messiness of life can always be organized into neatly formatted arrangements, but outlining is an analytic act that stimulates deep reflection on both the interconnect-edness and interrelationships of what we study. As an example, here are the thirty in vivo codes generated from the initial transcript analysis, arranged in such a way as to construct five major categories:

- I. "SCARY TIMES"
 - A. "SKYROCKETED"
 - B. "HORDE"
- II. "PRIORITY"
 - A. "HIGH MAINTENANCE"
 - B. "THINK TWICE"
 1. "DON'T REALLY NEED"
 2. "SKIP"
- III. "ANOTHER DING IN MY WALLET"
 - A. "PICK UP THE TAB"
 1. "INSURANCE IS JUST WORTHLESS"
 - B. "SPENDING MORE"
 1. "COUPLE OF THOUSAND"
 2. "NOT PUTTING AS MUCH INTO SAVINGS"
 - C. "IT ALL ADDS UP"
- IV. "THE LITTLE THINGS"
 - A. "BARGAIN"
 1. "COUPONS"
 2. "FREE"
 - B. "STOCK UP"
 1. "TWO-FOR-ONE"
 2. "ALL-YOU-CAN-EAT"
- V. "LIVED KIND OF CHEAP"
 - A. "CHEAP"
 1. "CHEAPER"
 2. "CHEAP AND FILLING"
 - B. "HAVEN'T CHANGED MY HABITS"
 1. "BAD HABITS"
 2. "NOT A BIG SPENDER"
 - C. "NOT AS BAD OFF"

Now that the codes have been rearranged into an outline format, an analytic memo is composed to expand on the rationale and constructed meanings in progress:

March 19, 2012

NETWORKS: EMERGENT CATEGORIES

The five major categories I constructed from the in vivo codes are: "SCARY TIMES," "PRIORITY," "ANOTHER DING IN MY WALLET," "THE LITTLE THINGS," and "LIVED KIND OF CHEAP."

One of the things that hit me today was that the reason he may be pinching pennies on smaller purchases is that he cannot *control* the larger ones he has to deal with. Perhaps the only way we can cope with or seem to have some sense of agency over major expenses is to cut back on the smaller ones that we *can* control. \$1,000 for a dental bill? Skip lunch for a few days a week. Insulin medication to buy for a pet? Don't buy a soft drink from a vending machine. Using this reasoning, let me try to interrelate and weave the categories together as they relate to this particular participant:

During these scary economic times, he prioritizes his spending because there seems to be just one ding after another to his wallet. A general lifestyle of living cheaply and keeping an eye out for how to save money on the little things compensates for those major expenses beyond his control.

QDA Strategy: To Code—In Even More Ways

The process and in vivo coding examples thus far have demonstrated only two specific methods of thirty-two documented approaches (Saldaña, 2013). Which one(s) you choose for your analysis depends on such factors as your conceptual framework, the genre of qualitative research for your project, the types of data you collect, and so on. The following sections present a few other approaches available for coding qualitative data that you may find useful as starting points.

Descriptive Coding

Descriptive codes are primarily nouns that simply summarize the topic of a datum. This coding approach is particularly useful when you have different types of data gathered for one study, such as interview transcripts, field notes, documents, and visual materials such as photographs. Descriptive

codes not only help categorize but also index the data corpus' basic contents for further analytic work. An example of an interview portion coded

P: ¹ When I go to the grocery store, I can't believe how much the price of meat has skyrocketed.

² The other day I was at the meat section, and there was a horde of people clustered around the chicken. It was because the store was offering two-for-one. Buy one package of chicken, get the second one free. Now *that* was a bargain. And I got some.

I: What other consumer habits of yours have been changed by the economy?

P: ³ Sometimes it's the little things. Like, at work, do I really want to pay \$1.50 for one bottle of Diet Coke from a vending machine? I can practically get a two-liter bottle for that same price at the grocery store.⁴ So I think twice before I put my dollar and coins in a machine.

For initial analysis, descriptive codes are clustered into similar categories to detect such patterns as frequency (i.e., categories with the largest number of codes), interrelationship (i.e., categories that seem to connect in some way), and initial work for grounded theory development.

Values Coding

Values coding identifies the values, attitudes, and beliefs of a participant, as shared by the individual and/or interpreted by the analyst. This coding method infers the "heart and mind" of an individual or group's worldview as to what is important, perceived as true, maintained as opinion, and felt strongly. The three constructs are coded separately but are part of a complex interconnected system.

P: ¹ In a way, I've always lived kind of cheap.

² I'm not a big spender, really, so I haven't changed my habits all that much,³ but I do notice I'm not putting as much into savings as I used to,⁴ so that's a sign that I'm spending more because the price of stuff has gone up.⁵ I heard that peanut butter's gonna go up because of some bad crop,⁶ so that's another ding in my wallet.

For analysis, categorize the codes for each of the three different constructs together (i.e., all values in one group, attitudes in a second group, and beliefs in

descriptively, taken from the participant living in tough economic times, follows to illustrate how the same data can be coded in multiple ways:

¹ MEAT PRICES

² GROCERY STORE SALE

³ SOFT DRINK PRICES

⁴ CONSUMER DECISION
MAKING

Briefly, a value (V) is what we attribute as important, be it a person, thing, or idea. An attitude (A) is the evaluative way we think and feel about ourselves, others, things, or ideas. A belief (B) is what we think and feel as true or necessary, formed from our "personal knowledge, experiences, opinions, prejudices, morals, and other interpretive perceptions of the social world" (Saldaña, 2009, pp. 89–90). Values coding explores intrapersonal, interpersonal, and cultural constructs or *ethos*. It is an admittedly slippery task to code this way, for it is sometimes difficult to discern what is a value, attitude, or belief because they are intricately interrelated. But the depth you can potentially obtain is rich. An example of values coding follows:

¹ B: LIVING CHEAPLY

² V: FRUGAL

³ B: LESS SAVINGS

⁴ B: "SPENDING MORE"

⁵ B: RISING PRICES

⁶ A: ECONOMIC
BITTERNESS

a third group). Analytic memo writing about the patterns and possible interrelationships may reveal a more detailed and intricate worldview of the participant.

Dramaturgical Coding

Dramaturgical coding perceives life as performance and its participants as characters in a social drama. Codes are assigned to the data (i.e., a “play script”) that analyze the characters in action, reaction, and interaction. Dramaturgical coding of participants examines their *objectives* (OBJ) or wants, needs, and motives; the *conflicts* (CON) or obstacles

P: ¹ I’ve been going to all-you-can-eat places a lot lately, because it’s both cheap and filling. I go to Peter Piper’s or Sweet Tomatoes or Golden Corral or some cheap Chinese buffet and² stock up on lunch so I can skip dinner. Or I skip lunch so I can stock up on dinner.³ With Sweet Tomatoes I get those coupons for a few bucks off for lunch, so that really helps.

I: What about purchases of non-food items? How have your spending habits changed these days?

P: ⁴ I still have my bad habits⁵ I refuse to give up: books and cigarettes, it’s always gonna be that.⁶ I look at clothes some, but when I see the prices I think I don’t really need them, what I’ve got is fine.⁷ I’ve got my cats to take care of, so they get priority with special foods, meds, vets.

Not included in this particular interview excerpt are the emotions the participant may have experienced or talked about. His later line, “that’s another ding in my wallet,” would have been coded EMO: BITTER. A reader may not have inferred that specific emotion from seeing the line in print. But the interviewer, present during the event and listening carefully to the audio recording during transcription, noted that feeling in his tone of voice.

For analysis, group similar codes together (e.g., all objectives in one group, all conflicts in another group, all tactics in a third group), or string together chains of how participants deal with their circumstances to overcome their obstacles through tactics (e.g., OBJ: SAVING MEAL MONEY > TAC: SKIPPING MEALS). Explore how the individuals or groups manage problem solving in their daily lives. Dramaturgical coding is particularly useful as preliminary work for

they face as they try to achieve their objectives; the *tactics* (TAC) or strategies they employ to reach their objectives; their *attitudes* (ATT) toward others and their given circumstances; the particular *emotions* (EMO) they experience throughout; and their *subtexts* (SUB) or underlying and unspoken thoughts. The following is an example of dramaturgically coded data:

¹ OBJ: SAVING MEAL
MONEY

² TAC: SKIPPING MEALS

³ TAC: COUPONS

⁴ CON: “BAD HABITS”

⁵ SUB: RESISTANCE

⁶ ATT: SELF-
COMPROMISING

⁷ OBJ: PET CARE

narrative inquiry story development or arts-based research representations such as performance ethnography.

Versus Coding

Versus coding identifies the conflicts, struggles, and power issues observed in social action, reaction, and interaction as an X VS. Y code, such as: MEN VS. WOMEN, CONSERVATIVES VS. LIBERALS, FAITH VS. LOGIC, and so on. Conflicts are rarely this dichotomous. They are typically nuanced and much more complex. But humans tend to perceive these struggles with an US VS. THEM mindset. The codes can range from the observable to the conceptual and can be applied to data that show humans in tension with others, themselves, or ideologies.

What follows are examples of versus codes applied to the case study participant’s descriptions of his major medical expenses:

I: You said you have cats to take care of.

P: Yeah, three of them.

I: What about their expenses?

P: ¹ Man, they are *so* high maintenance. All three are on some of type of meds of one kind or another. One's diabetic so he has to have insulin shots twice a day, another's got some kind of thyroid condition so he gets ear gel twice a day, and the third one gets his ear gel for urinary infections on an as-needed basis. Two of them need special food, there's lots of trips to the vet's for check-ups.

² I just had to have dental work recently, almost \$1,000 to fix up my teeth because I hadn't been taking care of them as good as I should have. And that was just round one, there's two more procedures I have to go through, and that'll be another couple of thousand.³ And my dental insurance is just worthless on this so I have to pick up the tab myself.

¹ PET CARE COSTS VS.
HUMAN
LIVING EXPENSES

² HEALTH CARE COSTS
VS. PERSONAL
RESPONSIBILITY

³ INSURANCE COMPANY
VS. CONSUMER
COSTS

As an initial analytic tactic, group the versus codes into one of three categories: the *Stakeholders*, their *Perceptions and/or Actions*, and the *Issues* at stake. Examine how the three interrelate and identify the central ideological conflict at work as an **X vs. Y** category. Analytic memos and the final write-up can detail the nuances of the issues.

Remember that what has been profiled in this section is a broad brushstroke description of just a few basic coding processes, several of which can be compatibly “mixed and matched” within a single analysis (see Saldaña’s [2013] *The Coding Manual for Qualitative Researchers* for a complete discussion). Certainly with additional data, more in-depth analysis can occur, but coding is only one approach to extracting and constructing preliminary meanings from the data corpus. What now follows are additional methods for qualitative analysis.

QDA Strategy: To Theme

To theme in QDA is to construct summative, phenomenological meanings from data through extended passages of text.

Unlike codes, which are most often single words or short phrases that symbolically represent a datum,

themes are extended phrases or sentences that summarize the manifest (apparent) and latent (underlying) meanings of data (Auerbach & Silverstein, 2003; Boyatzis, 1998). Themes, intended to represent the essences and essentials of humans’ lived experiences, can also be categorized or listed in superordinate and subordinate outline formats as an analytic tactic.

Below is the interview transcript example used in the coding sections above. (Hopefully you are not too fatigued at this point with the transcript, but it’s important to know how inquiry with the same data set can be approached in several different ways.) During the investigation of the ways middle-class Americans are influenced and affected by the current (2008–2012) economic recession, the researcher noticed that participants’ stories exhibited facets of what he labeled “economic intelligence” or EI (based on the formerly developed theories of Howard Gardner’s multiple intelligences and Daniel Goleman’s emotional intelligence). Notice how themeing interprets what is happening through the use of two distinct phrases—ECONOMIC INTELLIGENCE IS (i.e., manifest or apparent meanings) and ECONOMIC INTELLIGENCE MEANS (i.e., latent or underlying meanings):

P: When I go to the grocery store, I can't believe how much the price of meat has skyrocketed.

¹ The other day I was at the meat section, and there was a horde of people clustered around the chicken. It was because the store was offering two-for-one. Buy one package of chicken, get the second one free. Now *that* was a bargain. And I got some.

I: What other consumer habits of yours have been changed by the economy?

P: ² Sometimes it's the little things. Like, at work, do I really want to pay \$1.50 for one bottle of Diet Coke from a vending machine? I can practically get a two-liter bottle for that same price at the grocery store. So I think twice before I put my dollar and coins in a machine.

³ I've been going to all-you-can-eat places a lot lately, because it's both cheap and filling. I go to Peter Piper's or Sweet Tomatoes or Golden Corral or some cheap Chinese buffet and ⁴ I stock up on lunch so I can skip dinner. Or I skip lunch so I can stock up on dinner. ⁵ With Sweet Tomatoes I get those coupons for a few bucks off for lunch, so that really helps.

I: What about purchases of non-food items? How have your spending habits changed these days?

P: ⁶ I still have my bad habits I refuse to give up: books and cigarettes, it's always gonna be that. ⁷ I look at clothes some, but when I see the prices I think I don't really need them, what I've got is fine. ⁸ I've got my cats to take care of, so they get priority with special foods, meds, vets.

⁹ I don't go to movies anymore. I rent DVDs from Netflix or Redbox or watch movies online—so much cheaper than paying over ten or twelve bucks for a movie ticket.

¹⁰ In a way, I've always lived kind of cheap. I'm not a big spender, really, so I haven't changed my habits all that much, but ¹¹ I do notice I'm not putting as much into savings as I used to, so that's a sign that I'm spending more because the price of stuff has gone up. I heard that peanut butter's gonna go up because of some bad crop, so that's another ding in my wallet.

I: You said you have cats to take care of.

P: Yeah, three of them.

¹ EI IS TAKING
ADVANTAGE OF
UNEXPECTED
OPPORTUNITY

² EI MEANS THINKING
BEFORE YOU ACT

³ EI IS BUYING CHEAP

⁴ EI MEANS SACRIFICE

⁵ EI IS SAVING A FEW
DOLLARS NOW
AND THEN

⁶ EI MEANS KNOWING
YOUR FLAWS

⁷ EI MEANS THINKING
BEFORE YOU ACT

⁸ EI IS SETTING
PRIORITIES

⁹ EI IS FINDING
CHEAPER FORMS
OF
ENTERTAINMENT

¹⁰ EI MEANS LIVING AN
INEXPENSIVE
LIFESTYLE

¹¹ EI IS NOTICING
PERSONAL AND
NATIONAL
ECONOMIC
TRENDS

I: What about their expenses?

P: ¹² Man, they are *so* high maintenance. All three are on some of type of meds of one kind or another. One's diabetic so he has to have insulin shots twice a day, another's got some kind of thyroid condition so he gets ear gel twice a day, and the third one gets his ear gel for urinary infections on an as-needed basis. Two of them need special food, there's lots of trips to the vet's for check-ups.

¹³ I just had to have dental work recently, almost \$1,000 to fix up my teeth because I hadn't been taking care of them as good as I should have. And that was just round one, there's two more procedures I have to go through, and that'll be another couple of thousand. And my dental insurance is just worthless on this so I have to pick up the tab myself.

I: Sounds like it's just one thing after another.

P: Yeah, yeah, and it all adds up. ¹⁴ I'm surprised I've made it this far. I'm not as bad off as others are, so I thank God for that. But, man—scary times.

¹² EI MEANS YOU
CANNOT
CONTROL
EVERYTHING

¹³ EI IS TAKING CARE OF
ONE'S OWN
HEALTH

¹⁴ EI MEANS KNOWING
YOUR LUCK

Unlike the fifteen process codes and thirty in vivo codes in the previous examples, there are now fourteen themes to work with. In the order they appear, they are:

EI IS TAKING ADVANTAGE OF
UNEXPECTED OPPORTUNITY
EI MEANS THINKING BEFORE
YOU ACT
EI IS BUYING CHEAP
EI MEANS SACRIFICE
EI IS SAVING A FEW DOLLARS NOW
AND THEN
EI MEANS KNOWING YOUR FLAWS
EI MEANS THINKING BEFORE YOU ACT
EI IS SETTING PRIORITIES
EI IS FINDING CHEAPER FORMS OF
ENTERTAINMENT
EI MEANS LIVING AN INEXPENSIVE
LIFESTYLE
EI IS NOTICING PERSONAL AND
NATIONAL ECONOMIC TRENDS
EI MEANS YOU CANNOT CONTROL
EVERYTHING
EI IS TAKING CARE OF ONE'S
OWN HEALTH
EI MEANS KNOWING YOUR LUCK

There are several ways to categorize the themes as preparation for analytic memo writing. The first is to arrange them in outline format with superordinate and subordinate levels, based on how the themes seem to take organizational shape and structure. Simply cutting and pasting the themes in multiple arrangements on a word processor page eventually develops a sense of order to them. For example:

I. EI MEANS LIVING AN INEXPENSIVE LIFESTYLE

- A. EI IS SETTING PRIORITIES
- B. EI MEANS THINKING BEFORE YOU ACT
- C. EI IS BUYING CHEAP
- D. EI IS FINDING CHEAPER FORMS OF ENTERTAINMENT
- E. EI IS SAVING A FEW DOLLARS NOW AND THEN
- F. EI IS TAKING CARE OF ONE'S OWN HEALTH
- G. EI IS TAKING ADVANTAGE OF UNEXPECTED OPPORTUNITY

II. EI MEANS YOU CANNOT CONTROL EVERYTHING

- A. EI MEANS SACRIFICE
- B. EI MEANS KNOWING YOUR FLAWS
- C. EI MEANS KNOWING YOUR LUCK
- D. EI IS NOTICING PERSONAL AND NATIONAL ECONOMIC TRENDS
- E. EI MEANS THINKING BEFORE YOU ACT

A second approach is to categorize the themes into similar clusters and to develop different category labels or *theoretical constructs*. A theoretical construct is an abstraction that transforms the central phenomenon's themes into broader applications but can still use "is" and "means" as prompts to capture the bigger picture at work:

Theoretical Construct 1: EI Means Knowing the Unfortunate Present

Supporting Themes:

- EI MEANS YOU CANNOT CONTROL EVERYTHING
- EI IS SETTING PRIORITIES
- EI MEANS KNOWING YOUR FLAWS
- EI MEANS SACRIFICE

Theoretical Construct 2: EI is Cultivating a Small Fortune

Supporting Themes:

- EI MEANS LIVING AN INEXPENSIVE LIFESTYLE
- EI MEANS THINKING BEFORE YOU ACT
- EI IS BUYING CHEAP
- EI IS FINDING CHEAPER FORMS OF ENTERTAINMENT
- EI IS SAVING A FEW DOLLARS NOW AND THEN

Theoretical Construct 3: EI Means a Fortunate Future

Supporting Themes:

- EI IS NOTICING PERSONAL AND NATIONAL ECONOMIC TRENDS
- EI MEANS THINKING BEFORE YOU ACT
- EI IS TAKING ADVANTAGE OF UNEXPECTED OPPORTUNITY
- EI IS TAKING CARE OF ONE'S OWN HEALTH
- EI MEANS KNOWING YOUR LUCK

What follows is an analytic memo generated from the cut-and-paste arrangement of themes into an outline and into theoretical constructs:

March 19, 2012

EMERGENT THEMES: FORTUNE/
FORTUNATELY/UNFORTUNATELY

I first reorganized the themes by listing them in two groups: "is" and "means." The "is" statements seemed to contain positive actions and constructive strategies for economic intelligence. The "means" statements held primarily a sense of caution and restriction with a touch of negativity thrown in. The first outline with two major themes, LIVING AN INEXPENSIVE LIFESTYLE and YOU CANNOT CONTROL EVERYTHING also had this same tone. This reminded me of the old children's picture book, *Fortunately/Unfortunately*, and the themes of "fortune" as a motif for the three theoretical constructs came to mind.

Knowing the Unfortunate Present means knowing what's (most) important and what's (mostly) uncontrollable in one's personal economic life. **Cultivating a Small Fortune** consists of those small money-saving actions that, over time, become part of one's lifestyle. **A Fortunate Future** consists of heightened awareness of trends and opportunities at micro and macro levels, with the understanding that health matters can idiosyncratically affect one's fortune. These three constructs comprise this particular individual's EI—economic intelligence.

Again, keep in mind that the examples above for coding and themeing were from one small interview transcript excerpt. The number of codes and their categorization would obviously increase, given a longer interview and/or multiple interviews to analyze. But the same basic principles apply: codes and themes relegated into patterned and categorized forms are heuristics—stimuli for good thinking through the analytic memo-writing process on how everything plausibly interrelates. Methodologists vary in the number of recommended final categories that result from analysis, ranging anywhere from three to seven, with traditional grounded theorists prescribing one central or core category from coded work.

QDA Strategy: To Assert

To assert in QDA is to put forward statements that summarize particular fieldwork and analytic observations that the researcher believes credibly represent and transcend the experiences.

Educational anthropologist Frederick Erickson (1986) wrote a significant and influential chapter on qualitative methods that outlined heuristics for *assertion development*. Assertions are declarative statements of summative synthesis, supported by confirming evidence from the data, and revised

when disconfirming evidence or discrepant cases require modification of the assertions. These summative statements are generated from an interpretive review of the data corpus and then supported and illustrated through narrative vignettes—reconstructed stories from field notes, interview transcripts, or other data sources that provide a vivid profile as part of the evidentiary warrant.

Coding or themeing data can certainly precede assertion development as a way of gaining intimate familiarity with the data, but Erickson's methods are a more admittedly intuitive yet systematic heuristic for analysis. Erickson promotes *analytic induction* and exploration of and inferences about the data, based on an examination of the evidence and an accumulation of knowledge. The goal is not to look for "proof" to support the assertions but plausibility of inference-laden observations about the local and particular social world under investigation.

Assertion development is the writing of general statements, plus subordinate yet related ones called *subassertions*, and a major statement called a *key assertion* that represents the totality of the data. One also looks for *key linkages* between them, meaning that the key assertion links to its related assertions, which then link to their respective subassertions. Subassertions can include particulars about any discrepant related cases or specify components of their parent assertions.

Excerpts from the interview transcript of our case study will be used to illustrate assertion development at work. By now, you should be quite familiar with the contents, so I will proceed directly to the analytic example. First, there is a series of thematically related statements the participant makes:

- "Buy one package of chicken, get the second one free. Now *that* was a bargain. And I got some."
- "With Sweet Tomatoes I get those coupons for a few bucks off for lunch, so that really helps."
- "I don't go to movies anymore. I rent DVDs from Netflix or Redbox or watch movies online—so much cheaper than paying over ten or twelve bucks for a movie ticket."

Assertions can be categorized into *low-level* and *high-level inferences*. Low-level inferences address and summarize "what is happening" within the particulars of the case or field site—the "micro." High-level inferences extend beyond the particulars to speculate on "what it means" in the more general social scheme of things—the "meso" or "macro." A reasonable low-level assertion about the three statements above collectively might read: *The participant finds several small ways to save money during a difficult economic*

period. A high-level inference that transcends the case to the macro level might read: *Selected businesses provide alternatives and opportunities to buy products and services at reduced rates during a recession to maintain consumer spending.*

Assertions are *instantiated* (i.e., supported) by concrete instances of action or participant testimony, whose patterns lead to more general description outside the specific field site. The author's interpretive commentary can be interspersed throughout the report, but the assertions should be supported with the *evidentiary warrant*. A few assertions and subassertions based on the case interview transcript might read (and notice how high-level assertions serve as the paragraphs' topic sentences):

Selected businesses provide alternatives and opportunities to buy products and services at reduced rates during a recession to maintain consumer spending. Restaurants, for example, need to find ways during difficult economic periods when potential customers may be opting to eat inexpensively at home rather than spending more money by dining out. Special offers can motivate cash-strapped clientele to patronize restaurants more frequently. An adult male dealing with such major expenses as underinsured dental care offers: "With Sweet Tomatoes I get those coupons for a few bucks off for lunch, so that really helps."

The film and video industries also seem to be suffering from a double-whammy during the current recession: less consumer spending on higher-priced entertainment, resulting in a reduced rate of movie theatre attendance (currently 39 percent of the American population, according to CNN); coupled with a media technology and business revolution that provides consumers less costly alternatives through video rentals and internet viewing: "I don't go to movies anymore. I rent DVDs from Netflix or Redbox or watch movies online—so much cheaper than paying over ten or twelve bucks for a movie ticket."

"Particularizability"—the search for specific and unique dimensions of action at a site and/or the specific and unique perspectives of an individual participant—is not intended to filter out trivial excess but to magnify the salient characteristics of local meaning. Although generalizable knowledge serves little purpose in qualitative inquiry since each naturalistic setting will contain its own unique set of social and cultural conditions, there will be some aspects of social action that are plausibly universal or "generic" across settings and perhaps even across

time. To work toward this, Erickson advocates that the interpretive researcher look for “concrete universals” by studying actions at a particular site in detail, then comparing those to other sites that have also been studied in detail. The exhibit or display of these generalizable features is to provide a *synoptic* representation, or a view of the whole. What the researcher attempts to uncover is what is both particular and general at the site of interest, preferably from the perspective of the participants. It is from the detailed analysis of actions at a specific site that these universals can be concretely discerned, rather than abstractly constructed as in grounded theory.

In sum, assertion development is a qualitative data analytic strategy that relies on the researcher’s intense review of interview transcripts, field notes, documents, and other data to inductively formulate composite statements that credibly summarize and interpret participant actions and meanings, and their possible representation of and transfer into broader social contexts and issues.

QDA Strategy: To Display

To display in QDA is to visually present the processes and dynamics of human or conceptual action represented in the data.

Qualitative researchers use not only language but illustrations to both analyze and display the phenomena and processes at work in the data. Tables, charts, matrices, flow diagrams, and other models help both you and your readers cognitively and conceptually grasp the essence and essentials of your findings. As you have seen thus far, even simple outlining of codes, categories, and themes is one visual tactic for organizing the scope of the data. Rich text, font, and format features such as italicizing, bolding, capitalizing, indenting, and bullet pointing provide simple emphasis to selected words and phrases within the longer narrative.

“Think display” was a phrase coined by methodologists Miles and Huberman (1994) to encourage the researcher to think visually as data were collected and analyzed. The magnitude of text can be essentialized into graphics for “at-a-glance” review. Bins in various shapes and lines of various thicknesses, along with arrows suggesting pathways and direction, render the study as a portrait of action. Bins can include the names of codes, categories, concepts, processes, key participants, and/or groups.

As a simple example, Figure 28.1 illustrates the three categories’ interrelationship derived from process coding. It displays what could be the apex of this interaction, LIVING STRATEGICALLY, and its

connections to THINKING STRATEGICALLY, which influences and affects SPENDING STRATEGICALLY.

Figure 28.2 represents a slightly more complex (if not playful) model, based on the five major *in vivo* codes/categories generated from analysis. The graphic is used as a way of initially exploring the interrelationship and flow from one category to another. The use of different font styles, font sizes, and line and arrow thicknesses are intended to suggest the visual qualities of the participant’s language and his dilemmas—a way of heightening *in vivo* coding even further.

Accompanying graphics are not always necessary for a qualitative report. They can be very helpful for the researcher during the analytic stage as a heuristic for exploring how major ideas interrelate, but illustrations are generally included in published work when they will help supplement and clarify complex processes for readers. Photographs of the field setting or the participants (and only with their written permission) also provide evidentiary reality to the write-up and help your readers get a sense of being there.

QDA Strategy: To Narrate

To narrate in QDA is to create an evocative literary representation and presentation of the data in the form of creative nonfiction.

All research reports are stories of one kind or another. But there is yet another approach to QDA that intentionally documents the research experience *as* story, in its traditional literary sense. Narrative inquiry plots and story lines the participant’s experiences into what might be initially perceived as a fictional short story or novel. But the story is carefully crafted and creatively written to provide readers with an almost omniscient perspective about the participants’ worldview. The transformation of the corpus from database to creative nonfiction ranges from systematic transcript analysis to open ended literary composition. The narrative, though, should

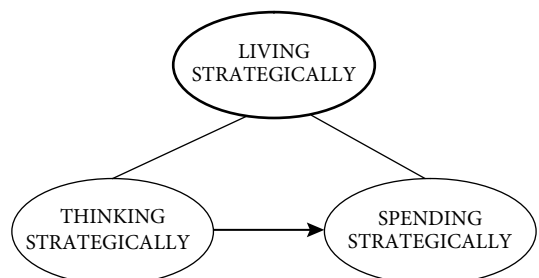


Figure 28.1 A simple illustration of category interrelationship.

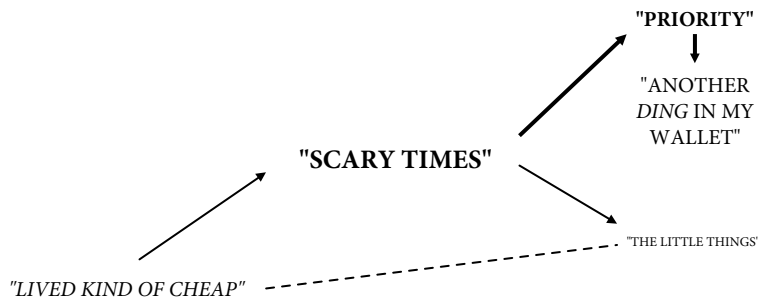


Figure 28.2 An illustration with rich text and artistic features.

be solidly grounded in and emerge from the data as a plausible rendering of social life.

The following is a narrative vignette based on interview transcript selections from the participant living through tough economic times:

Jack stood in front of the soft drink vending machine at work and looked almost worriedly at the selections. With both hands in his pants pockets, his fingers jingled the few coins he had inside them as he contemplated whether he could afford the purchase. One dollar and fifty cents for a twenty-ounce bottle of Diet Coke. One dollar and fifty cents. “I can practically get a two-liter bottle for that same price at the grocery store,” he thought.

Then Jack remembered the upcoming dental surgery he needed—that would cost one thousand dollars—and the bottle of insulin and syringes he needed to buy for his diabetic, “high maintenance” cat—about one hundred and twenty dollars. He sighed, took his hands out of his pockets, and walked away from the vending machine. He was skipping lunch that day anyway so he could stock up on dinner later at the cheap-but-filling-all-you-can-eat Chinese buffet. He could get his Diet Coke there.

Narrative inquiry representations, like literature, vary in tone, style, and point of view. The common goal, however, is to create an evocative portrait of participants through the aesthetic power of literary form. A story does not always have to have a moral explicitly stated by its author. The reader reflects on personal meanings derived from the piece and how the specific tale relates to one’s self and the social world.

QDA Strategy: To Poeticize

To poeticize in QDA is to create an evocative literary representation and presentation of the data in the form of poetry.

One form for analyzing or documenting analytic findings is to strategically truncate interview

transcripts, field notes, and other pertinent data into poetic structures. Like coding, poetic constructions capture the essence and essentials of data in a creative, evocative way. The elegance of the format attests to the power of carefully chosen language to represent and convey complex human experience.

In vivo codes (codes based on the actual words used by participants themselves) can provide imagery, symbols, and metaphors for rich category, theme, concept, and assertion development, plus evocative content for arts-based interpretations of the data. Poetic inquiry takes note of what words and phrases seem to stand out from the data corpus as rich material for reinterpretation. Using some of the participant’s own language from the interview transcript illustrated above, a poetic reconstruction or “found poetry” might read:

Scary Times

Scary times . . .
 spending more
 (another ding in my wallet)
 a couple of thousand
 (another ding in my wallet)
 insurance is just worthless
 (another ding in my wallet)
 pick up the tab
 (another ding in my wallet)
 not putting as much into savings
 (another ding in my wallet)
 It all adds up.

Think twice:
 don't really need
 skip

Think twice, think cheap:
 coupons
 bargains
 two-for-one
 free

Think twice, think cheaper:
stock up
all-you-can-eat
(cheap—and filling)

It all adds up.

Anna Deavere Smith, a verbatim theatre performer, attests that people speak in forms of “organic poetry” in everyday life. Thus in vivo codes can provide core material for poetic representation and presentation of lived experiences, potentially transforming the routine and mundane into the epic. Some researchers also find the genre of poetry to be the most effective way to compose original work that reflects their own fieldwork experiences and autoethnographic stories.

QDA Strategy: To Compute

To compute in QDA is to employ specialized software programs for qualitative data management and analysis.

CAQDAS is an acronym for Computer Assisted Qualitative Data Analysis Software. There are diverse opinions among practitioners in the field about the utility of such specialized programs for qualitative data management and analysis. The software, unlike statistical computation, does not actually analyze data for you at higher conceptual levels. CAQDAS software packages serve primarily as a repository for your data (both textual and visual) that enable you to code them, and they can perform such functions as calculate the number of times a particular word or phrase appears in the data corpus (a particularly useful function for content analysis) and can display selected facets after coding, such as possible interrelationships. Certainly, basic word-processing software such as Microsoft Word, Excel, and Access provide utilities that can store and, with some pre-formatting and strategic entry, organize qualitative data to enable the researcher’s analytic review. The following internet addresses are listed to help in exploring these CAQDAS packages and obtaining demonstration/trial software and tutorials:

- AnSWR: www.cdc.gov/hiv/topics/surveillance/resources/software/answr
- ATLAS.ti: www.atlasti.com
- Coding Analysis Toolkit (CAT): cat.ucsur.pitt.edu/
- Dedoose: www.dedoose.com
- HyperRESEARCH: www.researchware.com
- MAXQDA: www.maxqda.com
- NVivo: www.qsrinternational.com

- QDA Miner: www.provalisresearch.com
- Qualrus: www.qualrus.com
- Transana (for audio and video data materials): www.transana.org
- Weft QDA: www.pressure.to/qda/

Some qualitative researchers attest that the software is indispensable for qualitative data management, especially for large-scale studies. Others feel that the learning curve of CAQDAS is too lengthy to be of pragmatic value, especially for small-scale studies. From my own experience, if you have an aptitude for picking up quickly on the scripts of software programs, explore one or more of the packages listed. If you are a novice to qualitative research, though, I recommend working manually or “by hand” for your first project so you can focus exclusively on the data and not on the software.

QDA Strategy: To Verify

To verify in QDA is to administer an audit of “quality control” to your analysis.

After your data analysis and the development of key findings, you may be thinking to yourself, “Did I get it right?” “Did I learn anything new?” Reliability and validity are terms and constructs of the positivist quantitative paradigm that refer to the replicability and accuracy of measures. But in the qualitative paradigm, other constructs are more appropriate.

Credibility and *trustworthiness* (Lincoln & Guba, 1985) are two factors to consider when collecting and analyzing the data and presenting your findings. In our qualitative research projects, we need to present a convincing story to our audiences that we “got it right” methodologically. In other words, the amount of time we spent in the field, the number of participants we interviewed, the analytic methods we used, the thinking processes evident to reach our conclusions, and so on should be “just right” to persuade the reader that we have conducted our jobs soundly. But remember that we can never conclusively “prove” something; we can only, at best, convincingly suggest. Research is an act of persuasion.

Credibility in a qualitative research report can be established through several ways. First, citing the key writers of related works in your literature review is a must. Seasoned researchers will sometimes assess whether a novice has “done her homework” by reviewing the bibliography or references. You need not list everything that seminal writers have published about a topic, but their names should appear at least once as evidence that you know the field’s key figures and their work.

Credibility can also be established by specifying the particular data analytic methods you employed (e.g., “Interview transcripts were taken through two cycles of process coding, resulting in five primary categories”), through corroboration of data analysis with the participants themselves (e.g., “I asked my participants to read and respond to a draft of this report for their confirmation of accuracy and recommendations for revision”) or through your description of how data and findings were substantiated (e.g., “Data sources included interview transcripts, participant observation field notes, and participant response journals to gather multiple perspectives about the phenomenon”).

Creativity scholar Sir Ken Robinson is attributed with offering this cautionary advice about making a convincing argument: “Without data, you’re just another person with an opinion.” Thus researchers can also support their findings with relevant, specific evidence by quoting participants directly and/or including field note excerpts from the data corpus. These serve both as illustrative examples for readers and to present more credible testimony of what happened in the field.

Trustworthiness, or providing credibility to the writing, is when we inform the reader of our research processes. Some make the case by stating the duration of fieldwork (e.g., “Seventy-five clock hours were spent in the field”; “The study extended over a twenty-month period”). Others put forth the amounts of data they gathered (e.g., “Twenty-seven individuals were interviewed”; “My field notes totaled approximately 250 pages”). Sometimes trustworthiness is established when we are up front or confessional with the analytic or ethical dilemmas we encountered (e.g., “It was difficult to watch the participant’s teaching effectiveness erode during fieldwork”; “Analysis was stalled until I recoded the entire data corpus with a new perspective.”).

The bottom line is that credibility and trustworthiness are matters of researcher *honesty* and *integrity*. Anyone can write that he worked ethically, rigorously, and reflexively, but only the writer will ever know the truth. There is no shame if something goes wrong with your research. In fact, it is more than likely the rule, not the exception. Work and write transparently to achieve credibility and trustworthiness with your readers.

Conclusion

The length of this article does not enable me to expand on other qualitative data analytic strategies,

such as to conceptualize, abstract, theorize, and write. Yet there are even more subtle thinking strategies to employ throughout the research enterprise, such as to synthesize, problematize, persevere, imagine, and create. Each researcher has his or her own ways of working, and deep reflection (another strategy) on your own methodology and methods as a qualitative inquirer throughout fieldwork and writing provides you with metacognitive awareness of data analytic processes and possibilities.

Data analysis is one of the most elusive processes in qualitative research, perhaps because it is a backstage, behind-the-scenes, in-your-head enterprise. It is not that there are no models to follow. It is just that each project is contextual and case specific. The unique data you collect from your unique research design must be approached with your unique analytic signature. It truly is a learning-by-doing process, so accept that and leave yourself open to discovery and insight as you carefully scrutinize the data corpus for patterns, categories, themes, concepts, assertions, and possibly new theories through strategic analysis.

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Computer-Assisted Analysis of Qualitative Research

Christina Silver *and* Ann F. Lewins

Abstract

This chapter looks at the current state of technological support for qualitative research. Technological developments have enabled new forms of data and other analyses to be incorporated into qualitative work. The chapter focuses on technology and how it assists three main aspects of qualitative research: data collection, preparation, and/or transcription; bibliographic management and systematic literature reviews; and data management and analysis. The main body of the chapter discusses the functionality, role, and implications of Computer Assisted Qualitative Data Analysis (CAQDAS) tools. Three recent trends in computer assistance are emphasized: support for visual analysis, support for mixed methods approaches, and online solutions.

Key Words: Computer-Assisted Qualitative Data Analysis (CAQDAS), qualitative data analysis (QDA), interview data, literature, mixed-methods, multi-media, textual, transcripts

Using computer assistance has become a widely accepted strategy for the collection, storage, management, analysis, and reporting of research data across many academic disciplines and beyond. The different forms of qualitative research have a varied history, but trends in computational assistance facilitate opportunities for their systematic and transparent adoption. This is particularly so when the aim is to integrate analyses or to mix methods. Since the 1960s, computer technology revolutionized the potential for analyzing qualitative data. Initially literary analysis and the analysis of documentation in the “hard” sciences made use of mainframe lexical software. Such software is now available more generally, much of it being low cost or free. Concordances, one aspect of such textual analyses, provide listings of words surrounded by their immediate context. Long-standing examples of such packages are Concordance and TextWorld, some of them providing very fast processing of large amounts of data with new indexing and frequency tools. However,

discussion about computer assistance in qualitative research often focuses on the broad category of software commonly referred to as Computer Assisted Qualitative Data Analysis (CAQDAS). These tools developed early in the history of personal computing, when researchers in the social sciences initially began to create their own software, making them commercially available in the early 1980s. The focus of most CAQDAS packages from the outset was on providing tools to facilitate handling and analysis of textual data. The number of packages available has grown over time, as has the range of tools available within them. Development has been particularly rapid over the last 10 years with a number of significant developments, including the potential to handle, integrate, and analyze a wider range of data types; larger data corpora; and the inclusion of tools offering new ways to interrogate and visualize data.

However, there are several other peripheral areas of computer software development relevant to qualitative research that influence changes in the way

researchers work. This chapter discusses software designed to support three key stages of the research process: data collection, preparation, and transcription; bibliographic management and systematic literature reviews; and data handling and analysis. We also highlight three recent trends in computer assistance: support for visual analysis, support for mixed methods approaches, and online solutions. The main body of the chapter discusses the functionality, role, and implications of CAQDAS tools and discusses their role in supporting qualitative and mixed methods analyses.

Software Tools and Research Processes

Throughout all stages of data handling, available applications include downloadable and free or open-source and low cost, or they might be full-blown commercial applications. Indeed, there are many non-bespoke tools, originally developed for alternative purposes, that can be usefully manipulated to support researchers' needs at different stages of the research process. This is particularly so in the current internet environment (Snee 2008, di Gregorio 2010), where no downloading or installation is required. The proliferation of options makes investigating suitability for any particular purpose a somewhat daunting process. Here we first provide a general overview of some of the many tools that can facilitate qualitative research in terms of three core stages of the research process. Most of the packages mentioned (for computer use) are listed in the resource section at the end of the chapter.¹

Data Collection and Transcription

Capturing interviews, focus groups, and other forms of social interaction in optimum quality recordings is important in maintaining reliable access to data and the phenomena they represent. The move from analogue to digital has presented social researchers with a number of advantages, not least of which is the ease with which recordings can be generated and manipulated. No longer are bulky cassette recorders required, and the risk of damaging fragile tapes is obviated when using digital recording devices. In the case of generating audio recordings, even specialized recording equipment may no longer be required, as low-cost mobile phone applications are available that may be sufficient for the task. Examples include Recorder for the iPhone, Tape-a-Talk audio recorder from Google, and the Dragon Recorder application. When it is necessary to edit, improve, or manipulate audio recordings before

undertaking analysis, packages such as Audacity are useful.

Generating video recordings for research purposes is a much more complex and time-consuming process. Technical issues are more complicated and, importantly, ethical concerns are more complex to resolve. Although mobile devices often also provide video recorders, it will normally be necessary to use more sophisticated dedicated video recording devices. There are a number of video editing tools available, including free options (many of which can be downloaded from the CNET technology review site) as well as commercial packages such as Camtasia, MoviePlus, and AVS Video Editor.

Transcription of qualitative data has been aided to some extent by technological developments. There are a number of software packages, mostly free or low cost, that assist the routine processes of generating textual transcriptions of digitized sound or video files. Transana, Transcriber, HyperTRANSCRIBE, and F4/F5 are all specifically designed to assist in this process. They do not transcribe automatically but provide keyboard shortcuts and auto-structuring of the textual transcriptions. With these packages, the written transcript can remain linked to—or synchronized with—the associated media file. This allows subsequent analysis of the audio/visual concurrently with the written version. Some CAQDAS packages also provide the ability to transcribe audio/video and thereby to generate synchronized transcripts. However, few include advanced transcription tools such as those in bespoke transcription software, so it is usually preferable to transcribe first and subsequently import transcripts into the chosen CAQDAS package. Figure 29.1 shows the transcription tool F4, which offers a range of tools specifically designed to aid transcription of audio and video data. These include keyboard or foot pedal controls, audio visualization via the waveform, manual, or automatic insertion of time stamps (which enable the synchronization between the source media and the written transcript) and time stamp convertors for enabling transcripts to be imported and worked with using several CAQDAS packages.

In addition to transcription tools, there are several voice-recognition software packages that enable one voice to dictate text that the software then transcribes into a machine-readable format. Many computer users who have wrist and hand problems such as repetitive strain injury, which severely affect their ability to use the mouse, use voice-recognition software. Voice-recognition software can only recognize

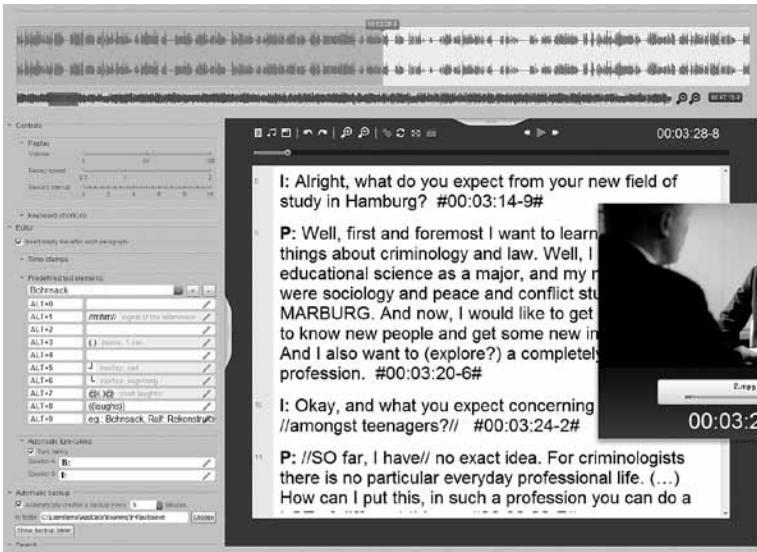


Figure 29.1 F4 tool for developing synchronized transcriptions.

speech by the person who has trained it, so to use the software for interview transcription involves listening to sections of the recording and then reading back the sections in your own voice. Additionally users have to familiarize themselves with a number of voice-activated editing commands to correct mistakes made by the software during inputting. Options include Dragon Naturally Speaking, Talking Desktop, and Express Scribe, although their reliability may vary in generating accurate and usable transcripts for analysis without further editing.

However written transcriptions are generated, their role in the research process is important (Oliver et al., 2005, Silver & Patashnick, 2011). In most cases, they become the vehicle through which data are analyzed. There are various different established protocols for generating transcripts, and the approach adopted will be informed by the analytic needs of the project. If you intend to use a CAQDAS package to facilitate analysis though, it is important when preparing for transcription to be aware of the requirements of the chosen package. Silver & Lewins 2014 dedicate a chapter to ideas and tips for good data management and transcription in particular. Keeping in mind that CAQDAS packages handle structures in textual transcripts in different ways, it is important to be aware of software specific requirements as early as possible. Data might contain repeated structures that would benefit from auto coding. Developing a transcription protocol for each specific form of data is a useful exercise, especially if using external transcribers.

See the section “Auto-coding of structures and content” later in this chapter for more discussion of this.

Epistemological Concerns

When discussing the role of technology to collect and transform data, it is important to be aware of principal epistemological concerns about the partial nature of transcripts, or even recordings, as data. This has always been the case and, before verbatim transcripts of interviews became more common, observational note taking was even more subject to this issue. Mason (2002) cautions against “overestimate[ing] the representational and reflective qualities of interview transcripts.” Not only is a transcript prone to subjective translation, it may additionally neglect significant non-verbal interaction. Equally, she reminds us that sound and video recordings necessarily leave out information and unrecorded circumstances. Discussing visual data in particular, Mason suggests that the “debate should not be about what technology can do but what it is about the visual that interests us and how we are doing the visualizing.” Harper (1994) debates the building of a visual sociology and the advantages and traps of technology: he warns that, for an ethnographer, the collection of thousands of images does not equate to knowledge. He says of his own photographs of a backwoods mechanic that “the transition from images that communicated poverty and disorder to those that showed community and creative intelligence was made only with the spirited involvement of my subject/friend.” In the software

context, practical possibilities combine with analytic requirement (Silver & Patashnick, 2011). There will always be tension between the nature of research, the fundamental nature of data, the new forms of data, and new methods of data handling enabled by technology. The important thing is to be reflexively aware and critical of the value and limitations of data and the way they are collected.

Document and Bibliographic Management

A key aspect of conducting qualitative research relates to data storage and management. As discussed later in this chapter, project management is a core potential of CAQDAS package functionality, and a variety of CAQDAS functions are described that have either specifically been designed for managing “literature” or documentary material or they can be adapted to manage such materials and references. These work alongside more analytic functions. In addition, there are many more generic data storage and management systems (open source/freeware and commercial) with fewer analytic functions, as we discuss here.

The emphasis of these tools is primarily on improving access to documentary material. Searching amongst document titles is usually possible, and when the whole file version of a document is available, some searching and exploring of

document content is enabled. To keep track of their relevance in an orderly way, cataloguing, “tagging,” and varied sorting and filtering processes are possible. Tagging facilities are tools that serve similar purposes to coding tools in CAQDAS packages (see section on Data Analysis later in this chapter) but they are usually far less powerful in retrieving information on that basis and do not include the other analytic tools characteristic of CAQDAS packages. Examples of freely available document-management systems include InfoRapid Cardfile, Epiware and, Kordil EDMS. Most enable collaborative, web-based access. Commercial options include eDocXL and DocPoint Personal. Figure 29.2 illustrates InfoRapid Cardfile.

Cloud-based document-management systems include Evernote² (available for iPhone, Android, and Blackberry platforms), where documents or pages that are “clipped” while browsing are automatically synchronized with the cloud archive. Many of these applications have free versions with premium versions with extra functionality available at an additional cost.

Bibliographic software has developed in tandem with other tools, providing sophisticated means to manage references, store links to electronic documents, hold researchers’ notes about documents, and generate citations in various academic styles. As well as the long-established standalone packages

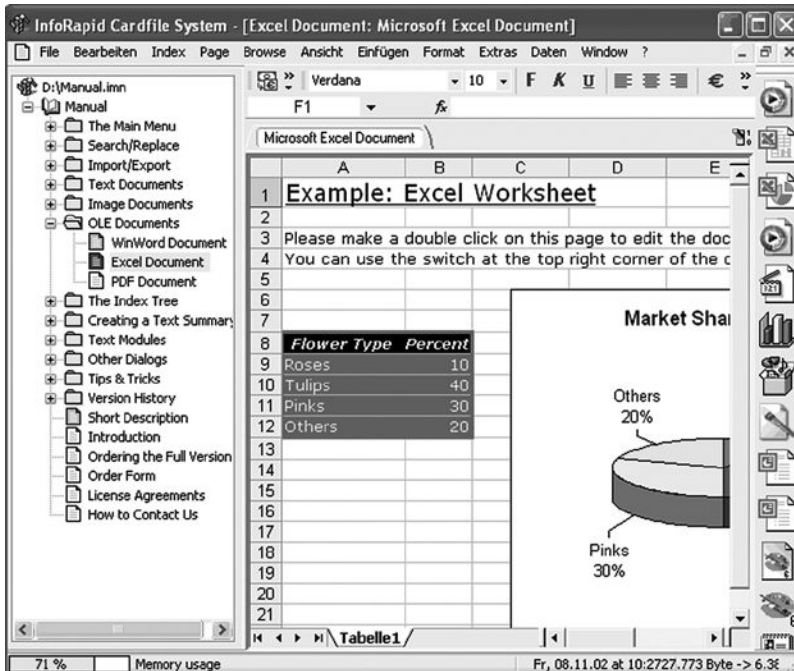


Figure 29.2 InfoRapid Cardfile System for managing documents.

such as Endnote and RefWorks, online versions (often free) are now also available (e.g., Zotero), which offer a particularly quick way of generating references out of web content as well as from online libraries. Figure 29.3 shows the online bibliographic tool Zotero, which is integrated with the Mozilla Firefox browser, enabling quick and easy transference of bibliographic materials into the system.

Some CAQDAS packages, notably NVivo, have developed routines for importing libraries from bibliographic software. Although there is no dynamic relationship between the CAQDAS package and the bibliographic package (in that changes made in one do not affect the content of the other), these routines can significantly increase the ways that CAQDAS packages can facilitate the creation of systematic literature reviews (Silver & Lewins, 2014).

Data Analysis

The analysis of qualitative data is central to the support offered by computer developments in terms of providing the means of handling and integrating data, recording ideas about them, and interrogating materials in ways that for some media might not be achievable “manually” or outside the package. Software packages that fall under the CAQDAS acronym focus on providing these extra analytic dimensions. They were initially developed by academics involved in qualitative data analysis during the late 1980s. Early pioneers were often

experienced analysts frustrated by the “messiness” of manual methods of qualitative analysis and eager to explore the opportunities afforded by new computer technologies to systematize processes and increase the reliability of data storage and access. Fielding & Lee (1991/1993) collected essays from the developers and methodologists involved in early innovations to document the various rationales behind developments and in the context to comment on qualitative methodologies and substantive uses of software.

In addition to the multiple forms of qualitative media handled by CAQDAS packages, the integration of quantitative information with and about qualitative data enables a range of possibilities for mixed-methods projects (Bazeley, 2006, Bazeley, 2008; Fielding, 2012). Amongst the main benefits of using CAQDAS packages are their support for data organization, the management and tracking of ideas, using memo and *code and retrieve* functions, and the support for sophisticated and systematic data interrogation. Mixed methods have a dominant place in many academic and applied social research settings as well as an increasing presence in arts and science disciplines and applied settings such as government and commercial sectors. There are a range of different products available from within the CAQDAS tradition and from outside with no global or dominant market leader. This speaks to the fact that researchers approach projects, the analysis of data, and the use of software from

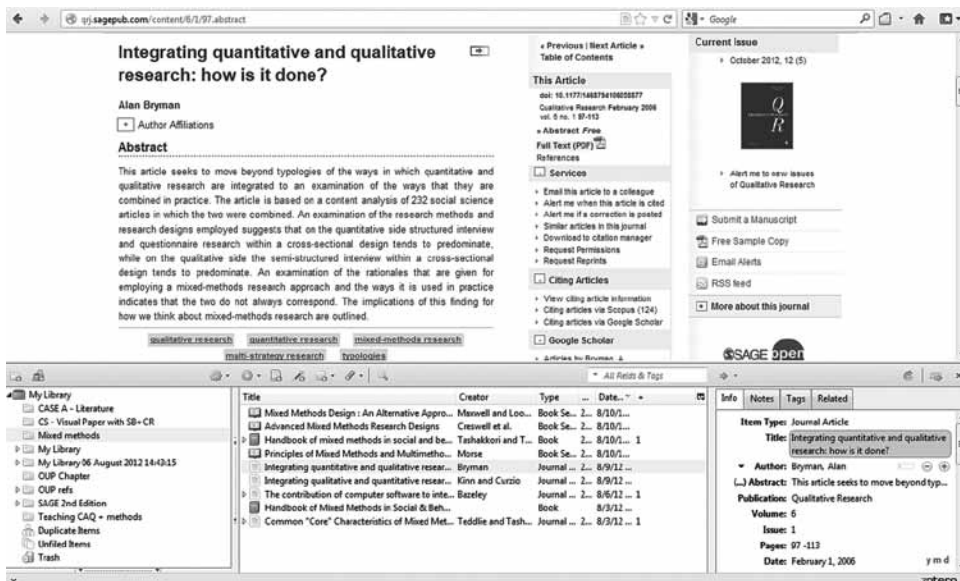


Figure 29.3 Zotero, an online bibliographic tool.

a range of methodological and epistemological perspectives. CAQDAS packages are discussed in detail later in this chapter.

Alongside the CAQDAS group of software packages are others that can facilitate data analysis. Mind-mapping and note-taking tools, as well as those developed specifically for market research requirements, are notable examples.

The use of models features in several analytic approaches. They might be theoretical frameworks informing problem conceptualization, abstract descriptions of generalizable relationships or hypotheses, or graphic maps illustrating interactions, processes, and connected concepts and issues. In deductive, theoretically driven projects, models may be pre-existent or developed at an early stage and used as a framework that guides analysis (as in Layder's 1998 adaptive theory). In more inductive, exploratory approaches, model development might be one of the analytic aims of the project, developed as a means by which to represent linkages and common interactions and to illustrate interpretations (as in grounded theory as espoused in 1990, by Strauss & Corbin). In either case, it is sometimes helpful to express a theoretical or explanatory model in a graphical sense. Outside of the traditions of qualitative data analysis are a range of broader mapping traditions and tools including mind mapping (Buzan, 1995), concept mapping (Novak, 1993; Novak & Gowin, 1984), and cognitive and causal mapping

(Eden 1988; Bryson, Ackermann et al 2004). These ideas and associated tools began to offer ways to express connections and structures in a visual way.

Some mapping software packages are developed to emulate particular theoretical models. Decision Explorer, mapping software widely used in academic and strategic management fields, was created with personal construct theory in mind, though its application and analytic functions are adaptable to other mapping traditions and modeling purposes. Other mapping tools like CMap, ConceptMap, and Inspiration have grown out of learning initiatives in higher education. Many others have been created to support strategic management decision making. Examples include Mindnode and XMind. Figure 29.4 illustrates the XMind, which provides various default map layouts from which to choose and enables the user to define layout options and design features.

The general distinction between such mapping programs and the related mapping functions in some CAQDAS packages is the integration between maps and the source data in the latter. This is discussed in the later section "Making Connections." *Maps* and *mapping* are terms that of course are relevant to many methodologies, situations, and sciences. Mapping tools integrated into CAQDAS packages have their own philosophical underpinnings and analytic purposes, much like bespoke mapping tools do. When using QDA Miner

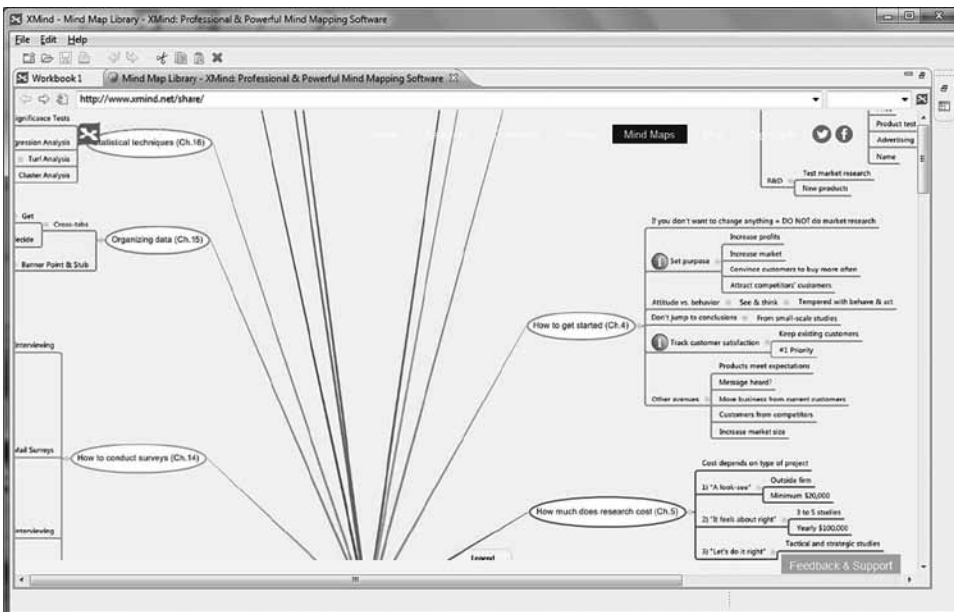


Figure 29.4 XMind mapping software.

combined with the WordStat module, for instance, researchers using a constructionist, language-based approach to data can find the occurrence of words (and codes) mapped in a number of different representations. For example, *dendrograms* and *cluster maps* chart proximity relationships between words and codes, and interactive *heatmaps* map the position and concentrations of words within files and according to variables. Such tools are not universally provided by CAQDAS packages; indeed the types of visualisation tool provided constitute a key difference between products.

Recent Trends

It is useful to highlight three areas of recent technological development that are important factors in the assessment of computer-assisted qualitative data analysis: support for visual analysis, support for mixed-methods approaches, and online qualitative solutions and collaborative analysis.

Support for Visual Analysis

Many CAQDAS packages enable the analysis of multimedia data, including digital audio, graphics, and video. However, most were initially developed

as text analysis tools and added multimedia data handling capabilities latterly (ATLAS.ti is a notable exception as it including the ability to handle still and moving images from very early on in its history). Tools developed for the analysis of text, however, are not always fine enough for the demands of multidimensional data such as video (Silver & Patashnick, 2011).

There are many bespoke tools for the analysis of visual data, some of which fall under the CAQDAS umbrella—such as Transana—in that they provide tools to facilitate *qualitative* analyses of visual data. Others—such as The Observer and Interact—were developed within the behavioral rather than social sciences, and therefore adopt a more *quantitative* approach to the analysis of visual data. These are not generally categorized as CAQDAS packages, although there are some significant overlaps in functionality.

Figure 29.5 illustrates the Transana main interface's four main windows: Visualization, Video Media File, Data, and Transcript. In this example, there are two separate written transcripts associated with the video media file on display (Transana enables up to four transcripts per media). This video

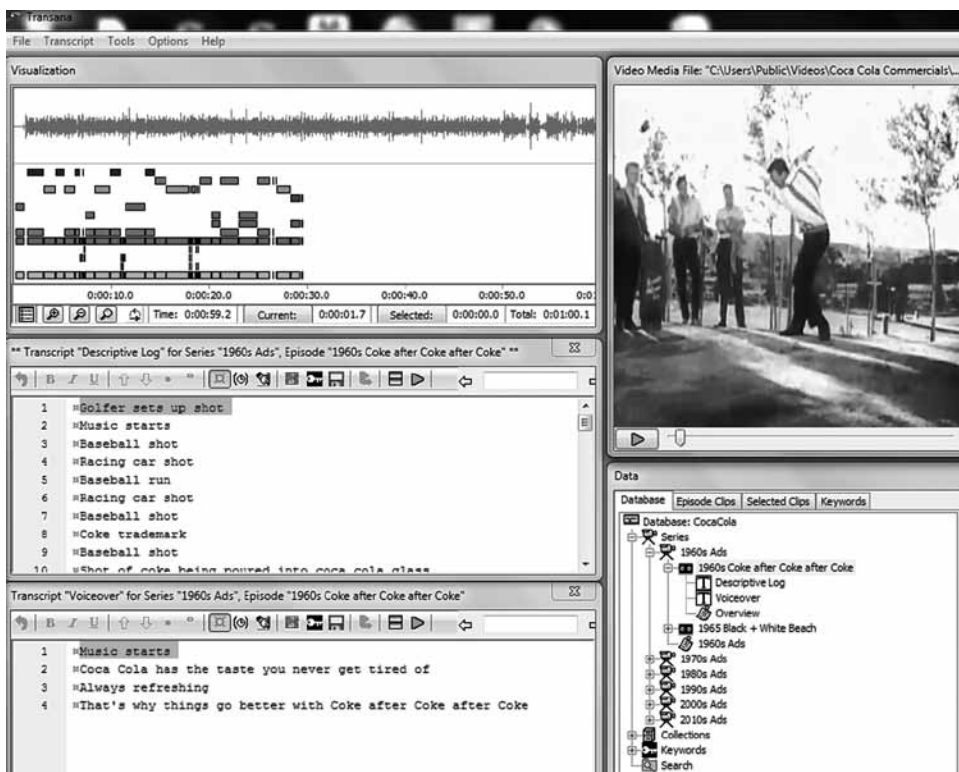


Figure 29.5 TRANSANA, a video transcription and analysis tool.

media file has been partially coded, illustrated by the coloured stripes appearing along the waveform visualization. Full synchronicity is enabled between the different windows such that, as the media file plays, the corresponding portion of the transcript(s) are highlighted in blue and a marker shows progress along the media timeline in the visualization view.

Video editing tools may also be used for analytic purposes, and in some cases may provide tools that are particularly conducive to the analytic needs of the multidimensionality of visual data. Options include Camtasia, VideoDub, and Corel VideoStudio.

There are a number of important considerations in choosing software for visual analysis, not least of which is whether the need is for direct (without an associated written transcript) or indirect coding (analysis via a written transcript of some type) of audio, video, and still images (Silver & Patashnick, 2011). In addition are considerations around the required outputs of visual analyses. It is important to distinguish what can be achieved inside software and what is required beyond or outside the software. Following analysis, if a new output file is required for playback outside the software—for example, all the clips put together of a classroom situation that have been coded or tagged for a particular behavior—then the software application requires a video editing capability. This usually requires a significant extra investment in software capability. The Observer is one application developed for visual analysis of behaviors that includes an editing module as standard to allow the export of coded clips. Most CAQDAS packages do not include such functionality, although Transana does. However, even in CAQDAS applications, as long as the user is working *in* the software, it is an easy matter to play back all the relevant coded clips.

Support for Mixed-Methods Approaches

Mixed-methods research is an area that has been receiving increasing attention in recent years but one that is varied in terms of definitions and applications. Creswell & Plano Clark (2010), for example, highlight the emergence of several different definitions focusing variously on elements of methods, research processes, philosophy, and research design. Tashakkori & Teddlie (2003), in the first significant handbook for mixed methods, make detailed distinctions between multi-method, mixed-method and mixed-modal (multiple stranded) research in an attempt to provide a rational and consistent typology of a maturing methodological area. Here we

refer to mixed methods in the context of technological support in a broad sense, using it to encompass both the combination and integration of diverse types of data (both different types of qualitative data and qualitative plus quantitative data) and the ability to conduct quantitative and qualitative analyses of those data. Software support is developing fast as new media and new functionality are enabled in existing software via quantitative elements added to qualitative packages or vice versa, qualitative elements added to particular types of quantitative provision, and other methods.

In terms of the use of a range of data types to inform a research project, CAQDAS packages are well developed, with most now enabling the direct handling of a range of textual and multimedia formats. Fielding comments that the “point of mixing methods is to see the analytic implications of linking data derived from different methods rather than have findings from different methods ‘talk past each other’” (2012:125). He goes on to illustrate three types of data integration supported by CAQDAS packages: the integration of geographic spatial and qualitative data, the integration of multi-stream visual data and the integration of quantitative and qualitative data. The former two have only relatively recently been enabled and remain in their infancy in terms of facilitation of analysis, whereas the latter has been enabled in most CAQDAS packages since their infancy.

Earlier we mentioned concordance style packages. However CAQDAS packages increasingly provide tools for enabling quantitative analyses of the content of qualitative data. Some, such as QDA Miner (with add-on modules, WordStat & SimStat) and Dedoose, developed with this function clearly in mind and include well-refined options. Others, such as NVivo, ATLAS.ti, and particularly MAXQDA, have focused on developments in this area recently.

Online Qualitative Solutions and Semi-Synchronous Collaborative Analysis

Software tools designed to facilitate the analysis of qualitative and mixed-methods projects historically have been developed largely as standalone applications, initially for individual users, but lately also for team-based research (see the section “Collaboration Using CAQDAS Packages” later in this chapter). There are, however, a number of web-based applications available that provide analytic tools and the ability to work almost synchronously on the same project from different locations. The degree

of synchronicity varies slightly with individual actions on each workstation, taking a moment to update to the shared project hosted at the web site or server. One benefit of these applications is that they obviate the issue of platform. Historically few CAQDAS packages have been developed specifically for Macintosh or as dual-platform packages (notable exceptions being HyperRESEARCH, Transana, and TAMS Analyser). Online applications therefore may be more widely accessible. They are, however, reliant on internet connectivity, which may present issues when traveling or working in the field. Amongst such bespoke research solutions are Coding Analysis Toolkit, Dedoose, Liveminds, and Transana MU (the multi-user version of Transana).

Dedoose is an online application specifically designed to support collaborative qualitative and mixed methods research projects and includes a good range of qualitative tools found in “traditional” CAQDAS packages, as well as additional quantitative analytic tools and representations. Liveminds is an example of a different online approach, with the emphasis on supporting “ongoing” research—in other words, to facilitate the collection and analysis of data online and in “real time.” Transana MU, specifically oriented to the analysis of video, allows users to edit and share in the production of transcriptions, annotations, and in the application of key words (coding).

Functionality, Role and Implication of CAQDAS Use

We have so far outlined the broader range of software tools available for qualitative approaches and emphasized a trend toward mixed-method provision and approaches. The remainder of this chapter focuses specifically on the functionality, role, and implications of the use of tools that are generally referred to as CAQDAS packages.

CAQDAS packages are designed to facilitate the management and analysis of qualitative studies in varying ways. Qualitative researchers have different perceptions about how they will develop analysis, and not all will use code-based approaches. Code-based tools were indeed the starting points for such packages, allowing for the application of codes to data chunks so that data with similar relevancies could be collected together and compared. However, work does not have to be based on a “thematic” approach, so we discuss how other ways of managing interpretation of text or multimedia data are enabled in CAQDAS packages. If mixing or integration of methods is required, this can be

enabled at different levels: the mixing of media, the mixing of analyzes, the mixing of contributions, as well as other elements or phases of project work. Examples include the integration of numeric data from a quantitative phase of a study to elaborate or enhance the testing of relationships in qualitative work; conversely the qualitative perspective could optimize the value of open-ended questions in surveys (Fielding, Fielding, & Hughes, 2012).

CAQDAS packages act as containers for accessing all materials pertaining to a research project and researchers’ ideas, analyzes, and results. Although many researchers come to CAQDAS packages at the point at which they feel “ready to analyze,” the potential values of using bespoke tools are more wide ranging. We encourage the conceptualization of these programs as project management tools that can be effectively manipulated to support all stages of the research process, from problem formulation and reviewing the literature, through project planning, management of primary and/or secondary data, as well as the analysis and write-up (Lewins & Silver, 2007, Silver & Lewins, 2014). The range of tools provided can be utilized to support different approaches to qualitative or mixed-methods data analysis. CAQDAS packages do not dictate the way in which tasks are performed, but the tools they offer may increase the complexity of tasks that are possible and the researcher’s readiness to perform them. In particular, they encourage flexibility in revisiting, rethinking, or repeating analytic processes.

The remainder of this chapter considers the potential role of CAQDAS packages in the research process, illustrating ways in which they can support organizational, analytic, and conceptual work and commenting on some limitations and constraints.

Processes of Analysis and Methodological Underpinnings

Qualitative analyses involve a range of tasks. Across the broad spectrum of approaches, there is no prerequisite set of procedures or strict sequence according to which they are undertaken. Rather than being linear, analysis is a cyclical and iterative process in which researchers need to be able to flick between tasks and processes as ideas are explored, hypotheses tested, patterns identified, and relationships expressed (see Figure 29.6). We can illustrate a similar process in a moment of activity “in a typical 5 minute period we might slip from carefully reading and coding the data to using fast exploration tools...to find similar occurrences of a particular

word...to reviewing what has been coded so far and to annotating what we see” (Lewins & Silver, 2007, 228). So among the main benefits of using bespoke tools to facilitate analysis are the flexibility they afford to jump around data to visualize them from different angles, to express ideas and concepts and link them to the data that prompted them, to explore a pattern or relationship seen in one part of the dataset and test whether it is also present elsewhere, and to “go down a blind alley” but not destroy anything else by doing so. Richards & Richards (1994), who themselves pioneered qualitative software (NUD*IST), point to the ability to achieve the shifts between data-level work and conceptual work more easily when using software. Figure 29.6 illustrates the inherently iterative and cyclical process of qualitative data analysis and the software tools that support the broad activities of integration, exploration, organization, reflection, and interrogation.

Silver & Lewins 2010 provide an overview of CAQDAS tools with respect to how they support these qualitative activities.³ Although much of the functionality could be relevant to many types of qualitative analyses, the precedence of each function and the way in which software tools are utilized to support them vary according to epistemological and methodological underpinnings and other project

characteristics. A grounded theory (GT) project, for example, may start off with an in-depth process of data familiarization, using annotation tools to mark and comment upon data segments that are of particular interest. This often acts as a pre-cursor to the inductive development of codes and the application of them to data segments as they are identified in the data. Although work may start inductively, it may become more deductive as emerging ideas are tested in further, theoretically sampled data. A more theoretically informed project may focus on the deductive development of a systematic coding schema that is applied to data files as a means of testing a pre-existent theory. In a less defined way, codes based on *sensitizing concepts* may still allow an inductive approach but help the researcher to be receptive to and explicit about acknowledged but subtle concepts. Blumer (1954) originated the term, stating that “whereas definitive concepts provide prescriptions of what to see, sensitizing concepts merely suggest directions along which to look.” More recently, Charmaz (2003)—in suggesting that “sensitizing concepts offer ways of seeing, organizing, and understanding experience; they are embedded in our disciplinary emphases and perspectival proclivities”—captures one of the essential elements that differentiates her constructivist use of GT from the earliest model of GT (Glaser

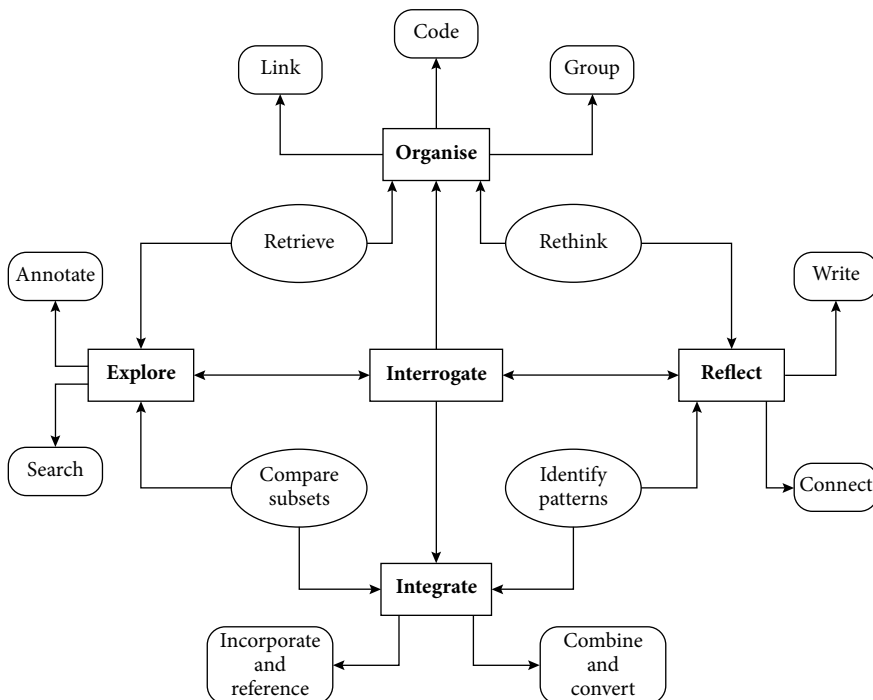


Figure 29.6 Qualitative Activities and Software Tools (adapted from Lewins & Silver, 2007).

& Strauss 1967). All these approaches might make different uses of annotating, coding, modeling, and query devices. Other projects using discourse or narrative approaches might be more interested in the role of language; if a CAQDAS application is used, such projects may utilize annotations and make more use of text- and phrase-searching tools to locate key passages of textual data and consider their usage within particular contexts (Silver & Fielding 2008). CAQDAS applications therefore can be seen as broadly practical toolboxes from which the researcher selects instruments that suit their individual methodological needs.

Since the early days of CAQDAS availability, many commentators have expressed concerns about the role of software. The following list addresses some of them:

- a critique concerning the over-emphasis of coding procedures (Coffey et al., 1996; Moss & Shank, 2002)
- suggestions that they fail to provide sufficient tools to facilitate conceptual work (Coffey et al., 1996, Lonkila, 1995);
- that the software acts as a “barrier” between the researcher and data (Fielding & Lee, 1998; Morison & Moir, 1998);
- software is too often used without sufficient epistemological or methodological grounding (Carvajal, 2002; Jones & Diment, 2010).

These concerns are valid in the sense that they highlight that software packages are merely tools and that the quality of analysis rests entirely with the researcher. Fast advances in CAQDAS technology support the argument that the first two of these concerns are less current as software has developed significantly in recent years. Indeed, as illustrated in Figure 29.6, coding tools are joined by other devices specifically designed to facilitate reflection. The other instruments within these packages can be central to the ways researchers bypass (or supplement) coding, to carry out conceptual or interpretive work at data level (Silver & Fielding, 2008). It is important, however, to air the critique in order to encourage openness to the varied use of available tools. Historically as the distribution and use of CAQDAS became more accepted, there was a perception amongst some ethnographers that “a taken for granted mode of data handling” was being established (Coffey et al., 1996). They accepted that, although such a norm was not inherent to the software development, there needed to be diversity in the approaches to ethnographic work. They

perceived that CAQDAS technology and the dominance of coding as a tool were a major influence leading to this convergence of method. Related to the usage of coding devices, Lonkila (1995) suggests that some techniques and elements of GT were being over-favored in the development and use of CAQDAS, while other approaches were being neglected in comparison. For instance, Coffey et al. (1996) suggest other types of software could support ethnographers as an alternative to the coding paradigm, stating, for example, that “hypertext software allows a reader to follow, and indeed to create, diverse pathways through a collection of textual materials.” Later in discussing the individual functions of CAQDAS packages more thoroughly, we discuss how ATLAS.ti in particular provides for this treatment of data in tandem with or to the exclusion of coding.

The third concern is largely a subjective evaluation that, when expressed, often transpires to be the result of a lack of detailed awareness about how these software packages are designed and operate. It always pays to work within your comfort zone, but some effort is also required to move beyond familiar ways of working in order to investigate the potential offered by new tools. Much work needs to be conducted behind a computer screen, which can indeed be experienced as less tactile than working with printouts. However, the organization of ideas about data—through the processes of marking, annotating, linking, and indexing or coding—subsequently allows faster and therefore closer access to significant parts of a dataset and their related aspects. In particular the ability to annotate, though it has much to add to code-based ways of working, provides non-code-based strategies the opportunity to link memos with data and easily improve not only the recall and retrieval of important insights but continued access to relevant material. If a researcher sees CAQDAS packages in terms of being distanced from data, then it is possible that encouragement is needed to see the simplicity and efficacy of such minimal tools as they inevitably and significantly improve closeness and access to data. In any case, the choice to use software need not occur to the exclusion of other ways of working. As we have commented previously, “often our most insightful thoughts occur at unexpected times, away from the computer... paper still has its place but the computer can provide you with the right bits of paper!” (Lewins & Silver, 2007)

The fourth concern is one that remains valid and always will. These tools are not designed as methods

of analysis, and informed teachers of them will emphasize the importance of approaching software methodology first. Indeed, early software manuals and the writings of pioneer software developers expressed their own concerns in this regard. Seidel (1991, 1993)⁴, for example, was concerned that computer technology would lead to a sacrifice of “resolution” in favor of “scope” leading to shallower exploration on larger and larger datasets. Jones & Diment (2010) suggest that in practice CAQDAS is often used as a “proxy for actual methods of research or analysis.” They link these ideas with Seale’s general observation that a decline in adhesion to philosophical foundations by researchers and the risk of trading quality for efficiency may lead to a general decline in methodological detail and poor research practice (1999). As software develops, the number and type of tools provided by individual packages is mushrooming as they seek to compete with one another and be taken up in new fields and sectors. The relationship between the quality of research and the tools used is very complex. Qualitative software itself can add to the process of analysis enhancing reliability and contact with the data. At its worst, especially as software becomes more complex, the software program can become a barrier, a distraction, and a delaying factor in simply getting on with the subtle processes of interpreting qualitative data. Coming before these two competing aspects is the quality of research design and the level of preparation, training, and familiarization with software that need to be properly factored in to research planning. In one sense, the software is just a tool; in another, the potential of the tool and how it might be used requires dovetailing with research design. Di Gregorio & Davidson (2008) set out to capture and illustrate some of these complex factors to show the links between project design and project set-up in software. Silver & Lewins (forthcoming) discuss research design in the context of qualitative software use as inherent in developing robust analyses and presenting them transparently. In terms of software and the increasing range of tools they provide, it is unlikely than any one research project will need to utilize all the tools available in the chosen program. Just as there is no one “best” or dominant approach to qualitative data analysis, there is no one “best” software product on the market to assure the quality of research. All work done inside or outside these packages should be of high quality, but how this is assured has to begin with project design. The criteria by which quality is measured vary across methodological approaches, academic disciplines,

and sectors. This is especially the case in the context of the increasing use of these packages outside of the academic social science settings in which they originated. Tools designed for one purpose can be manipulated for another, and in this way a strategy for analysis can be “created.” That said, it is also possible to manipulate the tools provided by individual software packages to follow a precise set of methodologically informed analytic procedures.

Data Storage and Access

Most CAQDAS packages enable direct work with a range of qualitative data formats, including most textual formats, audio, video, and graphic. Some also enable numeric information to be associated with qualitative data records (see the sections “Organizing Factual Features” and “Integrating Quantitative and Qualitative Data” later in this chapter).

CAQDAS packages as we conceptualize them act as containers for—or connectors to—all the material and data pertaining to a project. Programs differ in the way they store data, but all enable quick and easy access to them once associated with the software project. Most provide means of organizing data files using folder-type storage similar to standard computer filing systems. As well as incorporating materials into a central “database,” CAQDAS packages allow referencing to externally held information, such as literature files, websites, and other electronic media. This facilitates the management of the project as a whole, and referencing secondary information in this way is particularly useful when using these packages to facilitate a systematic literature review (see Bazeley (2008) for a discussion of this in relation to the use of NVivo).

As mentioned earlier, audiovisual data can be incorporated into a CAQDAS project alongside or independent from other data. Where methodology necessitates the generation of an associated written transcript, this can usually be done either within the CAQDAS package, or an existing transcript can be imported and synchronized to the media as a secondary step. The decision as to whether to analyze audiovisual data directly (without an associated textual transcript) or indirectly (via an associated textual transcript) is a significant one methodologically, analytically and practically (Silver & Patashnick, 2011).

In some packages (e.g., Transana and DRS), multiple media that represent different perspectives on the same phenomena can be synchronized for concurrent analysis. In Transana, the ability

to create and synchronize several different sets of notes or transcripts for one video (as illustrated in Figure 29.5, offers a multi-modal representation where different foci of attention (e.g., speech, body language, behavior) can be accommodated (Woods & Dempster, 2011; Halverson et al., 2012).

Exploration and Discovery: Marking, Annotating, and Searching

Discovering significant aspects in data through careful viewing and revisiting is likely to be the most thorough method of exploring any type of qualitative data. When analyzing primary data in the form of textual transcripts of interviews or group discussions, processes of exploration and discovery can be varied and frequently repeated. The first contact with data occurs when they are collected and recorded. The process of transcription is the second contact, and the way this is achieved comprises many analytic decisions. Transcriptions can be problematic. As discussed earlier, a transcript can only ever be a partial record of the interactions during data collection. If the transcription is subcontracted to a third party, other different subjective compromises are made about the translation of, for example, the interview into text, and importantly the analyst misses out on that contact with the data.

No technology can replace the value of reading or viewing the data on the way to analysis though this becomes less feasible with a very large corpus of data. However, the ability to explore data using *text searching* or *word frequency* tools can help to establish closer and more reliable contact with the data. Where the analyst did not transcribe or even collect the data, this provides rapid ways to familiarize and sensitize to some aspects of the data. However imperfect the end result and however difficult it is to achieve, having a textual representation of an interview or group discussion allows a level of fast access, which cannot be replicated if dealing for instance with sound or video files alone. Listening or watching such data in real time slows down every task performed on the data. Missing out the transcription stage may be analytically desirable for some data and for some projects, but hardly ever saves time. More importantly, if transcription is omitted purely because of practical constraints, this will almost certainly impact on the freedom to explore and interrogate data in different ways and from varying perspectives. Annotation tools come to the rescue again as we later discuss a compromise solution where, for instance, the full transcript is

not required but the ability to make notes alongside the multimedia is desirable.

Once data have been incorporated into a software project the iterative analytic process continues. Analytically interesting portions can be marked and annotated. Annotation tools allow the researcher to be reflexive and reflective not only about the data but about the conduct of the interview, to remark on aspects of the interview process as well as on statements by the respondent. Such devices in specialized software at first glance are no better than those provided in word processing applications (certain types of marking and annotation in, e.g., Microsoft Word, offer a way to keep ideas linked to but separated from text⁵). Functionality is enhanced within CAQDAS packages because, in parallel to such routine tools, the data files are always contained in one unit making them accessible and searchable across the whole or filtered parts of the dataset. Annotation tools in CAQDAS packages usually function in similar ways to footnoting or comment tools in word processing applications; they allow the researcher to log an insight or reminder about a specific data segment and anchor it to the relevant point in the data. Annotations can usually be retrieved and output in several ways, although flexibility in this regard does vary. If a researcher knows that much analytic work will be done in a CAQDAS package, care should be taken about doing too much early annotation in the word processing application (see note 5).

Some packages allow the marking or creation of data segments independently of their annotation or coding, and these also offer more flexibility in linking data segments to track associations (Coffey et al., 1996 Lewins & Silver, 2007; Silver & Fielding, 2008). Earlier we mentioned ATLAS.ti—which alongside the usual range of CAQDAS functions provides for effective hyperlinking between passages of text, enabling the creation of multiple trails through the data for conceptual reasons—to track processes or linkages. This means that the user can remain based at data level and navigate data in a non-linear way. Concerned about the dominance of the coding paradigm, Coffey et al. offered specific hypertext tools as a feasible alternative. ATLAS.ti combines both coding and flexible hypertext functions. The linked passages can be viewed in context or arranged in a visual map to represent a clear picture of how interactions are related. This is illustrated in Figure 29.7.

Non-textual forms of qualitative data, such as audio recordings and still or moving images, can also be marked and annotated in many CAQDAS

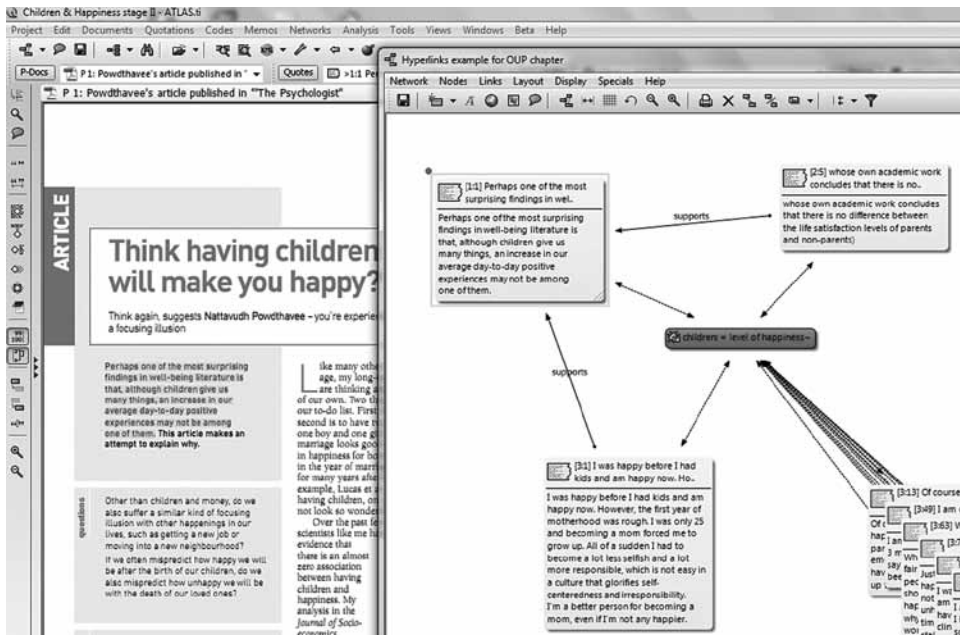


Figure 29.7 Hyperlinking “quotations” in ATLAS.ti and arranging them graphically.

packages. This may be achieved as an adjunct to developing associated written transcripts or as an alternative. When media are analyzed directly (i.e., without associated written transcripts), perhaps when the analytic focus is on non-verbal interactions, marking and annotation are likely to be the main instruments to record thoughts and analyses (Silver & Patashnick, 2011). The viewing of annotations alongside the image is particularly useful, and ATLAS.ti, NVivo, and MAXQDA manage this well in different ways, with NVivo maintaining a sense of the sequential “flow” of notes or the partial transcripts alongside the multimedia.

With very large corpus of textual data, exploration may be impracticable without using the software to locate words and phrases that signal particular topics of interest. Word frequency and text search tools are common and provide efficient and accurate means of locating keywords and phrases. The numeric overviews that can result provide a quantitative overview of content, which can be useful in identifying apparent salience, although repeated occurrence may be a reflection of the questioning style of an interviewer or another feature. Where word frequency and text-searching tools incorporate Key Word In Context (KWIC) functionality, they can be located quickly in their original source context. User-identified units of surrounding context may then be marked, annotated, linked, or coded. The differences, contexts,

and debate concerning the use of these tools are discussed at length by Fisher with reference to both the code-based and text retriever categories of software (1997, pp. 39–66).

Figure 29.8 shows the Phrase Finder tool in WordStat, here being used in conjunction with QDA Miner. The tool automatically searches data files for commonly occurring phrases, listing them in frequency order. Focussing on one phrase—in this example, “American dream”—illustrates the phrase in both summary format (upper part of window) and full source context (lower part of window). This hybrid view facilitates comparison and results may subsequently be coded.

The frequent revisiting of data is characteristic of qualitative analysis, and bespoke software both reflects and encourages this. Exploring data and logging insights through reading, searching, marking, and annotating provide important means by which to discover and stay in contact with areas of particular interest. Such work may go on to inform the focus of coding work and the development of a meaningful indexing system (see the section “Coding, Categorizing, Conceptualizing” later in this chapter). Equally, if the approach to analysis does not include code-based tasks, these exploration processes are central to how the researcher will improve access to data and analyze at the data level. In most cases, these tools are useful at all stages of work, applied early on to aid later recall of ideas and

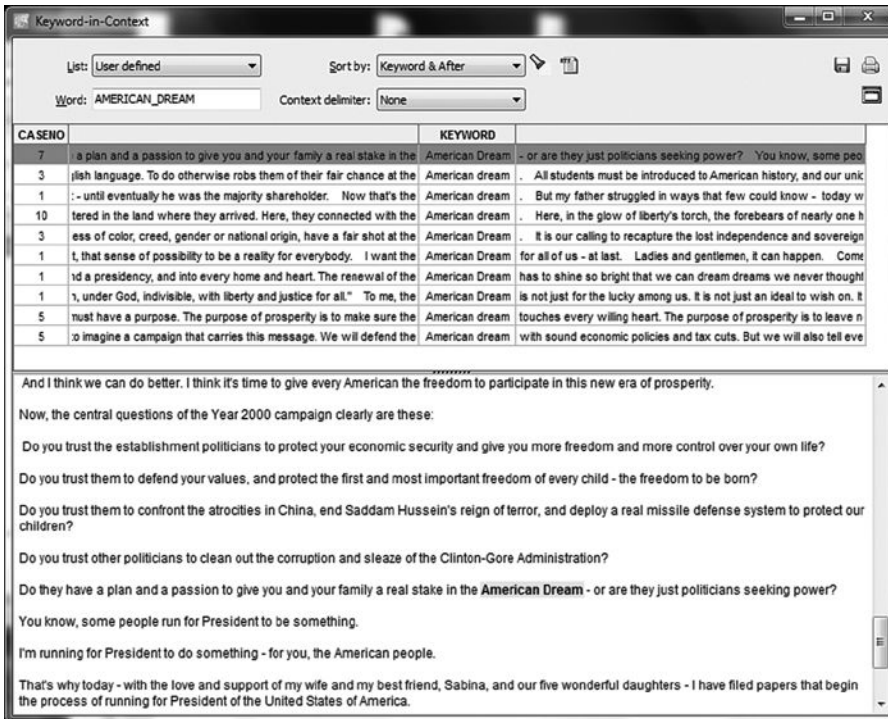


Figure 29.8 The Phrase Finder tool in QDA Miner + WordStat.

later for checking through the data and generally adding to the support for iterative work in software.

Auditing Process and Tracking Analysis

The ability to write analytical or procedural notes while working is provided by CAQDAS packages in a number of ways. As discussed earlier, embedding notes at particular points in the data is a form of analytic commentary. In addition are more centralized memo tools that constitute larger writing spaces that can be utilized for any type of note taking. Such tools are particularly relevant when viewing CAQDAS potential as the overall location of project management from research planning to data collection and onwards through to analysis and the production of findings.

Depending on the stage of work and the focus of the analysis, memos will take different forms and perform different roles. At the early stages, when undertaking a literature review and conceptualizing the research problem, memos can be used to write critical appraisals of literature and develop an account of the place of the study within the broader substantive and methodological field. Subsequently a separate memo may be created for each research question or hypothesis. During the stage of planning the research design, data-collection tools can be

stored as memos for reference purposes. Additional purposes of memos include the keeping of notes about individual data files or respondents, defining codes and the reasoning behind their creation, developing accounts behind concepts, the reasoning for themes and categories, keeping track of changes in analytic direction, action points for further discussion and so forth. Together with a centralized research diary where the day-to-day processes of the study are logged, memo writing is thus an important aspect of the management and continuity of analysis in all qualitative and mixed-methods projects.

Storing notes in CAQDAS packages rather than elsewhere is beneficial for two main reasons. First, doing so keeps all the information about the study in the same place, making it easier to find notes, to follow up on points of action, and to build on ideas. Second, notes kept as memos can be integrated with other aspects of work within CAQDAS projects. Packages vary in how this is enabled, but memos can often be linked to codes, data files, data segments, and other memos. The content of memos and annotations can sometimes also be coded. This enables the researchers' own thoughts to be treated as data in their own right, significantly aiding reflexivity. The researcher

is responsible for developing appropriate and systematic memo system structures and note-taking procedures, but getting into habit of noting down thoughts immediately means those ideas are not lost. This aids continuity in the process of analysis and improves transparency. Figure 29.9 shows MAXQDA's memo system in which all memos are listed, regardless of whether they originated linked to codes, data files or are free standing.

Memos in MAXQDA can be organized in various ways through this list and linked to the codes and data they are about or which prompted their creation. They can also be output in text, spreadsheet, or HTML format.

Coding, Categorizing, Conceptualizing

Coding in CAQDAS packages allows the linking of codes to passages of data. Technically it is a linking rather than a “cutting and pasting” process, as is typical when working with non-bespoke software tools. Retrieving coded data activates these links. It is usually possible to apply as many codes as required to the same, or overlapping, segments of data. As discussed, an analytic approach affects how codes are drawn out from and applied to data, but software supports explorative, inductive, or theoretically informed, deductive approaches equally well.

Figure 29.10 shows a coding schema structure in Transana (which refers to codes as keywords). In this example, a simple two-level coding schema has

been generated, although many packages allow the user to create many levels of subcodes.

Although coding strategy is not a software issue, the flexibility with which codes can be created, grouped, merged, and modified and the ease with which coding schema structures can be manipulated and refined will be improved by the use of software. For example, codes can be grouped or sorted to encompass more abstract concepts by combining issues and themes that seem to be related in some way. Figure 29.11 shows the Code Manager window in ATLAS.ti with groups of codes (called “Families”) listed on the left. The members of the highlighted family (in this example, “Children”) are listed to the right. Any number of Families can be created and the same code can belong to multiple families as they function as short-cut groupings. This facilitates reflective processes in that alternative collections of codes can be explored, which cut across the main structures of the coding schema.

In ATLAS.ti, Families are also functional in the sense that filtering to the members of a family focuses the project in that respect. In the case of code families, one result of such filtering is that the code margin view is restricted to show only the occurrence of codes belonging to the family to which the filter is currently set (as shown). Very often such combinations are useful, not just because they are part of a refinement process but because getting all the coded segments together in

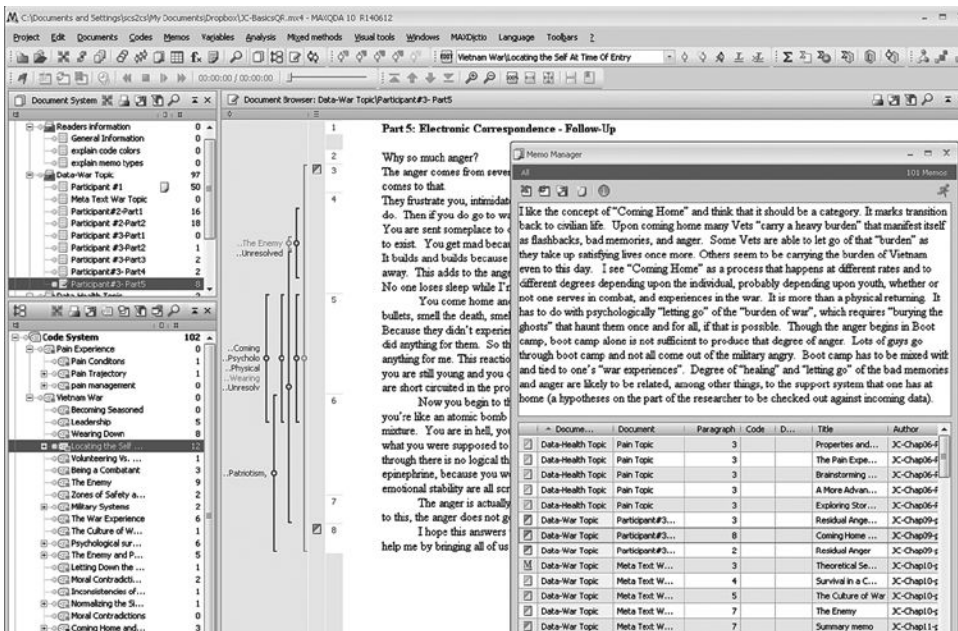


Figure 29.9 The centralized list of memos in MAXQDA.

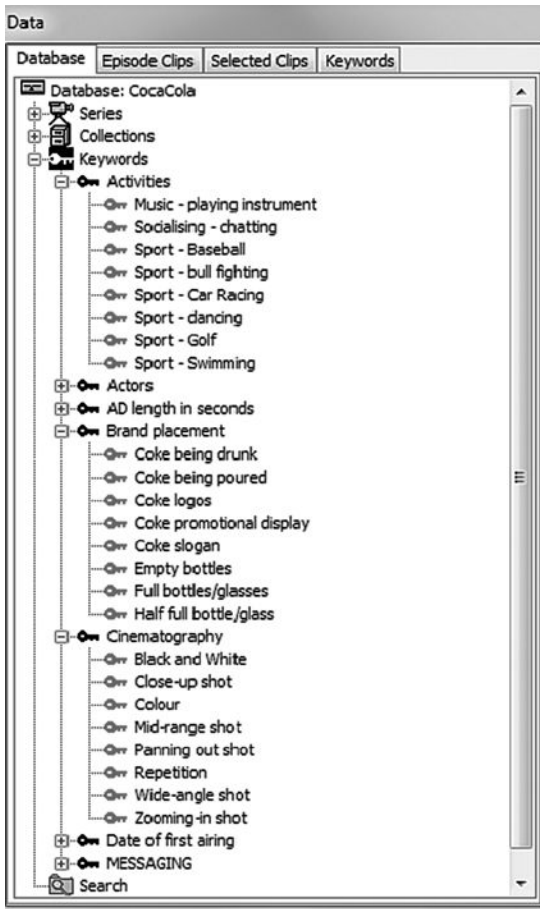


Figure 29.10 List of keywords used for coding in Transana.

different ways within the software, and in various output reports, can help to uncover new elements or dimensions in a topic.

CAQDAS packages enable the retrieval of passages of data based on the location of previously assigned codes. This facilitates the process of data reduction, allowing the examination of data segments related to an individual, or collection of, themes(s) across the whole data set. Some packages allow coded data segments to be viewed together within a modeling or networking tool, allowing data to be compared and recoded in a visual space akin to how this may be done in manual methods. This is illustrated in Figure 29.12 in the package Qualrus.

Retrieved data can be refined, recoded, output, and linked to memos and other related data segments. Usually there is good interactive contact from the coded segments back to the source context from which it came. Retrieval can be sent to output or report files that can be opened in other applications (e.g., a word processor and spreadsheet applications).

Organizing Factual Features

Factual features about data and respondents are important for identifying patterns and relationships and making comparisons across and within cases, subsets of data, and respondents. During the early stages of setting up a software

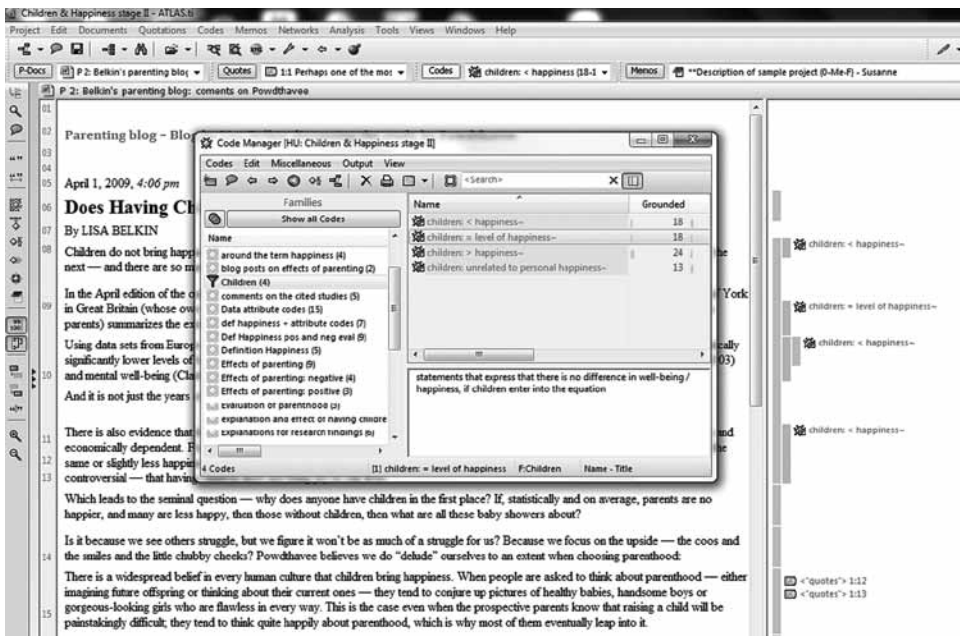


Figure 29.11 Grouping codes using code families in ATLAS.ti.

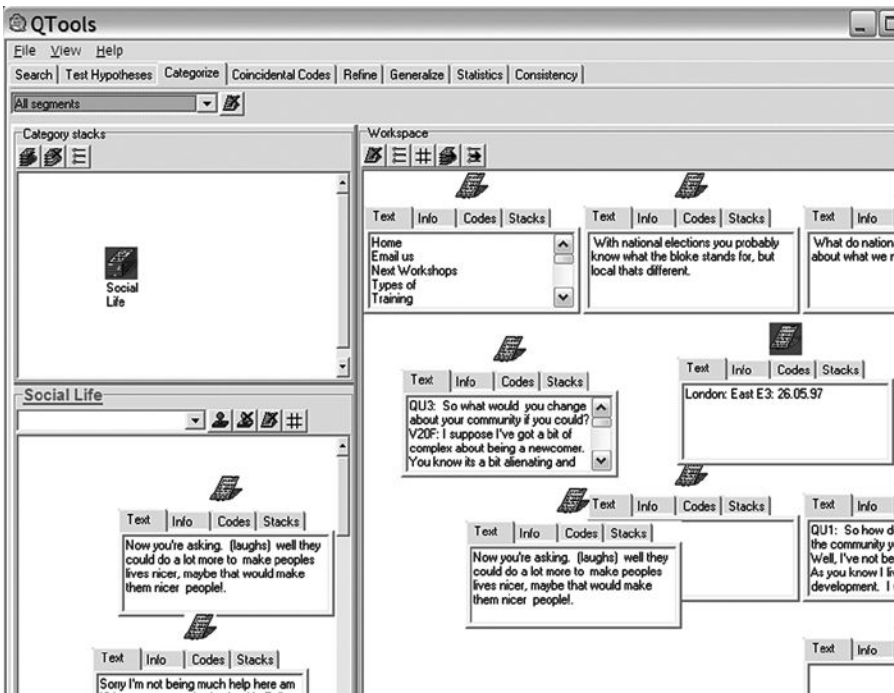


Figure 29.12 The categorization tool in Qualrus for visualizing and comparing coded data segments.

project, known characteristics such as respondents' socio-demographic variables (age, gender, income, etc.) or case descriptors (size, location, phase, etc.) that may have been sampled for are often key defining features. In this sense the structures (sometimes called attributes or variables) that embody factual organization in a software project may reflect the

research design. It therefore often makes sense to give thought to such basic structures at an early stage, although factual features pertaining to respondents and data can be assigned at any stage of work. Indeed, in some instances it may be more appropriate to organize factual features later on.

Name	Created On	Type	Created On	Modified On	Created By	Modified On
Person	04/06/2010 16:44				WWS	15/10/2012 15:16
Township	25/06/2010 20:09	Text	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16
Community	25/06/2010 20:09	Text	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16
Generations Down East	25/06/2010 20:09	Text	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16
Commercial Fishing	25/06/2010 20:09	Bookend	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16
Recreational Fishing	25/06/2010 20:09	Bookend	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16
Income Text to Resources	25/06/2010 20:09	Text	25/06/2010 20:09	15/10/2012 15:16	WWS	15/10/2012 15:16

	A: Township	B: Community	C: Generations Down East	D: Connecti...	E: Recreation...	F: Income Tax...	G: Derson ...	H: Age Group	I: Gender	J: Education
1: Paul	Shahs	Shahs	3 or none	Unassigned	Unassigned	Unassigned	Unassigned	Unassigned	Male	Unassigned
2: Betty	Shahs	Shahs	3 or none	Unassigned	Unassigned	Unassigned	Unassigned	Unassigned	Female	Unassigned
3: Helen	Shahs	Onway	3 or none	Unassigned	Unassigned	Unassigned	Unassigned	Unassigned	Female	Unassigned
4: Wilken	Shahs	Onway	1	Unassigned	Unassigned	Unassigned	Unassigned	Unassigned	Male	Unassigned
5: DE201	Shahs	none	none	No	No	no, never	undecided	60 - 69	Female	Completed high s
6: DE305	Shahs	Gloucester	1	No	No	no, never	about right	40 - 49	Male	Completed trade
7: DE306	Shahs	Bettle	3 or none	No	No	no, never	too fast	60 - 69	Male	Completed high s
8: DE308	Shahs	Onway	1	Yes	Yes	no, never	about right	50 - 59	Male	Some trade scho
9: DE313	Shahs	Shahs	3 or none	No	No	no, never	too fast	50 - 59	Male	Completed high s
10: DE314	Shahs	Bettle	1	Yes	Yes	yes	too fast	60 - 69	Male	Completed trade
11: DE318	Shahs	Gloucester	3 or none	Yes	Yes	yes	too fast	30 - 39	Female	Completed trade
12: DE320	Shahs	Shahs	none	No	Yes	no, never	about right	60 - 69	Male	Completed trade
13: DE325	Shahs	Gloucester	2	Yes	Yes	yes	too fast	60 - 69	Male	Completed trade
14: DE332	Shahs	Shahs	3 or none	Yes	No	yes	too fast	40 - 49	Male	Completed trade
15: DE333	Shahs	Onway	1	No	No	no, never	too fast	60 - 69	Male	Some undergradu
16: DE334	Shahs	Shahs	3 or none	No	Yes	no, never	too fast	60 - 69	Male	Completed high s
17: DE329	Shahs	Onway	none	No	No	yes	too fast	60 - 69	Male	Completed trade
18: DE342	Shahs	Bettle	none	No	Yes	no, never	too fast	40 - 49	Male	Some graduate s
19: DE345	Shahs	Onway	3 or none	No	No	no, never	too fast	40 - 49	Female	Some trade scho
20: DE346	Shahs	Bettle	1	No	Yes	no, never	about right	50 - 59	Male	Some undergradu
21: DE352	Shahs	Gloucester	3 or none	No	No	no, but use	too fast	60 - 69	Male	Some graduate s
22: DE353	Shahs	Onway	none	No	Yes	no, never	unconcerned	70 - 79	Male	Completed high s
23: DE354	Shahs	Onway	3 or none	No	Yes	no, never	about right	30 - 39	Female	Some trade scho
24: DE356	Shahs	Bettle	3 or none	Yes	Yes	yes	about right	40 - 49	Female	Completed trade

Figure 29.13 The NVivo classification sheet for recording factual characteristics about data and respondents.

Figure 29.13 shows a “classification sheet” in which factual characteristics pertaining to research respondents are stored. Attributes can be created manually within the software or imported from spreadsheet applications. In either case they form the basis from which later comparisons can be made through the use of queries.

Opinions differ about the need to create a clear organizational structure early on. Di Gregorio & Davidson (2008) illustrate how such structures can help to reflect individual project designs to frame later work. They refer to the notion of a “shell,” which has within its early constructs most of the potential for later interrogations across and within different groupings of data. Such constructs may be ideal starting points for some, but as some software applications become more and more complex, in practice, some aspects of the constructs can be difficult to achieve when a researcher is relatively unfamiliar with software, which can simply delay the researcher from getting on with more conceptual work.

It is important to have in mind, right at the outset of planning to use software in a research project, what the potential is and what the principles behind such organization of data are. Closely related to this “need to know” is a related “need to know” as early as possible about data preparation principles for the relevant software. Effective data formatting for, say, focus group data may allow particular organizational tasks to occur for each speaker. If appropriate, this might enable interrogation across individual members, or subsets of members (of the group discussions) to occur (see also the section “Auto coding of structures and content”).

Integrating Quantitative and Qualitative Data

In large-scale or mixed-methods projects, qualitative data may be part of a more complex research design. In such circumstances, the ability of some CAQDAS packages to combine and convert qualitative and quantitative data adds an additional dimension. This section concerns the integration of data themselves, focusing on three important aspects: the integration of qualitative and quantitative data pertaining to the same respondents; the analysis of open-ended responses to survey questions; and the conversion of qualitative data to numeric frequencies, potentially for subsequent statistical analysis. The later section “Quantitative Analyses and Representations” builds on this by discussing ways in which CAQDAS packages support

quantitative approaches to the analysis of qualitative and mixed data sets.

Where quantitative data are available that correspond directly to qualitative records, these different forms can be linked. This is usually done by means of importing numeric information from spreadsheets or statistical applications as variables assigned to qualitative records. For example, in a mixed-methods project, an initial stage of work may have involved conducting a questionnaire to survey a broad population. Subsequently, a small sample of questionnaire respondents may be followed up for interviews in order to investigate in more detail important issues identified by the survey. The statistical analysis of the larger survey provides the broad context while the qualitative interviews provide a more in-depth understanding. Being able to link the two enables explanations provided by the qualitative element to be crosschecked and interrogated with the quantitative.

The in-depth analysis of open-ended responses to survey questions is a relatively neglected area. Yet such responses are a valuable source of qualitative data that, when systematically analyzed, can add value to an otherwise wholly quantitative analysis. Many CAQDAS packages offer means of importing such survey data semi-automatically, and in some cases question-based coding is conducted as part of the data “import” process (Fielding, Fielding, & Hughes, 2012).

Whether conducting a mixed-methods project or not, generating a numeric overview of aspects of a software project can provide an alternative representation of the dataset and its analyses. Frequency information provides an overview of the status of coding, which facilitates the identification of relationships and comparison. Such numeric summary representations require careful interpretation, however, as they are based entirely on the way that data have been coded. Whilst software can provide means to check for consistency in the application of coding, this cannot be achieved automatically, as the researcher always remains in control of processes. For example, frequency counts relating to coding relate to the number of times a code has been applied, and this may not necessarily correspond to occurrence in terms of the number of times an issue is mentioned—unless coding has been consistently applied in a particular way. Where analytic output necessitates the ability, for example, to state that theme (a) has been mentioned (n) times by subgroup (y), codes must have been applied to a uniform unit of context throughout the dataset. The

sentence is likely the most meaningful unit for this type of analysis, but not all CAQDAS packages can automatically recognize the sentence. In any case, unless automatic coding devices (which come with their own limitations) are utilized, it will be up to the individual researcher to ensure each sentence (or other discernible unit of context) is consistently used as the minimum unit of coded data. This particular issue is relevant only when there is a need to quantify coding in such a manner, but it highlights just one of the inherent problems associated with “counting” what is essentially interpretive work in the data.

Notwithstanding such limitations, numeric code frequency information offers a general overview, which provides an additional means of reflecting upon data and analyses. In mixed-methods projects and those that consist of voluminous data corpora, this is likely of particular value. Frequency information may have its value in smaller-scale qualitative projects, though. Although it may not be appropriate to quote frequency information in the final reporting of small-scale projects, this information can be an invaluable part of the analytic process. Gaps and clusters illustrated by numeric overviews, in particular, represent aspects, which may not otherwise be reliably identified, that require further investigations. As such, quantitative representations of code frequencies in qualitative data not only

provide broad summaries but in the right context could add to the rigor of the analysis.

Figure 29.14 illustrates one example of a “mixed methods chart” in which factual characteristics of respondents (referred to in Dedoose as “descriptors”) are considered in tandem with qualitative codes. The bar chart view in the background shows the frequency of code application by the proportion of respondents from different ethnic origins. The Chart Selection Viewer on top enables the corresponding qualitative data to be viewed, along with any co-occurring codes for those data segments.

Auto-Coding of Structures and Content

Most CAQDAS packages provide means of identifying repeated structures and content within textual material, enabling (semi-)automatic coding based on their presence. Such tools are particularly valuable when working with large datasets or when conducting analyses that focus on narrative structures or the use of language. However, these tools may also have a place in smaller-scale projects, if not as a means of generating coding then as a means of initial data exploration and familiarization.

Inherently structured textual data come in many forms. For example, structured interview and open-ended survey datasets contain repeated sections across data files based on questions having been asked



Figure 29.14 Mixed-methods charts in Dedoose.

systematically in the same way. Focus-group discussions and field-note records of observations, however, are examples of data types that contain repeated structures within individual data files—in the former relating to speaker sections, in the latter to contexts, settings, or respondents under observation. However data are structured, in order to enable the (semi-) automatic coding of them within software, files must have been transcribed and formatted in a particular way. The formatting required to enable auto-coding for structures varies depending on the CAQDAS package being used. However, where this sort of auto-coding is appropriate, time taken on preparing data yields comparative benefits later on.

In terms of content, as discussed earlier in relation to exploration and discovery, locating individual or collections of keywords or phrases provides an accurate, reliable, and quick means of accessing textual material. Word frequency counts provide frequency—and sometimes statistical—information about the presence of individual words, often (although not always) providing KWIC functionality. Some packages additionally enable the searching for commonly occurring phrases. Text search query tools offer the researcher the ability to specify which strings to search for. These tools may be used as exploratory devices, as a means of becoming familiar with data, enabling quick access to keyword- or phrase-based content. Additionally, they may be used as coding tools for very large datasets where it is unfeasible to read through data for the purposes of coding. Although fast and accurate, these tools cannot be relied on as reliable in many forms of qualitative data, as topics may be discussed at length without the use of particular keywords.

Interrogating the Data Set

Much of the power of CAQDAS packages, particularly in terms of what they can offer above and beyond what is practically possible when working manually, lies in the potential of query tools. As discussed earlier, word and phrase searches can result in bodies of coded data, and in this way queries can play an important role in iterative processes. Boolean, proximity, and semantic query operators, however, enable patterns and relationships concerning the way data have been coded to be discovered or further interrogated. Such queries can be scoped to particular data files in order to focus an interrogation on a particular subset of data, or comparisons can be made according to identified factual features. Benefits of such queries range from the sheer ability to interrogate in varied and incremental ways (not

possible outside software) to the speed and reliability of their execution.

Packages vary in the range and complexity of queries that are enabled and, in projects with large volumes of data or complex research designs, query options may be an important basis upon which to choose between packages. Some packages like NVivo have an almost infinite number of query possibilities, though the price to be paid is the degree of complexity concerning the structures within the software. Other applications like MAXQDA offer sophisticated but simple ways to carry out queries though the range of queries may be smaller. The query tools offered by individual packages are related to the philosophical underpinnings of software development as well as software architecture. In general however, where queries are based on the occurrence of codes and their relative position within source data, results are reliant on the nature and consistency of previous coding. Figure 29.15 shows the Theory Tester tool in HyperRESEARCH. The “IF” and “THEN” panes at the base allow sets of rules to be developed, and the adding of new “Themes” to the cases that satisfy each rule. Combining Themes produces a final “Goal,” and the displayed report lists the cases in which the theory or hypothesis holds true.

Some packages provide tools specifically designed to optimize or investigate coding consistency—particularly in team projects where multiple researchers are involved in coding. There are also bespoke tools and add-ons for calculating inter-coder reliability, such as Coding Analysis Toolkit. Nevertheless, the “quality” of results—in terms both of their reliability and validity—is based on how data have previously been coded.

The approach adopted to data analysis will affect the queries that are usefully executed and their role in the analytic process. For example, projects informed by existing theoretical frameworks are likely to adopt a largely deductive approach to coding—at least initially—and therefore after coding will need to manipulate query tools in order to test the relevance of theories. Conversely, projects informed by derivatives of GT, which are likely to adopt a more inductive approach to initial coding, will use query tools to facilitate the *building* of theory. Projects concerned with the structures of discourse or the linguistic characteristics of texts will use query tools to identify those patterns and interrogate their frequency, sequence, and context. Mixed-methods projects will use query tools to integrate qualitative and quantitative analyses as

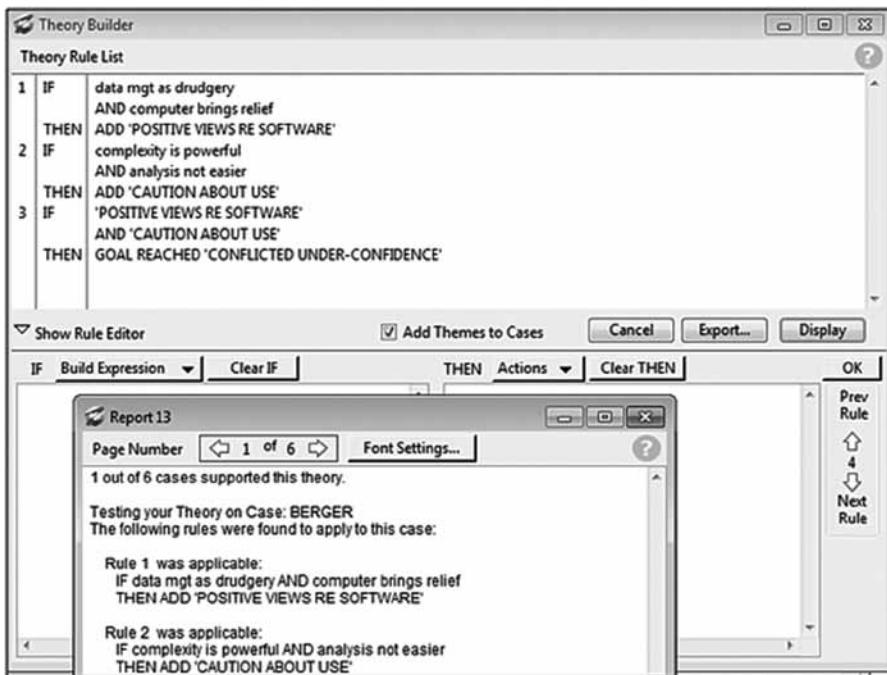


Figure 29.15 The theory tester tool in HyperRESEARCH for testing relationships.

a means of testing for convergent validation—for instance, whether the findings of different methods agree (Fielding, 2012).

The range of query tools available for data interrogation when using CAQDAS packages offers researchers the opportunity to explore alternative analytic perspectives. Some queries may not be appropriate for certain data types or analytic approaches, but their very existence challenges conventional ways of approaching qualitative projects. The mere use of these customized tools opens up many new possibilities for data interrogation, far beyond what is possible using manual methods.

Quantitative Analyses and Representations

As CAQDAS packages develop, many are adding functions that allow more quantitative representations of qualitative analyses and tools that facilitate quantitative analyses of qualitative data. As discussed earlier, the ability to make comparisons has long been at the heart of what these packages can offer the researcher, but more recent developments have emphasized triangulation of data and other mixed-methods dimensions. In terms of providing quantitative representations of qualitative analyses, matrices, charts, and text-based statistical visualizations are becoming more prevalent.

Querying co-occurrence in a comparative tabular format, for example, has become more

commonly available, as have the ways in which such results can be visualized and manipulated. Importantly for the qualitative dimension, most such tabular or chart visualizations are interactively connected to the relevant qualitative data behind them. The incorporation of quantitative content analysis or text-mining tools for interrogating large volumes of texts has also been incorporated into several CAQDAS packages—although to varying degrees of complexity and user-friendliness. Viewing the results of a given query in alternative ways may sometimes enable a pattern to be more readily seen, and as such these tools have a place in facilitating analysis. Figure 29.16 shows the results of a matrix query conducted in NVivo in which qualitative coding of several themes (represented as rows in the tabular view) have been interrogated according to factual features (in this example, whether respondents’ participate in commercial or recreational fishing).

The numeric information presented in the matrix can be varied on a number of bases (e.g., providing an overview of volume of coding as opposed to frequency of code application). In this example, the coded qualitative data are also shown, together with a bar chart visualization. Figure 29.17 shows the results of a similar type of query in QDA Miner. In this package, the tabular view is calculated automatically—without having to fill out a complicated

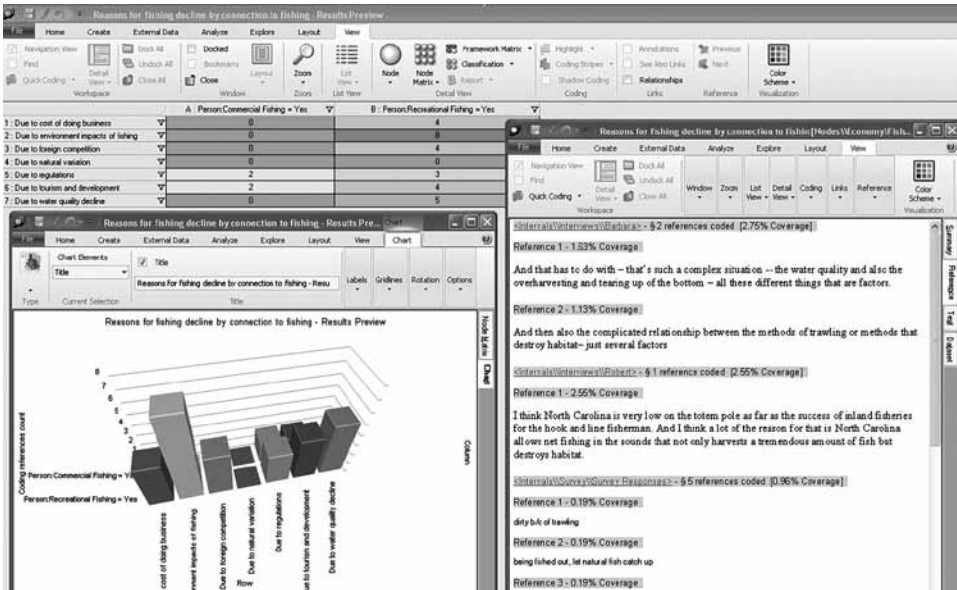


Figure 29.16 The matrix query tool in NVivo for making comparisons.

query dialog box as is the case with NVivo. The types of results that can be generated, are, however, similar.

Some packages, such as the QDA Miner/WordStat/SimStat suite of programs and the online tool Dedoose, were explicitly designed to fulfill the needs of varying styles of mixed-methods research. Others, such as MAXQDA, initially developed for the qualitative analysis of qualitative data, have more recently put a lot of developmental effort into providing additional quantitative analytic tools.

The incorporation of more quantitative analytic tools and visualizations offers the qualitative researcher a more extensive suite of tools from which to choose. Such freedom also brings with it the need for more value judgments to be made about them in specific contexts. Inappropriate uses of easily generated quasi-quantitative findings when the sample size does not support any such quantification is not helpful. Though some of these tools may not produce appropriate findings in the context of small-scale, in-depth qualitative analyses,

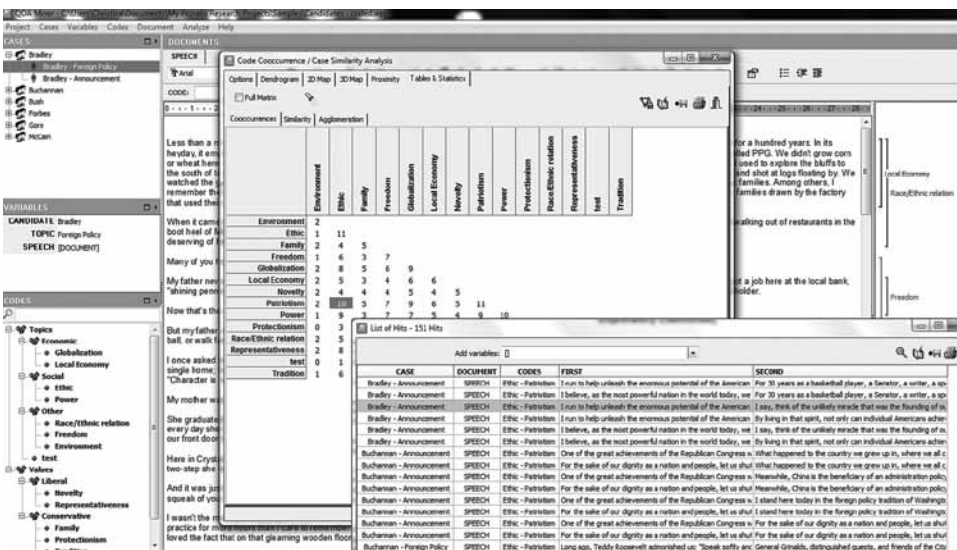


Figure 29.17 Code co-occurrence tools in QDA Miner for identifying patterns in coding.

their availability opens up the ways in which traditional qualitative researchers can assess aspects of data not least because these tools tend to offer an opportunity to step right back from the data to see a bird's-eye view. For instance it may only be at those moments that telling gaps can be seen. In both qualitative and quantitative analysis, what is absent can be as important as what is present.

Making Connections

The ability to step back from data and coding to reflect on ideas and relationships is a key aspect of analytic process in qualitative analyses. Most CAQDAS packages include mapping, modeling, or networking tools designed to facilitate these processes. Abstract maps can be generated that reflect ideas independent of data and ahead of analysis. Alternatively, codes, categories, and data can be visualized within maps in order to investigate connections and visually represent theories. These *maps*, *models*, or *networks* can be generated at any time of the process. In some projects, this may occur at an early stage to represent the theoretical framework or to visually represent hypotheses, in other projects at a later stage as a means of illustrating patterns and relationships identified in the data or to track interpretations amongst sub-sets of data. Figure 29.18 shows a map generated in MAXQDA. Maps may be completely abstract from other elements of the software project, or contain direct links to the codes, data files and coded segments to which

they refer. From MAXQDA maps, coded segments can be retrieved and outputted as well as visualized graphically.

There are some limitations to the flexibility of these integrated mapping tools in comparison to bespoke mapping software such as MindManager, Inspiration, and MindNode, which are specifically developed to draw maps and have sophisticated graphic representation tools. They may additionally be available as apps for iPhones or Android devices. Another program, Tinderbox, uses hypertext and mapping to allow the researcher to plot a summarized map (e.g., of notes about a work of literature or an aspect of society or work) and then create specific relationships between elements in the map. However, these programs do not include the other features of CAQDAS packages. Although the mapping facilities are less sophisticated, the main benefit of using mapping, modeling, or networking tools within CAQDAS packages lies in their dynamism and interactivity with source data. They provide means of abstracting from source data while maintaining direct links with them. This is important in terms of researchers' ability to check source data for the evidence which underpins the connections created within maps.

Reporting and Outputting

CAQDAS packages provide various ways of reporting about data and analyses and of generating output. At the basic level, textual material

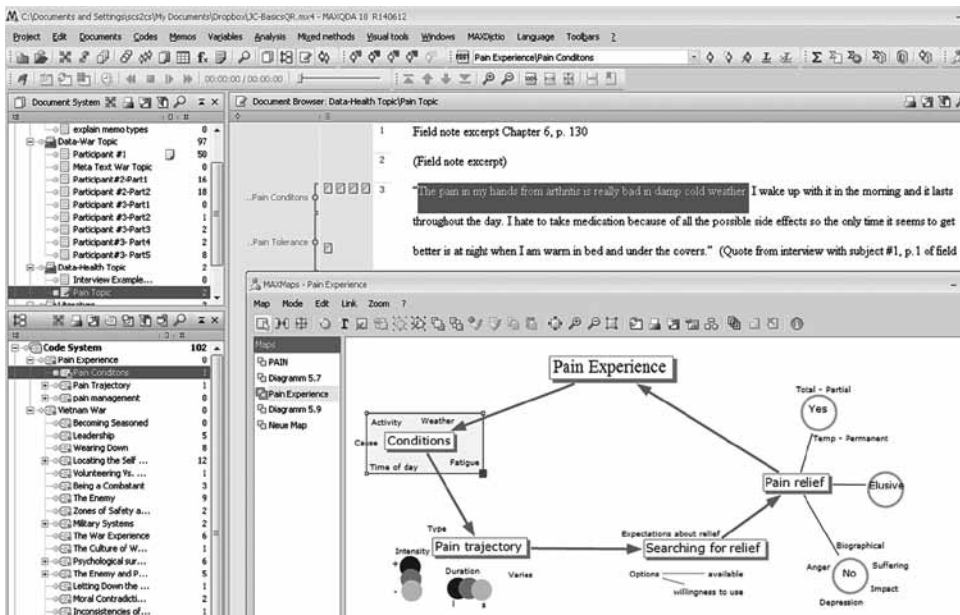


Figure 29.18 Visualizing connections in a map view using MAXQDA.

(e.g., coded passages of transcripts) can always be easily copied and pasted into other applications. More formal reports can also be generated that usually provide additional information about coded data—for example relating to frequency of code application. Any matrices or lists can be outputted and viewed in spreadsheet or statistical applications for further manipulation where appropriate.

There is less flexibility in outputting coded audiovisual data as most CAQDAS packages do not include audiovisual editing capabilities (see Silver & Patashnick, 2011 for more discussion of this). Similarly, diagrams generated in mapping or networking tools as means of visualizing connections between concepts, although possible to output, can usually only be done so as static objects—in other words, there is no possibility to further edit them outside of the CAQDAS package.

Nevertheless, reporting and outputting are important aspects of most project work allowing reflection, the sharing of work as well as contributing to the writing of final research reports. Reporting on progress forms part of the iterative process of analysis, providing snapshots of all types of interrogation and visualization in order to create explanations and build theory.

Collaboration Using CAQDAS Packages

As well as the specific analytic functionality already discussed, there has been a concurrent advance amongst CAQDAS packages in facilitating collaborative working. Some packages have long provided multiuser versions that allow several researchers to work on the same data set at the same time. Others have facilitated the sharing and combining of work through project merging tools. The recent increase in availability of server versions and sophisticated, web-based tools offers wider choices for team-based research, which requires the ability for concurrent working amongst geographically dispersed researchers. The way CAQDAS packages work in this respect differs. Therefore if concurrent working is an important dimension required by a team project, it is worth investigating the options before settling on a package.

The practice of conducting team-based analysis using software is not always straightforward and requires careful planning and preparation. Team dynamics and the complexity of collaborative projects vary. Being realistic about the role of software and the contributions of individual researchers to the analytic process is important in developing an efficient and effective strategy. This is particularly

important when splitting work into separate software projects with the intention of subsequently *merging* work. Replicating basic structures across separate projects, for example, is important in ensuring smooth merging of the constituent parts of projects. Some cleaning up of the resulting merged project is often required to manually merge aspects that are named inconsistently or need to be gathered together. The CAQDAS Networking Project website provides some general guidance on planning for working with qualitative software in team situations and some specific protocols developed for individual packages.

Limitations and Constraints

CAQDAS packages are powerful tools for managing qualitative and mixed-methods research projects and as discussed earlier, they provide an increasingly broad and sophisticated suite of tools to support researchers working in a variety of disciplinary, methodological, and practical contexts. However, they are not universally used, and their critics remain despite developments. For those that have successfully used software, it may seem inconceivable to return to non-computerized manual methods. Increasingly we are witnessing a generation of students and researchers coming through who take it for granted that software facilitates qualitative and mixed-methods research—accustomed as they are to the digital world. But for those with well-established manual methods, the idea of learning a different way of working can seem daunting. Some are skeptical about software functionality, perhaps concerned about too prominent a role of technology at the expense of human interpretation. Others may just not like the idea of working behind a computer screen, afraid of losing the “tactile” connection with their data. Although we argue that using bespoke CAQDAS packages to facilitate analysis has a number of important benefits, there are also a number of aspects relating to their use about which it is right to be cautious. Here we discuss these in terms of risks related to an underestimation of the time involved, an overreliance on software, the increasing proliferation of tools, and the potential for confused analytic strategies.

Underestimation of Time Involved

Some of the problems of using computers result from a lack of adequate hardware, money, time to familiarize, and local support. The most common constraint is time, especially since many of the problems associated with effective use of

qualitative software are linked to the challenges of qualitative data analysis itself. In many projects, the data-collection and data-preparation phases spread into the time allotted for analysis. Aggravating these difficulties is the lack of formally prescribed techniques for analyzing data as a result of the variety of analytic paradigms and the personal nature of the relationship between data and the researcher. These issues, when added to the need to choose and become familiar with a program, combine to make researchers' challenges more complex. It is important to be realistic about how using software will facilitate your work, given that software familiarization often occurs in tandem with analysis, and it is important to feel secure about using software in order to work with it effectively and efficiently. Many packages are "front-loaded" in terms of the amount of preparation and planning that is usefully done before commencing work with data. As such it is these stages that can be the particularly time consuming. It is therefore beneficial to become familiar with software as soon as possible in order that serious work, when it happens in the software, feels comfortable. This is particularly important if the software is to be utilized as a tool to manage the whole research project. The CAQDAS Networking Project and the Online QDA site are two sources of support that can be helpful (see "Resources" later in this chapter).

Overreliance on Software

Expectations concerning the role of CAQDAS packages are closely related to general issues about the time involved in planning for software use. Here we highlight cautions concerning viewing software as offering a shortcut to analysis and a sense that "everything needs to happen inside the software."

SHORTCUT TO ANALYSIS

Expectations amongst new users of CAQDAS packages vary, with many being unsure what to expect. Those with experience of manual analysis are often principally looking for more systematic and practicable ways of handling large volumes of data. The immediacy of data access afforded by software is therefore seen as one of the main benefits. Yet it is important not to view software as offering a shortcut to analysis. Software use will neither necessarily speed up work (particularly during the early stages), nor will it necessarily result in "better quality" analysis. Exploration tools such as word and phrase-based searches, for example, may provide reliable and instantaneous access to seemingly relevant points in the data, but they both capture

irrelevancies and miss salience. There are many ways in which the use of CAQDAS packages may change the way analysts work, not least in terms of data exploration. If coding is the main vehicle to conceptual organization of the data, the responsibility for valid, meaningful, and consistent coding rests with the researcher, and that process cannot be cut short or automated. Even in packages that include special features designed to aid researchers in the prediction of relevant codes—such as Qualrus—ultimately, there are no shortcuts to qualitative coding.

SLAVE TO THE COMPUTER

Not all analysis can or should happen within a software package. This may be obvious, but it is important to make the point since so much of our lives revolve around the expectation that computers are part of the way we work. Researchers experienced in undertaking analysis manually usually seek ways of outputting coded data in order to view them outside of the software and away from the computer, as well as means of replicating their established practices within the tool. As mentioned, all CAQDAS packages discussed here provide a range of ways of generating output reports. The benefit is that, when using software, the user can easily control which parts of the dataset to output and print. There is no harm—in fact, it is often beneficial—in reflecting upon progress by printing coded information and considering the next analytic steps away from the computer. Highlighter pens can even be used! Indeed, reconceptualizing a body of coded data is often the type of work that is easier away from the software. Some packages even include special functions for inputting hard-copy (re)coding as a clerical task. Coding schemes can become unwieldy and repetitive, so a printout with frequencies allows the researcher to contemplate the rationalization of the codes in slow time. Similar codes can be highlighted and arrows drawn to show what needs to be moved and where and which need to be deleted or given less priority in the way codes are listed.

The discussion of core aspects of qualitative software here emphasizes how CAQDAS packages have added to researchers' flexibility and ability to revisit and change ideas. When working inside the software, the immediacy of data access is what underpins all of the advantages of managing data and their analysis.

Increasing Complexity and Proliferation of Tools

As discussed earlier, many CAQDAS packages were initially developed by academics who sought

computational solutions to handling the “messy” processes of qualitative data analysis. Some packages still remain within, or closely associated with, academic institutions. Others have become solely commercial enterprises. Regardless of continued academic links, the commercial pressures associated with software development are important to bear in mind. The majority of packages discussed in this chapter are commercially available and are in competition with one another. This inevitably affects the way in which they are developed and the range of tools provided within them. The result is the gradual convergence of functionality. The leading packages, for example, are closer now in terms of the tools they offer than ever before. In terms of choosing between packages, this means that there are fewer criteria upon which to base a decision. In many senses, it is now the *emphasis* of a software package or the underlying architecture—which affects what is possible later—that determines the most important differences.

This raises the question as to whether commercial pressures on qualitative software development are resulting in more refined qualitative tools or a dissolution of them. The “race” to provide the next “fancy” tool or even more complex mixed-methods support might be attractive commercially, but what is the effect on the analytic requirements of individual (or teams of) researchers? There is a significant danger involved in the attempt to “be all things to all researchers.” We have touched on the critique concerning the need to maintain quality of qualitative research in the context of using CAQDAS and mentioned that software can enable tasks not previously possible. We suggest that, used properly in the context of well-designed research, such tools add to the potential for reliable and robust research findings. Such packages need to remain comprehensible for these benefits to be realized. The expansion of market appeal is an understandable aim amongst software developers. Occasionally, though, the routes taken to provide enhanced support for a wider range of data types and/or analytic approaches risk creating overly complex products. Such products, though powerful and sophisticated, can become inaccessible without extensive support that goes beyond mere training to the need for subcontracted project “set-up” or analysis. This might be appropriate where such expense is within reach for, say, commercial researchers. For most academic researchers, however, this is not practical or, in the case of students, allowed. Consequently there is a risk that the complexity of software itself might begin to impact the quality of work whether or not research design is good. Most

leading developers of qualitative software work hard to retain intuitive, architectural simplicity. However, there have been recent failures in this respect. The trend to enhance mixed-methods support is a useful continuing development but should not be at the expense of most researchers’ access to the qualitative tools that were the starting points and the priority for such packages.

In actuality, competition between products may have created regional leaders, but it has not resulted in a global market leader. Researchers make decisions concerning qualitative software based on a number of issues and—in contrast to quantitative analytic software—no one package dominates. While the three or four most well-known CAQDAS packages grapple for market-share, others do not become overtly involved in the same contest. Perhaps this is partly as a result of financial capacity, but it is also as a result of allegiance to academic beginnings or conceptualizations of analytic purpose. Transana, for example, remains an open-source, low-cost software. It has always placed itself explicitly as an audiovisual transcription and analysis software, and the development of its tools adheres to a particular approach to analysis of visual data. QDA Miner was developed to “add on” a qualitative dimension to a suite of packages that clearly espoused a quantitative approach to qualitative data. This background places it within the mixed-methods paradigm and has an effect on the way its tools are developed. Qualrus sought to attend to the “artificial intelligence” possibilities of software to contribute to both the clerical and “thinking” tasks of coding, based on a number of criteria, among them patterns in the data and language-based ideas about analysis.

We make the point that neither sophistication in software capability nor variety in a vast range of available tools are meaningful bases upon which to choose between CAQDAS packages. No one qualitative (or mixed-methods) research project is likely to use all the tools available by a given software package. Therefore being “seduced” by seeming sophistication or commercial marketing is likely to be a false economy.

The Potential for Confused Analytic Strategy

Closely related to the proliferation of software tools is the relationship between technology and methodology. No qualitative software package provides or automatically creates a “method” of analysis. Analytically informed training emphasizes the importance of encountering software “methodology aware.” In fact

methodologies have always adapted to and been affected by technological development, so the concern is more that a confused sense of analytic direction is *always* a liability, whether software is being used at all, let alone which package is being utilized. The earlier discussion of the proliferation of software tools is illustrative of the fact that tools within CAQDAS packages are best *picked* and *manipulated* according to the methodological, analytical, and practical needs of a given research project. In discussing the mixing of methods, Mason (2002) accepts that multiple methods may be used to address research questions but that “a researcher must think strategically about the integration of multiple methods rather than piecing them together in an ad hoc and eclectic way.” To an extent, the tools in CAQDAS applications might encourage a similar ad hoc use of devices. It is important to look at the benefits of every tool used in the context of clear analytic direction.

The need to be clear about direction and the debates concerning their methodology is particularly important to novice researchers. In that context we have mentioned the debate concerning the dominance of code-based approaches to qualitative analysis reinforced by mainstream bespoke tools such as CAQDAS. There are two main responses to such contentions: first, many, if not all, CAQDAS packages as discussed provide alternative means of approaching analysis (Lewins & Silver, 2007; Silver & Lewins 2014; Silver & Fielding, 2008); and second, “codes” and “coding” need not be utilized in *traditional* or thematic ways if used within these applications. Many researchers will have to use the software package that is available locally so the subtle choice of tools will not be about which package to use but which devices or instruments to use within the default application. The researcher needs to evaluate what seems to be a dominant aspect of any application and adapt that aspect to a specific analytic purpose or not use it at all. With the creative use of technology comes the opportunity for increasing the diverse usefulness of such software applications.

Summary

This chapter has outlined and discussed various software packages available to facilitate the analysis of qualitative and some types of mixed-methods research projects. Also discussed were tools that may aid the many stages of the research process, from data collection through reviewing literature, research design, transcription, analysis, and reporting. In any one research project, it may be appropriate to use several different tools. Equally, in any one

research project not all the tools available in a particular software package will be appropriate to use.

The tools available are extensive and varied, and some have not been designed specifically for research purposes. Those developed to support qualitative research tend to have the most appropriate and well-developed tools for that purpose, although each has its own set of advantages, levels of intuitiveness, and usability. While it is good advice to come to software with methodology in mind first, in using these tools researchers quickly realize that certain tasks are simply more practicable with the support of software. Despite the sophisticated range of tools available in CAQDAS packages, it is often the basic aspects of data management and access that afford the greatest benefits—especially for researchers practiced in the often cumbersome and time-consuming process of data analysis using more manual craft traditions. The ability to handle larger volumes and varied forms of data and to enable closer collaborative work opens up the possibilities for more research contexts. Though “thinking” remains largely the responsibility of the researcher, it is the speed with which data, coded or uncoded, can be accessed, searched, reviewed, recoded, or annotated that in practice frees up researchers’ time to do the thinking—to delve more deeply into data, make comparisons, and interrogate patterns and relationships to a level that may not be conceivable otherwise.

The cautions raised in this chapter are important to keep in mind. Being able to familiarize and work confidently with software while exploring ideas in data is key to the effective utilization of software tools. That said, using software can facilitate and enhance the processes of analysis and offer means of systematizing procedures. The range of functionality contributes to the ability to integrate varied data sources, to conduct more rigorous and robust analyses, and at the same time to be more ambitious and more transparent about the processes followed and enabled by software.

Notes

1. Software available at multiple retail outlets is generally not included in the resource section.
2. NVivo 10, has enabled direct data import from Evernote.
3. Silver & Lewins (2014) present and discuss a more complex version of this model of qualitative activities and software tools, relating them explicitly to aspects of research design and analytic processes, and using case-study examples to illustrate possible use of software tools in different methodological contexts.
4. Seidel was the original developer of The Ethnograph software, created initially to support his own ethnographic analysis.

5. The use of certain word processing functions are not usually compatible with CAQDAS packages. For example, the balloon comments common to Microsoft Word will be lost after the data have been imported into the CAQDAS application and line numbering devices in Word, often used by those who are thinking of “line by line” analysis, usually will not be directly carried through into the CAQDAS application.

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Resources

DISCLAIMERS & RECOMMENDATIONS:
We do not security or quality check all these applications or accept responsibility for installation or running errors. Users should make their own inquiries and check for precise platform and operating system information. Try to get an idea of how well each package is supported, when it was last updated, and an idea of the user base. Browse user forums for operating issues, reviews, and snags. Freeware and shareware are variably supported. We can reliably say that the commercial CAQDAS packages listed are well supported, frequently updated, and widely used. We have included just one URL to access each software, but in some cases this might be the URL of the distributor. There may be other distributors to investigate.

Software application	Basis	Summary information and URL
DATA PROCESSING		
Audacity	Free/open source	Cross-platform: AUDIO PROCESSING, records live sound, edit, splice, improve sound quality, convert to digital formats, etc. http://audacity.sourceforge.net/
DocPoint Personal,	Purchase	PC: DOCUMENT MANAGEMENT solution for the home and small office user. http://www.docpoint.biz/docpoint_personal.html
eDocXL,	Purchase	PC: DOCUMENT MANAGEMENT Document capture, PDF creation and conversion, includes OCR ¹ . http://download.cnet.com/eDocXL-Pro-Desktop/3000-10743_4-10431873.html
Epiware	Free/open-source or purchase pro edition	Linux (free version): DOCUMENT MANAGEMENT enabling users to collaborate on projects, share documents, create schedules, and manage tasks. http://www.epiware.com/
Evernote	Free basic or rent Premium edition	PC, Apple, OS X, and most mobile devices: DOCUMENT MANAGEMENT—Manage and write notes, web clips, files, and images—and share between devices http://evernote.com/evernote/
InfoRapid Cardfile, Ingo Straub	Shareware	PC: DOCUMENT MANAGEMENT—electronic database system to manage text and image documents. http://www.inforapid.de/html/cardfile.htm
Kordil EDMS	Free open-source	WEB 2.0 (no platform issues): DOCUMENT MANAGEMENT—collects all the files of collaborators under a single meeting point. http://www.kordil.net/
F4/F5	Free or purchase higher edition	PC or Mac: Keyboard- (or optional foot pedal-) controlled aid to digital transcription of audio/visual files. http://www.audiotranskription.de/english/f4.htm

(continued)

Software application	Basis	Summary information and URL
Express Scribe , NCH Software	Freeware or purchase pro edition	PC or Mac: TRANSCRIPTION—Foot pedal–controlled digital transcription audio player software to assist the synched transcription of audio recordings. http://www.nch.com.au/scribe/index.html
Transcriber AG	Freeware	PC Mac OS X, and Linux: TRANSCRIPTION and annotation of speech signals http://transag.sourceforge.net/
HyperTRANSCRIBE , Researchware (see also HyperRESEARCH)	Purchase	PC and OS X: TRANSCRIPTION of audio and video files that can be bundled with HyperRESEARCH http://www.researchware.com/products/hypertranscribe.html
WITH ANALYTIC TOOLS		
ATLAS.ti , Scientific Software	Purchase	PC: CAQDAS category application, qualitative textual and multimedia analysis, code and retrieve, annotate, organize, map, and interrogate.
Coding Analysis Toolkit	Free	Web 2.0: CAQDAS category application, textual data, with additional tools for calculating inter-coder reliability. ATLAS.ti coded datasets can be uploaded for calculating coder statistics.
Concordance	Purchase	PC: CONCORDANCES—instant lists of words in immediate context texts of any size (wordlists, frequency) Web Concordances—one-click turn concordance into linked HTML files, ready for publishing on the Web. www.concordancesoftware.co.uk/
Dedoose	Rent	Web 2.0: MIXED-METHODS application (no platform issues) CAQDAS category application, mixed methods, textual, and multimedia analysis.
Digital Replay System	Free open- source	PC: ANALYSIS application developed at the University of Nottingham. Large heterogeneous datasets, synchronized playback of related multimedia file types. http://sourceforge.net/projects/thedrs/
HyperRESEARCH , Researchware	Purchase	PC and Apple OS X: CAQDAS category application, qualitative textual and multimedia analysis, case-based, code and retrieve, annotate, and interrogate.
MAXQDA , Verbi Software	Purchase	PC: CAQDAS category application, qualitative textual and multimedia analysis, code and retrieve, annotate, organize, map, interrogate http://www.maxqda.com
QDA Miner (with WordStat) , Provalis	Purchase	PC: CAQDAS category application, qualitative textual and multimedia analysis, code & retrieve, annotate, organize, map, interrogate, text mining, and stats.
NVivo & NVivo “Server” version, QSR	Purchase	PC: CAQDAS category application, qualitative textual and multimedia analysis, code & retrieve, annotate, organize, map, and interrogate.
Transana and Transana MU University of Wisconsin-Madison, Center for Education Research	Open-source purchase	PC or Apple OS X: CAQDAS category application transcribe and analyze digital video or audio data. Create multiple transcripts synched with same file. http://www.transana.org/

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TAMS Analyser , Matthew Weinstein	Free open-source	Unix, Linux, Apple OS X: ANALYSIS—Ethnographic and discourse research and text analysis mark-up system to assign codes in texts (web pages, interviews, field notes). Designed for http://tamsys.sourceforge.net/
TextWorld , Alan Reed	Free	PC and Mac: CONCORDANCES—create word lists and search natural language text files for words, phrases, and patterns. Multiple languages. http://www.textworld.com/scp
BIBLIOGRAPHIC		
Software application	Basis	Summary information and URL
RefWorks		Cross platform: BIBLIOGRAPHIC—online research manager, writing tool, sharing data http://www.refworks.com/
Endnote and Endnote Web	Purchase	PC, Apple OS X: BIBLIOGRAPHIC—searching, organizing, and sharing writing, online or not. http://endnote.com/
Zotero	Free	Web 2.0 (no platform issues): BIBLIOGRAPHIC—collect, organize, cite, tag, make notes, and share sources. http://www.zotero.org/
MAPPING		
CmapTools , IHMC (not CMAP)	Free	PC: MAPPING—construct, navigate, share, and criticize knowledge models represented as a concept map http://cmap.ihmc.us/
ConceptMap , North West Missouri State University	Free	Web 2.0: MAPPING http://www.nwmissouri.edu/library/courses/research/conceptMap.html
iMindMap , ThinkBuzan	Free basic version or purchase plus versions	Mac, Apple iOS: MAPPING from inventor of mind maps—mapping, notes, organizing, and illustrating ideas. http://www.thinkbuzan.com/
Inspiration , Inspiration Software Inc.	Purchase	PC and Apple: MAPPING—visual learning strategies using images and mapping helps students organize and analyze information, integrate new knowledge, and think critically. Graphic organizers, diagrams, and outlines are just some strategies of visual learning. http://www.inspiration.com/
MindManager , Mindjet	Purchase	PC and mobile devices: MAPPING—interactive information maps to illustrate the big picture, and contain all relevant details in one single, centralised view. http://www.mindjet.com/
MindNode	Purchase	Apple mobile devices: MAPPING and document sharing http://mindnode.com/#!/mac
XMind	Free open-source or purchase	PC: MAPPING, charting, brainstorming http://www.xmind.net/
MULTIMEDIA		
Camtasia		PC and Apple: Video screen capture tool to film an on-screen process. http://www.camtasia.com/camtasia/index.htm
VideoDub	Free	PC: Video editing—delete unwanted parts from video files without re-encoding, and program preserves original quality of the input video files. http://www.dvdvideosoftware.com/products/dvd/free-video-dub.htm

(continued)

Software application	Basis	Summary information and URL
Corel Video Studio	Purchase	PC: HD video creation, video editing, advanced visual effects with complete screen recording and sharing
Interact, Mangold	Purchase	PC: Simultaneous video analysis, live observation, and analysis http://www.mangold-international.com/software/interact/
The Observer, Noldus	Purchase	PC: Modular tools for behavioural coding and detailed analysis of video. Modules include facial expression reader and eye movement detectors. http://www.noldus.com/human-behavior-research/products/the-observer-xt
OTHER		
Liveminds	Purchase	Qualitative research software (and services) for data gathering—online prompts and conversations with focus groups, individuals, blogs, auto-ethnography. Customizable software. http://www.liveminds.co.uk/
Audacity	Free open source	Record live sound, edit, splice, improve sound quality, convert to digital formats, etc. http://audacity.sourceforge.net/

¹ OCR is optical character recognition—where scanning also includes facility to recognize text. Varied functionality which is constantly being enhanced. Included in ‘Document Imaging’ module in MS Office™ and various document management software.

Interpretation Strategies: Appropriate Concepts

Allen Trent *and* Jeasik Cho

Abstract

This chapter addresses a wide range of concepts related to interpretation in qualitative research, examines the meaning and importance of interpretation in qualitative inquiry, and explores the ways methodology, data, and the self/researcher as instrument interact and impact interpretive processes. Additionally, the chapter presents a series of strategies for qualitative researchers engaged in the process of interpretation. The article closes by presenting a framework for qualitative researchers designed to inform their interpretations. The framework includes attention to the key qualitative research concepts transparency, reflexivity, analysis, validity, evidence, and literature. Four questions frame the article: What is interpretation, and why are interpretive strategies important in qualitative research? How do methodology, data, and the researcher/self impact interpretation in qualitative research? How do qualitative researchers engage in the process of interpretation? And, in what ways can a framework for interpretation strategies support qualitative researchers across multiple methodologies and paradigms?

Key Words: qualitative research, interpretation, analysis, data, transparency, reflexivity, validity, evidence, literature review

“All human knowledge takes the form of interpretation.” In this seemingly simple statement, the late German philosopher Walter Benjamin asserts that all knowledge is mediated and constructed. He makes no distinction between physical and social sciences, and so situates himself as an interpretivist, one who believes that human subjectivity, individuals’ characteristics, feelings, opinions, and experiential backgrounds impact observations, analysis of these observations, and resultant knowledge/truth constructions. Contrast this perspective with positivist claims that knowledge is based exclusively on external facts, objectively observed and recorded. Interpretivists then, acknowledge that, if positivistic notions of knowledge and truth are inadequate to explain social phenomena, then positivist, hard science approaches to research (i.e., the scientific method and its variants) are also inadequate. So, although the literature often contrasts quantitative

and qualitative research as largely a difference in kinds of data employed (numerical vs. linguistic), instead, the primary differentiation is in the foundational, paradigmatic assumptions about truth, knowledge, and objectivity.

This chapter is about interpretation and the strategies that qualitative researchers use to interpret a wide variety of “texts.” Knowledge, we assert, is constructed, both individually (constructivism) and socially (constructionism). We accept this as our starting point. Our aim here is to share our perspective on a broad set of concepts associated with the interpretive or meaning-making process. Although it may happen at different times and in different ways, interpretation is a part of almost all qualitative research.

Qualitative research is an umbrella term that encompasses a wide array of paradigmatic views, goals, and methods. Still, there are key unifying elements that include a generally constructionist

epistemological standpoint, attention to primarily linguistic data, and generally accepted protocols or syntax for conducting research. Typically, qualitative researchers begin with a starting point—a curiosity, a problem in need of solutions, a research question, or a desire to better understand a situation from the perspectives of the individuals who inhabit that context (what qualitative researchers call the “emic,” or insider’s, perspective).

From this starting point, researchers determine the appropriate kinds of data to collect, engage in fieldwork as participant-observers to gather these data, organize the data, look for patterns, and then attempt to make sense out of the data by synthesizing research “findings,” “assertions,” or “theories” in ways that can be shared so that others may also gain insights from the conducted inquiry.

Although there are commonalities that cut across most forms of qualitative research, this is not to say that there is an accepted, linear, standardized approach. To be sure, there are an infinite number of variations and nuances in the qualitative research process. For example, some forms of inquiry begin with a firm research question, others without even a clear focus for study. Grounded theorists begin data analysis and interpretation very early in the research process, whereas some case study researchers, for example, may collect data in the field for a period of time before seriously considering the data and its implications. Some ethnographers may be a part of the context (e.g., observing in classrooms), but they may assume more observer-like roles, as opposed to actively participating in the context. Alternatively, action researchers, in studying issues about their own practice, are necessarily situated toward the “participant” end of the participant–observer continuum.

Our focus here is on one integrated part of the qualitative research process, interpretation, the process of collective and individual “meaning making.” As we discuss throughout this chapter, researchers take a variety of approaches to interpretation in qualitative work. Four general questions guide our explorations:

1. What is interpretation, and why are interpretive strategies important in qualitative research?
2. How do methodology, data, and the researcher/self impact interpretation in qualitative research?
3. How do qualitative researchers engage in the process of interpretation?
4. In what ways can a framework for interpretation strategies support qualitative researchers across multiple methodological and paradigmatic views?

We address each of these guiding questions in our attempt to explicate our interpretation of “interpretation,” and, as educational researchers, we include examples from our own work to illustrate some key concepts.

What Is Interpretation, and Why Are Interpretive Strategies Important in Qualitative Research?

Qualitative researchers and those writing about qualitative methods often intertwine the terms *analysis* and *interpretation*. For example, Hubbard and Power (2003) describe data analysis as, “bringing order, structure, and meaning to the data” (p. 88). To us, this description combines analysis with interpretation. Although there is nothing wrong with this construction, our understanding aligns more closely with Mills’s (2007) claim that, “put simply, analysis involves summarizing what’s in the data, whereas interpretation involves making sense of—finding meaning in—that data” (p. 122). For the purpose of this chapter, we’ll adhere to Mills’s distinction, understanding analysis as summarizing and organizing, and interpretation as meaning making. Unavoidably, these closely related processes overlap and interact, but our focus will be primarily on the more complex of these endeavors, interpretation. Interpretation, in this sense, is in part translation, but translation is not an objective act. Instead, translation necessarily involves selectivity and the ascribing of meaning. Qualitative researchers “aim beneath manifest behavior to the meaning events have for those who experience them” (Eisner, 1991, p. 35). The presentation of these insider/emic perspectives is a hallmark of qualitative research.

Qualitative researchers have long borrowed from extant models for fieldwork and interpretation. Approaches from anthropology and the arts have become especially prominent. For example, Eisner’s form of qualitative inquiry, “educational criticism” (1991), draws heavily on accepted models of art criticism. Barrett (2000), an authority on art criticism, describes interpretation as a complex set of processes based on a set of principles. We believe many of these principles apply as readily to qualitative research as they do to critique. The following principles, adapted from Barrett’s principles of interpretation (2000, pp. 113–120), inform our examination:

- *Qualitative phenomena have “aboutness”*: All social phenomena have meaning, but meanings in this context can be multiple, even contradictory.
- *Interpretations are persuasive arguments*: All interpretations are arguments, and qualitative

researchers, like critics, strive to build strong arguments grounded in the information, or data, available.

- *Some interpretations are better than others:* Barrett notes that, “some interpretations are better argued, better grounded with evidence, and therefore more reasonable, more certain, and more acceptable than others” (p. 115). This contradicts the argument that “all interpretations are equal,” heard in the common refrain, “well, that’s just your interpretation.”

- *There can be different, competing, and contradictory interpretations of the same phenomena:* As noted at the beginning of this chapter, we acknowledge that subjectivity matters, and, unavoidably, it impacts one’s interpretations. As Barrett notes (2000), “Interpretations are often based on a worldview” (p. 116).

- *Interpretations are not (and can’t be) “right,” but instead, they can be more or less reasonable, convincing, and informative:* There is never one “true” interpretation, but some interpretations are more compelling than others.

- *Interpretations can be judged by coherence, correspondence, and inclusiveness:* Does the argument/interpretation make sense (coherence)? Does the interpretation fit the data (correspondence)? Have all data been attended to, including outlier data that don’t necessarily support identified themes (inclusiveness)?

- *Interpretation is ultimately a communal endeavor:* Initial interpretations may be incomplete, nearsighted, and/or narrow, but eventually, these interpretations become richer, broader, and more inclusive. Feminist revisionist history projects are an exemplary case. Over time, the writing, art, and cultural contributions of countless women, previously ignored, diminished, or distorted, have come to be accepted as prominent contributions given serious consideration.

So, meaning is conferred; interpretations are socially constructed arguments; multiple interpretations are to be expected; and some interpretations are better than others. As we discuss later in this chapter, what makes an interpretation “better” often hinges on the purpose/goals of the research in question. Interpretations designed to generate theory, or generalizable rules, will be “better” for responding to research questions aligned with the aims of more traditional quantitative/positivist research, whereas interpretations designed to construct meanings through social interaction, to generate multiple perspectives, and to represent the context-specific

perspectives of the research participants are “better” for researchers constructing thick, contextually rich descriptions, stories, or narratives. The former relies on more “atomistic” interpretive strategies, whereas the latter adheres to a more “holistic” approach (Willis, 2007). Both approaches to analysis/interpretation are addressed in more detail later in this chapter.

At this point, readers might ask, why does interpretation matter, anyway? Our response to this question involves the distinctive nature of interpretation and the ability of the interpretive process to put unique fingerprints on an otherwise relatively static set of data. Once interview data are collected and transcribed (and we realize that even the process of transcription is, in part, interpretive), documents are collected, and observations are recorded, qualitative researchers could just, in good faith and with fidelity, represent the data in as straightforward ways as possible, allowing readers to “see for themselves” by sharing as much actual data (e.g., the transcribed words of the research participants) as possible. This approach, however, includes analysis, what we have defined as summarizing and organizing data for presentation, but it falls short of what we actually reference and define as interpretation—attempting to explain the meaning of others’ words and actions. “While early efforts at qualitative research might have stopped at description, it is now more generally accepted that a qualitative researcher adds understanding and interpretation to the description” (Lichtman, 2006, p. 8).

As we are fond of the arts and arts-based approaches to qualitative research, an example from the late jazz drummer, Buddy Rich, seems fitting. Rich explains the importance of having the flexibility to interpret: “I don’t think any arranger should ever write a drum part for a drummer, because if a drummer can’t create his own interpretation of the chart, and he plays everything that’s written, he becomes mechanical; he has no freedom.” The same is true for qualitative researchers; without the freedom to interpret, the researcher merely regurgitates, attempting to share with readers/reviewers exactly what the research subjects shared with him or her. It is only through interpretation that the researcher, as collaborator with unavoidable subjectivities, is able to construct unique, contextualized meaning. Interpretation then, in this sense, is knowledge construction.

In closing this section, we’ll illustrate the analysis versus interpretation distinction with the following transcript excerpt. In this study, the authors (Trent

Transcript	Descriptive & in vivo codes
<p>She didn't know how to control the class and she always thought that someone was doing something bad and yeah, we were in the same class. She just thought certain kids were bad and that they were always doing something wrong. Like, so she thought they were picking on her and they weren't and so she would get flustered really easy and overwhelmed I think. She was just never able to get the kids focused on what she was teaching.</p>	<p><i>Could not control class</i> <i>Assumed bad behavior</i> <i>Singled out kids</i> <i>Felt targeted</i> <i>Overwhelmed</i> <i>Students off task</i></p>

& Zorko, 2006) were studying student teaching from the perspective of K–12 students. This quote comes from a high school student in a focus group interview. She is describing a student teacher she had:

The right-hand column contains “codes” or labels applied to parts of the transcript text. Coding will be discussed in more depth later in this chapter, but, for now, note that the codes are mostly summarizing the main ideas of the text, sometimes using the exact words of the research participant. This type of coding is a part of what we’ve called analysis—organizing and summarizing the data. It’s a way of beginning to say, “what is” there. As noted, though, most qualitative researchers go deeper. They want to know more than “what is”; they also ask, “what does it mean?” This is a question of interpretation.

Specific to the transcript excerpt, researchers might next begin to cluster the early codes into like groups. For example, the teacher “felt targeted,” “assumed kids were going to behave inappropriately,” and appeared to be “overwhelmed.” A researcher might cluster this group of codes in a category called “teacher feelings and perceptions” and may then cluster the codes “could not control class,” and “students off task” into a category called “classroom management.” The researcher then, in taking a fresh look at these categories and the included codes, may begin to conclude that what’s going on in this situation is that the student teacher does not have sufficient training in classroom management models and strategies and may also be lacking the skills she needs to build relationships with her students. These then would be interpretations, persuasive arguments connected to the study’s data. In this specific example, the researchers might proceed to write a memo about these emerging interpretations. In this memo, they might more clearly define their early categories and may also look through other data to see if there are other codes or categories that align with or overlap with this initial analysis. They might write further about their emergent interpretations and, in doing so, may inform future data collection in ways that will allow them

to either support or refute their early interpretations. These researchers will also likely find that the processes of analysis and interpretation are inextricably intertwined. Good interpretations very often depend on thorough and thoughtful analyses.

How Do Methodology, Data, and the Researcher/Self Impact Interpretation in Qualitative Research?

Methodological conventions guide interpretation and the use of interpretive strategies. For example, in grounded theory and in similar methodological traditions, “formal analysis begins early in the study and is nearly completed by the end of data collection” (Bogdan & Biklen, 2003, p. 66). Alternatively, for researchers from other traditions, for example, case study researchers, “Formal analysis and theory development [interpretation] do not occur until after the data collection is near complete” (p. 66).

Researchers subscribing to methodologies that prescribe early data analysis and interpretation may employ methods like analytic induction or the constant comparison method. In using analytic induction, researchers develop a rough definition of the phenomena under study; collect data to compare to this rough definition; modify the definition as needed, based on cases that both fit and don’t fit the definition; and finally, establish a clear, universal definition (theory) of the phenomena (Robinson, 1951, cited in Bogdan & Biklen, 2003, p. 65). Generally, those using a constant comparison approach begin data collection immediately; identify key issues, events, and activities related to the study that then become categories of focus; collect data that provide incidents of these categories; write about and describe the categories, accounting for specific incidents and seeking others; discover basic processes and relationships; and, finally, code and write about the categories as theory, “grounded” in the data (Glaser, 1965). Although processes like analytic induction and constant comparison can be listed as “steps” to follow, in actuality, these are more typically recursive processes in which the researcher

repeatedly goes back and forth between the data and emerging analyses and interpretations.

In addition to methodological conventions that prescribe data analysis early (e.g., grounded theory) or later (e.g., case study) in the inquiry process, methodological approaches also impact the general approach to analysis and interpretation. Ellingson (2011) situates qualitative research methodologies on a continuum spanning “science”-like approaches on one end juxtaposed with “art”-like approaches on the other.

Researchers pursuing a more science-oriented approach seek valid, reliable, generalizable knowledge; believe in neutral, objective researchers; and ultimately claim single, authoritative interpretations. Researchers adhering to these science-focused, post-positivistic approaches may count frequencies, emphasize the validity of the employed coding system, and point to intercoder reliability and random sampling as criteria that bolsters the research credibility. Researchers at or near the science end of the continuum might employ analysis and interpretation strategies that include “paired comparisons,” “pile sorts,” “word counts,” identifying “key words in context,” and “triad tests” (Ryan & Bernard, 2000, pp. 770–776). These researchers may ultimately seek to develop taxonomies or other authoritative final products that organize and explain the collected data.

For example, in a study we conducted about preservice teachers’ experiences learning to teach second-language learners, the researchers collected larger datasets and used a statistical analysis package to analyze survey data, and the resultant findings included descriptive statistics. These survey results were supported with open-ended, qualitative data. For example, one of the study’s findings was “a strong majority of candidates (96%) agreed that an immersion approach alone will not guarantee academic or linguistic success for second language learners.” In narrative explanations, one preservice teacher remarked, “there has to be extra instructional efforts to help their students learn English...they won’t learn English by merely sitting in the classrooms” (Cho, Rios, Trent, & Mayfield, 2012, p. 75).

Methodologies on the “art” side of Ellingson’s (2011) continuum, alternatively, “value humanistic, openly subjective knowledge, such as that embodied in stories, poetry, photography, and painting” (p. 599). Analysis and interpretation in these (often more contemporary) methodological approaches strive not for “social scientific truth,” but instead are formulated to “enable us to learn about ourselves,

each other, and the world through encountering the unique lens of a person’s (or a group’s) passionate rendering of a reality into a moving, aesthetic expression of meaning” (p. 599). For these “artistic/interpretivists, truths are multiple, fluctuating and ambiguous” (p. 599). Methodologies taking more artistic, subjective approaches to analysis and interpretation include autoethnography, testimonio, performance studies, feminist theorists/researchers, and others from related critical methodological forms of qualitative practice.

As an example, one of us engaged in an artistic inquiry with a group of students in an art class for elementary teachers. We called it “Dreams as Data” and, among the project aims, we wanted to gather participants’ “dreams for education in the future” and display these dreams in an accessible, interactive, artistic display (see Trent, 2002). The intent here was not to statistically analyze the dreams/data; instead, it was more universal. We wanted, as Ellingson (2011) noted, to use participant responses in ways that “enable us to learn about ourselves, each other, and the world.” The decision was made to leave responses intact and to share the whole/raw dataset in the artistic display in ways that allowed the viewers to holistically analyze and interpret for themselves. The following text is an excerpt from one response: Almost a century ago, John Dewey eloquently wrote about the need to imagine and create the education that ALL children deserve, not just the richest, the Whitest, or the easiest to teach. At the dawn of this new century, on some mornings, I wake up fearful that we are further away from this ideal than ever... Collective action, in a critical, hopeful, joyful, anti-racist and pro-justice spirit, is foremost in my mind as I reflect on and act in my daily work... Although I realize the constraints on teachers and schools in the current political arena, I do believe in the power of teachers to stand next to, encourage, and believe in the students they teach—in short, to change lives. (Trent, 2002, p. 49)

In sum, researchers whom Ellingson (2011) characterizes as being on the science end of the continuum typically use more detailed or “atomistic” strategies to analyze and interpret qualitative data, whereas those toward the artistic end most often employ more holistic strategies. Both of these general approaches to qualitative data analysis and interpretation, atomistic and holistic, will be addressed later in this chapter.

As noted, qualitative researchers attend to data in a wide variety of ways depending on paradigmatic

and epistemological beliefs, methodological conventions, and the purpose/aims of the research. These factors impact the kinds of data collected and the ways these data are ultimately analyzed and interpreted. For example, life history or testimonio researchers conduct extensive individual interviews, ethnographers record detailed observational notes, critical theorists may examine documents from pop culture, and ethnomethodologists may collect videotapes of interaction for analysis and interpretation.

In addition to the wide range of data types that are collected by qualitative researchers (and most qualitative researchers collect multiple forms of data), qualitative researchers, again influenced by the factors noted earlier, employ a variety of approaches to analyzing and interpreting data. As mentioned earlier in this article, some advocate for a detailed/atomistic, fine-grained approach to data (see e.g., Miles & Huberman, 1994); others, a more broad-based, holistic, “eyeballing” of the data. “Eyeballers reject the more structured approaches to analysis that break down the data into small units and, from the perspective of the eyeballers, destroy the wholeness and some of the meaningfulness of the data” (Willis, 2007, p. 298).

Regardless, we assert, as illustrated in Figure 30.1, that as the process evolves, data collection becomes less prominent later in the process, as interpretation and making sense/meaning of the data becomes more prominent. It is through this emphasis on interpretation that qualitative researchers put their individual imprints on the data, allowing for the emergence of multiple, rich perspectives. This space for interpretation allows researchers the “freedom” Buddy Rich alluded to in his quote about interpreting musical charts. Without this freedom, Rich noted that the process would be simply “mechanical.” Furthermore, allowing space for multiple interpretations nourishes the perspectives of many

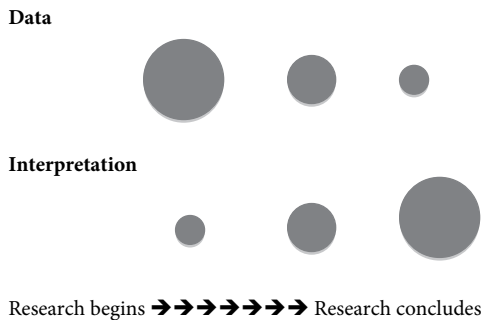


Figure 30.1 As emphasis on data/data collection decreases, emphasis on interpretation increases.

others in the community. Writer and theorist Meg Wheatley explains, “everyone in a complex system has a slightly different interpretation. The more interpretations we gather, the easier it becomes to gain a sense of the whole.”

In addition to the roles methodology and data play in the interpretive process, perhaps the most important is the role of the self/the researcher in the interpretive process. “She is the one who asks the questions. She is the one who conducts the analyses. She is the one who decides who to study and what to study. The researcher is the conduit through which information is gathered and filtered” (Lichtman, 2006, p. 16). Eisner (1991) supports the notion of the researcher “self as instrument,” noting that expert researchers don’t simply know what to attend to, but also what to neglect. He describes the researcher’s role in the interpretive process as combining *sensibility*, the ability to observe and ascertain nuances, with *schema*, a deep understanding or cognitive framework of the phenomena under study.

Barrett (2007) describes self/researcher roles as “transformations” (p. 418) at multiple points throughout the inquiry process: early in the process, researchers create representations through data generation, conducting observations and interviews and collecting documents and artifacts. Another “transformation occurs when the ‘raw’ data generated in the field are shaped into data records by the researcher. These data records are produced through organizing and reconstructing the researcher’s notes and transcribing audio and video recordings in the form of permanent records that serve as the ‘evidentiary warrants’ of the generated data. The researcher strives to capture aspects of the phenomenal world with fidelity by selecting salient aspects to incorporate into the data record” (p. 418). Transformation continues when the researcher analyzes, codes, categorizes, and explores patterns in the data (the process we call analysis). Transformations also involve interpreting what the data mean and relating these “interpretations to other sources of insight about the phenomena, including findings from related research, conceptual literature, and common experience. . . . Data analysis and interpretation are often intertwined and rely upon the researcher’s logic, artistry, imagination, clarity, and knowledge of the field under study” (Barrett, 2007, p. 418).

We mentioned the often-blended roles of participation and observation earlier in this chapter. The role(s) of the self/researcher are often described as points along a “participant/observer continuum” (see, e.g., Bogdan & Biklen, 2003). On the far

“observer” end of this continuum, the researcher situates as detached, tries to be inconspicuous (so as not to impact/disrupt the phenomena under study), and approaches the studied context as if viewing it from behind a one-way mirror. On the opposite, “participant” end, the researcher is completely immersed and involved in the context. It would be difficult for an outsider to distinguish between researcher and subjects. For example, “some feminist researchers and some postmodernists take on a political stance as well and have an agenda that places the researcher in an activist posture. These researchers often become quite involved with the individuals they study and try to improve their human condition” (Lichtman, 2006, p. 9).

We assert that most researchers fall somewhere between these poles. We believe that complete detachment is both impossible and misguided. In doing so, we, along with many others, acknowledge (and honor) the role of subjectivity, the researcher’s beliefs, opinions, biases, and predispositions. Positivist researchers seeking objective data and accounts either ignore the impact of subjectivity or attempt to drastically diminish/eliminate its impact. Even qualitative researchers have developed methods to avoid researcher subjectivity affecting research data collection, analysis, and interpretation. For example, foundational phenomenologist Husserl (1962/1913) developed the concept of “bracketing,” what Lichtman describes as “trying to identify your own views on the topic and then putting them aside” (2006, p. 13). Like Slotnick and Janesick (2011), we ultimately claim, “it is impossible to bracket yourself” (p. 1358). Instead, we take a balanced approach, like Eisner, understanding that subjectivity allows researchers to produce the rich, idiosyncratic, insightful, and yet data-based interpretations and accounts of lived experience that accomplish the primary purposes of qualitative inquiry. “Rather than regarding uniformity and standardization as the summum bonum, educational criticism [Eisner’s form of qualitative research] views unique insight as the higher good” (Eisner, 1991, p. 35). That said, we also claim that, just because we acknowledge and value the role of researcher subjectivity, researchers are still obligated to ground their findings in reasonable interpretations of the data. Eisner (1991) explains:

This appreciation for personal insight as a source of meaning does not provide a license for freedom. Educational critics must provide evidence and reasons. But they reject the assumption that

unique interpretation is a conceptual liability in understanding, and they see the insights secured from multiple views as more attractive than the comforts provided by a single right one. (p. 35)

Connected to this participant/observer continuum is the way the researcher positions him- or herself in relation to the “subjects” of the study. Traditionally, researchers, including early qualitative researchers, anthropologists, and ethnographers, referenced those studied as “subjects.” More recently, qualitative researchers better understand that research should be a reciprocal process in which both researcher and the foci of the research should derive meaningful benefit. Researchers aligned with this thinking frequently use the term “participants” to describe those groups and individuals included in a study. Going a step farther, some researchers view research participants as experts on the studied topic and as equal collaborators in the meaning-making process. In these instances, researchers often use the terms “co-researchers” or “co-investigators.”

The qualitative researcher, then, plays significant roles throughout the inquiry process. These roles include transforming data, collaborating with research participants or co-researchers, determining appropriate points to situate along the participant/observer continuum, and ascribing personal insights, meanings, and interpretations that are both unique and justified with data exemplars. Performing these roles unavoidably impacts and changes the researcher. “Since, in qualitative research the individual is the research instrument through which all data are passed, interpreted, and reported, the scholar’s role is constantly evolving as self evolves” (Slotnick & Janesick, 2011, p. 1358).

As we note later, key in all this is for researchers to be transparent about the topics discussed in the preceding section: what methodological conventions have been employed and why? How have data been treated throughout the inquiry to arrive at assertions and findings that may or may not be transferable to other idiosyncratic contexts? And, finally, in what ways has the researcher/self been situated in and impacted the inquiry? Unavoidably, we assert, the self lies at the critical intersection of data and theory, and, as such, two legs of this stool, data and researcher, interact to create the third, theory.

How Do Qualitative Researchers Engage in the Process of Interpretation?

Theorists seem to have a propensity to dichotomize concepts, pulling them apart and placing

binary opposites on far ends of conceptual continuums. Qualitative research theorists are no different, and we have already mentioned some of these continua in this chapter. For example, in the last section, we discussed the participant–observer continuum. Earlier, we referenced both Willis’s (2007) conceptualization of “atomistic” versus “holistic” approaches to qualitative analysis and interpretation and Ellingson’s (2011) science–art continuum. Each of these latter two conceptualizations inform “how qualitative researchers engage in the process of interpretation.”

Willis (2007) shares that the purpose of a qualitative project might be explained as “what we expect to gain from research” (p. 288). The purpose, or “what we expect to gain,” then guides and informs the approaches researchers might take to interpretation. Some researchers, typically positivist/postpositivist, conduct studies that aim to test theories about how the world works and/or people behave. These researchers attempt to discover general laws, truths, or relationships that can be generalized. Others, less confident in the ability of research to attain a single, generalizable law or truth, might seek “local theory.” These researchers still seek truths, but “instead of generalizable laws or rules, they search for truths about the local context...to understand what is really happening and then to communicate the essence of this to others” (Willis, 2007, p. 291). In both of these purposes, researchers employ atomistic strategies in an inductive process in which researchers “break the data down into small units and then build broader and broader generalizations as the data analysis proceeds” (p. 317). The earlier mentioned processes of analytic induction, constant comparison, and grounded theory fit within this conceptualization of atomistic approaches to interpretation. For example, a line-by-line coding of a transcript might begin an atomistic approach to data analysis.

Alternatively, other researchers pursue distinctly different aims. Researchers with an “objective description” purpose focus on accurately describing the people and context under study. These researchers adhere to standards and practices designed to achieve objectivity, and their approach to interpretation falls between the binary atomistic/holistic distinction.

The purpose of hermeneutic approaches to research is to “understand the perspectives of humans. And because understanding is situational, hermeneutic research tends to look at the details of the context in which the study occurred. The result is generally rich data reports that include multiple perspectives” (Willis, 2007, p. 293).

Still other researchers see their purpose as the creation of stories or narratives that utilize “a social process that constructs meaning through interaction...it is an effort to represent in detail the perspectives of participants...whereas description produces one truth about the topic of study, storytelling may generate multiple perspectives, interpretations, and analyses by the researcher and participants” (Willis, 2007, p. 295).

In these latter purposes (hermeneutic, storytelling, narrative production), researchers typically employ more holistic strategies. “Holistic approaches tend to leave the data intact and to emphasize that meaning must be derived for a contextual reading of the data rather than the extraction of data segments for detailed analysis” (p. 297). This was the case with the “Dreams as Data” project mentioned earlier.

We understand the propensity to dichotomize, situate concepts as binary opposites, and to create neat continua between these polar descriptors. These sorts of reduction and deconstruction support our understandings and, hopefully, enable us to eventually reconstruct these ideas in meaningful ways. Still, in reality, we realize most of us will, and should, work in the middle of these conceptualizations in fluid ways that allow us to pursue strategies, processes, and theories most appropriate for the research task at hand. As noted, Ellingson (2011) sets up another conceptual continuum, but, like ours, her advice is to “straddle multiple points across the field of qualitative methods” (p. 595). She explains, “I make the case for qualitative methods to be conceptualized as a continuum anchored by art and science, with vast middle spaces that embody infinite possibilities for blending artistic, expository, and social scientific ways of analysis and representation” (p. 595).

We explained at the beginning of this chapter that we view analysis as organizing and summarizing qualitative data, and interpretation as constructing meaning. In this sense, analysis allows us to “describe” the phenomena under study. It enables us to succinctly answer “what” and “how” questions and ensures that our descriptions are grounded in the data collected. Descriptions, however, rarely respond to questions of “why?” Why questions are the domain of interpretation, and, as noted throughout this text, interpretation is complex. “Traditionally, qualitative inquiry has concerned itself with *what* and *how* questions...qualitative researchers typically approach *why* questions cautiously, explanation is tricky business” (Gubrium & Holstein, 2000, p. 502). Eisner (1991) describes

this distinctive nature of interpretation: “it means that inquirers try to *account for* [interpretation] what they have given *account of*” (p. 35).

Our focus here is on interpretation, but interpretation requires analysis, for without having clear understandings of the data and its characteristics, derived through systematic examination and organization (e.g., coding, memoing, categorizing, etc.), “interpretations” resulting from inquiry will likely be incomplete, uninformed, and inconsistent with the constructed perspectives of the study participants. Fortunately for qualitative researchers, we have many sources that lead us through analytic processes. We earlier mentioned the accepted processes of analytic induction and the constant comparison method. These detailed processes (see e.g., Bogdan & Biklen, 2003) combine the inextricably linked activities of analysis and interpretation, with “analysis” more typically appearing as earlier steps in the process and meaning construction—“interpretation”—happening later.

A wide variety of resources support researchers engaged in the processes of analysis and interpretation. Saldaña (2011), for example, provides a detailed description of coding types and processes. He shows researchers how to use process coding (uses gerunds, “-ing” words to capture action), in vivo coding (uses the actual words of the research participants/subjects), descriptive coding (uses nouns to summarize the data topics), versus coding (uses “vs.” to identify conflicts and power issues), and values coding (identifies participants’ values, attitudes, and/or beliefs). To exemplify some of these coding strategies, we include an excerpt from a transcript of a meeting of a school improvement committee. In this study, the collaborators were focused on building “school community.” This excerpt illustrates the application of a variety of codes described by Saldaña to this text:

To connect and elaborate the ideas developed in coding, Saldaña (2011) suggests researchers categorize the applied codes, write memos to deepen understandings and illuminate additional questions, and identify emergent themes. To begin the categorization process, Saldaña recommends all codes be “classified into similar clusters...once the codes have been classified, a category label is applied to them” (p. 97). So, in continuing with the study of school community example coded here, the researcher might create a cluster/category called: “Value of Collaboration,” and in this category might include the codes, “relationships,” “building community,” and “effective strategies.”

Having coded and categorized a study’s various data forms, a typical next step for researchers is to write “memos” or “analytic memos.” Writing analytic memos allows the researcher(s) to “set in words your interpretation of the data... an analytic memo further articulates your... thinking processes on what things may mean... as the study proceeds, however, initial and substantive analytic memos can be revisited and revised for eventual integration into the report itself” (Saldaña, 2011, p. 98). In the study of student teaching from K–12 students’ perspectives (Trent & Zorko, 2006), we noticed throughout our analysis a series of focus group interview quotes coded “names.” The following quote from a high school student is representative of many others:

I think that, ah, they [student teachers] should like know your face and your name because, uh, I don’t like it if they don’t and they’ll just like... cause they’ll blow you off a lot easier if they don’t know, like our new principal is here... he is, like, he always, like, tries to make sure to say hi even to the, like, not popular people if you can call it that, you know, and I mean, yah, and the people that don’t usually socialize a lot, I mean he makes an effort to know them and know their name like so they will cooperate better with him.

Although we didn’t ask the focus groups a specific question about whether or not student teachers knew the K–12 students’ names, the topic came up in every focus group interview. We coded the above excerpt and the others, “knowing names,” and these data were grouped with others under the category “relationships.” In an initial analytic memo about this, the researchers wrote:

STUDENT TEACHING STUDY—MEMO
#3 “Knowing Names as Relationship Building”

Most groups made unsolicited mentions of student teachers knowing, or not knowing, their names. We haven’t asked students about this, but it must be important to them because it always seems to come up. Students expected student teachers to know their names. When they did, students noticed and seemed pleased. When they didn’t, students seemed disappointed, even annoyed. An elementary student told us that early in the semester, “she knew our names... cause when we rose [sic] our hands, she didn’t have to come and look at our name tags... it made me feel very happy.” A high schooler, expressing displeasure that his student teacher didn’t know students’

Meeting Transcript

Process Coding

Let's start talking about what we want to get out of this. What I'd like to hear is each of us sharing what we're doing relative to this idea of building community. "Here's what I'm doing. Here's what worked. Here's what didn't work. I'm happy with this. I'm sad with this," and just hearing each of us reflecting about what we're doing I think will be interesting. That collaboration will be extremely valuable in terms of not only our relationships with one another, but also understanding the idea of community in more specific and concrete ways.

Talking
Sharing
Building
Listening
Collaborating
Understanding

IN VIVO CODING

Let's start talking about what we want to get out of this. What I'd like to hear is each of us sharing what we're doing relative to this idea of building community. "Here's what I'm doing. Here's what worked. Here's what didn't work. I'm happy with this. I'm sad with this," and just hearing each of us reflecting about what we're doing I think will be interesting. That collaboration will be extremely valuable in terms of not only our relationships with one another, but also understanding the idea of community in more specific and concrete ways.

Talking about what we want to get out of this
Each of us sharing
Hearing each of us reflecting
Collaboration will be extremely valuable
Relationships

DESCRIPTIVE CODING

Let's start talking about what we want to get out of this. What I'd like to hear is each of us sharing what we're doing relative to this idea of building community. "Here's what I'm doing. Here's what worked. Here's what didn't work. I'm happy with this. I'm sad with this," and just hearing each of us reflecting about what we're doing I think will be interesting. That collaboration will be extremely valuable in terms of not only our relationships with one another, but also understanding the idea of community in more specific and concrete ways.

Open, participatory discussion
Identification of effective strategies
Collaborative, productive relationships
Robust Understandings

VERSUS CODING

Let's start talking about what we want to get out of this. What I'd like to hear is each of us sharing what we're doing relative to this idea of building community. "Here's what I'm doing. Here's what worked. Here's what didn't work. I'm happy with this. I'm sad with this," and just hearing each of us reflecting about what we're doing I think will be interesting. That collaboration will be extremely valuable in terms of not only our relationships with one another, but also understanding the idea of community in more specific and concrete ways.

Effective vs. Ineffective strategies
Positive reflections vs. negative reflections

VALUES CODING

Let's start talking about what we want to get out of this. What I'd like to hear is each of us sharing what we're doing relative to this idea of building community. "Here's what I'm doing. Here's what worked. Here's what didn't work. I'm happy with this. I'm sad with this," and just hearing each of us reflecting about what we're doing I think will be interesting. That collaboration will be extremely valuable in terms of not only our relationships with one another, but also understanding the idea of community in more specific and concrete ways.

Sharing
Building community
Reflection
Collaboration
Relationships
Deeper Understandings

names, told us, "They should like know your name because it shows they care about you as a person. I mean, we know their names, so they should take the time to learn ours too." Another high school student said that even after 3 months, she wasn't sure the student teacher knew her name. Another student echoed, "same here." Each of these students asserted that this (knowing students'

names) had impacted their relationship with the student teacher. This high school student focus group stressed that a good relationship, built early, directly impacts classroom interaction and student learning. A student explained it like this: "If you get to know each other, you can have fun with them . . . they seem to understand you more, you're more relaxed, and learning seems easier."

As noted in these brief examples, coding, categorizing, and writing memos about a study's data are all accepted processes for data analysis and allow researchers to begin constructing new understandings and forming interpretations of the studied phenomena. We find the qualitative research literature to be particularly strong in offering support and guidance for researchers engaged in these analytic practices. In addition to those already noted in this chapter, we have found the following resources provide practical, yet theoretically grounded approaches to qualitative data analysis. For more detailed, procedural, or atomistic approaches to data analysis, we direct researchers to Miles and Huberman's classic 1994 text, *Qualitative Data Analysis*, and Ryan and Bernard's (2000) chapter on "Data Management and Analysis Methods." For analysis and interpretation strategies falling somewhere between the atomistic and holistic poles, we suggest Hesse-Biber and Leavy's (2011) chapter, "Analysis and Interpretation of Qualitative Data," in their book, *The Practice of Qualitative Research* (2nd edition); Lichtman's chapter, "Making Meaning From Your Data," in her book *Qualitative Research in Education: A User's Guide*; and "Processing Fieldnotes: Coding and Memoing" a chapter in Emerson, Fretz, and Shaw's (1995) book, *Writing Ethnographic Fieldnotes*. Each of these sources succinctly describes the processes of data preparation, data reduction, coding and categorizing data, and writing memos about emergent ideas and findings. For more holistic approaches, we have found Denzin and Lincoln's (2007) *Collecting and Interpreting Qualitative Materials*, and Ellis and Bochner's (2000) chapter "Autoethnography, Personal Narrative, Reflexivity," to both be very informative.

We have not yet mentioned the use of computer software for data analysis. The use of CAQDAS (Computer Assisted Qualitative Data Analysis Software) has become prevalent. That said, it is beyond the scope of this chapter because, generally, the software is very useful for analysis, but only human researchers can interpret in the ways we describe. Multiple sources are readily available for those interested in exploring computer-assisted analysis. We have found the software to be particularly useful when working with large sets of data.

Even after reviewing the multiple resources for treating data included here, qualitative researchers might still be wondering, "but exactly how do we interpret?" In the remainder of this section, and in the concluding section of this chapter, we more concretely provide responses to this question, and, in closing, propose a framework for researchers to

utilize as they engage in the complex, ambiguous, and yet exciting process of constructing meanings and new understandings from qualitative sources.

These meanings and understandings are often presented as theory, but theories in this sense should be viewed more as "guides to perception" as opposed to "devices that lead to the tight control or precise prediction of events" (Eisner, 1991, p. 95). Perhaps Erickson's (1986) concept of "assertions" is a more appropriate aim for qualitative researchers. He claimed that assertions are declarative statements; they include a summary of the new understandings, and they are supported by evidence/data. These assertions are open to revision and are revised when disconfirming evidence requires modification. Assertions, theories, or other explanations resulting from interpretation in research are typically presented as "findings" in written research reports. Belgrave and Smith (2002) emphasize the importance of these interpretations (as opposed to descriptions), "the core of the report is not the events reported by the respondent, but rather the subjective meaning of the reported events for the respondent" (p. 248).

Mills (2007) views interpretation as responding to the question, "So what?" He provides researchers a series of concrete strategies for both analysis and interpretation. Specific to interpretation, Mills suggests a variety of techniques, including the following:

- "*Extend the Analysis*": In doing so, researchers ask additional questions about the research. The data appear to say X, but could it be otherwise? In what ways do the data support emergent finding X? And, in what ways do they not?

- "*Connect Findings with Personal Experience*": Using this technique, researchers share interpretations based on their intimate knowledge of the context, the observed actions of the individuals in the studied context, and the data points that support emerging interpretations, as well as their awareness of discrepant events or outlier data. In a sense, the researcher is saying, "based on my experiences in conducting this study, this is what I make of it all."

- "*Seek the Advice of 'Critical' Friends*": In doing so, researchers utilize trusted colleagues, fellow researchers, experts in the field of study, and others to offer insights, alternative interpretations, and the application of their own unique lenses to a researcher's initial findings. We especially like this strategy because we acknowledge that, too often, qualitative interpretation is a "solo" affair.

- “*Contextualize the Findings in the Literature*”: This allows researchers to compare their interpretations to others writing about and studying the same/similar phenomena. The results of this contextualization may be that the current study’s findings correspond with the findings of other researchers. The results might, alternatively, differ from the findings of other researchers. In either instance, the researcher can highlight his or her unique contributions to our understanding of the topic under study.

- “*Turn to Theory*”: Mills defines theory as “an analytical and interpretive framework that helps the researcher make sense of ‘what is going on’ in the social setting being studied.” In turning to theory, researchers search for increasing levels of abstraction and move beyond purely descriptive accounts. Connecting to extant or generating new theory enables researchers to link their work to the broader contemporary issues in the field. (p. 136)

Other theorists offer additional advice for researchers engaged in the act of interpretation. Richardson (1995) reminds us to account for the power dynamics in the researcher–researched relationship and notes that, in doing so, we can allow for oppressed and marginalized voices to be heard in context. Bogdan and Biklen (2003) suggest that researchers engaged in interpretation revisit foundational writing about qualitative research, read studies related to the current research, ask evaluative questions (e.g., is what I’m seeing here good or bad?), ask about implications of particular findings/interpretations, think about the audience for interpretations, look for stories and incidents that illustrate a specific finding/interpretation, and attempt to summarize key interpretations in a succinct paragraph. All of these suggestions can be pertinent in certain situations and with particular methodological approaches. In the next and closing section of this chapter, we present a framework for interpretive strategies we believe will support, guide, and be applicable to qualitative researchers across multiple methodologies and paradigms.

In What Ways Can a Framework for Interpretation Strategies Support Qualitative Researchers Across Multiple Methodological and Paradigmatic Views?

The process of qualitative research is often compared to a journey, one without a detailed itinerary and ending, but instead a journey with general direction and aims and yet an open-endedness

that adds excitement and thrives on curiosity. Qualitative researchers are travelers. They travel physically to field sites; they travel mentally through various epistemological, theoretical, and methodological grounds; they travel through a series of problem finding, access, data collection, and data analysis processes; and, finally—the topic of this chapter—they travel through the process of making meaning out of all this physical and cognitive travel via interpretation.

Although travel is an appropriate metaphor to describe the journey of qualitative researchers, we’ll also use “travel” to symbolize a framework for qualitative research interpretation strategies. By design, this is a framework that applies across multiple paradigmatic, epistemological, and methodological traditions. The application of this framework is not formulaic or highly prescriptive, it is also not an “anything goes” approach. It falls, and is applicable, between these poles, giving concrete (suggested) direction to qualitative researchers wanting to make the most out of the interpretations that result from their research, and yet allows the necessary flexibility for researchers to employ the methods, theories, and approaches they deem most appropriate to the research problem(s) under study.

TRAVEL, a Comprehensive Approach to Qualitative Interpretation

In using the word “TRAVEL” as a mnemonic device, our aim is to highlight six essential concepts we argue all qualitative researchers should attend to in the interpretive process: Transparency, Reflexivity, Analysis, Validity, Evidence, and Literature. The importance of each is addressed here.

Transparency, as a research concept seems, well . . . transparent. But, too often, we read qualitative research reports and are left with many questions: How were research participants and the topic of study selected/excluded? How were the data collected, when, and for how long? Who analyzed and interpreted these data? A single researcher? Multiple? What interpretive strategies were employed? Are there data points that substantiate these interpretations/findings? What analytic procedures were used to organize the data prior to making the presented interpretations? In being transparent about data collection, analysis, and interpretation processes, researchers allow reviewers/readers insight into the research endeavor, and this transparency leads to credibility for both researcher and researcher’s claims. Altheide and Johnson (2011) explain, “There

is great diversity of qualitative research. . . . While these approaches differ, they also share an ethical obligation to make public their claims, to show the reader, audience, or consumer why they should be trusted as faithful accounts of some phenomenon” (p. 584). This includes, they note, articulating “what the different sources of data were, how they were interwoven, and . . . how subsequent interpretations and conclusions are more or less closely tied to the various data . . . the main concern is that the connection be apparent, and to the extent possible, transparent” (p. 590).

In the “Dreams as Data” art and research project noted earlier, transparency was addressed in multiple ways. Readers of the project write-up were informed that interpretations resulting from the study, framed as “themes,” were a result of collaborative analysis that included insights from both students and instructor. Viewers of the art installation/data display had the rare opportunity to see *all* participant responses. In other words, viewers had access to the entire raw dataset (see Trent, 2002). More frequently, we encounter only research “findings” already distilled, analyzed, and interpreted in research accounts, often by a single researcher. Allowing research consumers access to the data to interpret for themselves in the “dreams” project was an intentional attempt at transparency.

Reflexivity, the second of our concepts for interpretive researcher consideration, has garnered a great deal of attention in qualitative research literature. Some have called this increased attention the “reflexive turn” (see e.g., Denzin & Lincoln, 2004:

Although you can find many meanings for the term reflexivity, it is usually associated with a critical reflection on the practice and process of research and the role of the researcher. It concerns itself with the impact of the researcher on the system and the system on the researcher. It acknowledges the mutual relationships between the researcher and who and what is studied . . . by acknowledging the role of the self in qualitative research, the researcher is able to sort through biases and think about how they affect various aspects of the research, especially interpretation of meanings. (Lichtman, 2006, pp. 206–207)

As with transparency, attending to reflexivity allows researchers to attach credibility to presented findings. Providing a reflexive account of researcher subjectivity and the interactions of this subjectivity within the research process is a way for researchers to communicate openly with their audience. Instead

of trying to exhume inherent bias from the process, qualitative researchers share with readers the value of having a specific, idiosyncratic positionality. As a result, situated, contextualized interpretations are viewed as an asset, as opposed to a liability.

LaBanca (2011), acknowledging the often solitary nature of qualitative research, calls for researchers to engage others in the reflexive process. Like many other researchers, LaBanca utilizes a researcher journal to chronicle reflexive thoughts, explorations and understandings, but he takes this a step farther. Realizing the value of others’ input, LaBanca posts his reflexive journal entries on a blog (what he calls an “online reflexivity blog”) and invites critical friends, other researchers, and interested members of the community to audit his reflexive moves, providing insights, questions, and critique that inform his research and study interpretations.

We agree this is a novel approach worth considering. We, too, understand that multiple interpreters will undoubtedly produce multiple interpretations, a richness of qualitative research. So, we suggest researchers consider bringing others in before the production of the report. This could be fruitful in multiple stages of the inquiry process, but especially so in the complex, idiosyncratic processes of reflexivity and interpretation. We are both educators and educational researchers. Historically, each of these roles has tended to be constructed as an isolated endeavor, the solitary teacher, the solo researcher/fieldworker. As noted earlier and in the “analysis” section that follows, introducing collaborative processes to what has often been a solitary activity offers much promise for generating rich interpretations that benefit from multiple perspectives.

Being consciously reflexive throughout our practice as researchers has benefitted us in many ways. In a study of teacher education curricula designed to prepare preservice teachers to support second-language learners, we realized hard truths that caused us to reflect on and adapt our own practices as teacher educators. Reflexivity can inform a researcher at all parts of the inquiry, even in early stages. For example, one of us was beginning a study of instructional practices in an elementary school. The communicated methods of the study indicated that the researcher would be largely an observer. Early fieldwork revealed that the researcher became much more involved as a participant than anticipated. Deep reflection and writing about the classroom interactions allowed the researcher to realize that the initial purpose of the research was not being accomplished, and the researcher believed

he was having a negative impact on the classroom culture. Reflexivity in this instance prompted the researcher to leave the field and abandon the project as it was just beginning. Researchers should plan to openly engage in reflexive activities, including writing about their ongoing reflections and subjectivities. Including excerpts of this writing in research account supports our earlier recommendation of transparency.

Early in this chapter, for the purposes of discussion and examination, we defined *analysis* as “summarizing and organizing” data in a qualitative study, and interpretation as “finding” or “making” meaning. Although our focus has been on interpretation as the primary topic here, the importance of good analysis cannot be underestimated for, without it, resultant interpretations are likely incomplete and potentially uninformed. Comprehensive analysis puts researchers in a position to be deeply familiar with collected data and to organize these data into forms that lead to rich, unique interpretations, and yet to interpretations clearly connected to data exemplars. Although we find it advantageous to examine analysis and interpretation as different but related practices, in reality, the lines blur as qualitative researchers engage in these recursive processes.

We earlier noted our affinity for a variety of approaches to analysis (see e.g., Lichtman, 2006; Saldaña, 2011; or Hesse-Biber & Leavy 2011). Emerson, Fretz, and Shaw (1995) present a grounded approach to qualitative data analysis: in early stages, researchers engage in a *close, line-by-line reading* of data/collected text and accompany this reading with *open coding*, a process of categorizing and labeling the inquiry data. Next, researchers write *initial memos* to describe and organize the data under analysis. These analytic phases allow the researcher(s) to prepare, organize, summarize, and understand the data, in preparation for the more interpretive processes of *focused coding* and the writing up of interpretations and themes in the form of *integrative memos*.

Similarly, Mills (2007) provides guidance on the process of analysis for qualitative action researchers. His suggestions for organizing and summarizing data include *coding* (labeling data and looking for patterns), *asking key questions* about the study data (who, what, where, when, why, and how), developing *concept maps* (graphic organizers that show initial organization and relationships in the data), and *stating what’s missing* by articulating what data are not present (pp. 124–132).

Many theorists, like Emerson, Fretz, and Shaw (1995) and Mills (2007) noted here, provide guidance for individual researchers engaged in individual data collection, analysis, and interpretation; others, however, invite us to consider the benefits of collaboratively engaging in these processes through the use of collaborative research and analysis teams. Paulus, Woodside, and Ziegler (2008) wrote about their experiences in collaborative qualitative research: “Collaborative research often refers to collaboration among the researcher and the participants. Few studies investigate the collaborative process among researchers themselves” (p. 226).

Paulus, Woodside, and Ziegler (2008) claim that the collaborative process “challenged and transformed our assumptions about qualitative research” (p. 226). Engaging in reflexivity, analysis, and interpretation as a collaborative enabled these researchers to reframe their views about the research process, finding that the process was much more recursive, as opposed to following a linear progression. They also found that cooperatively analyzing and interpreting data yielded “collaboratively constructed meanings” as opposed to “individual discoveries.” And finally, instead of the traditional “individual products” resulting from solo research, collaborative interpretation allowed researchers to participate in an “ongoing conversation” (p. 226).

These researchers explain that engaging in collaborative analysis and interpretation of qualitative data challenged their previously held assumptions. They note, “through collaboration, procedures are likely to be transparent to the group and can, therefore, be made public. Data analysis benefits from an iterative, dialogic, and collaborative process because thinking is made explicit in a way that is difficult to replicate as a single researcher” (Paulus, Woodside, & Ziegler, 2008, p. 236). They share that during the collaborative process, “we constantly checked our interpretation against the text, the context, prior interpretations, and each other’s interpretations” (p. 234).

We, too, have engaged in analysis similar to these described processes, including working on research teams. We encourage other researchers to find processes that fit with the methodology and data of a particular study, use the techniques and strategies most appropriate, and then cite to the utilized authority to justify the selected path. We urge traditionally solo researchers to consider trying a collaborative approach. Generally, we suggest researchers be familiar with a wide repertoire of practices. In doing so, they’ll be in better positions to select and use strategies most appropriate for their studies and

data. Succinctly preparing, organizing, categorizing, and summarizing data sets the researcher(s) up to construct meaningful interpretations in the forms of assertions, findings, themes, and theories.

Researchers want their findings to be sound, backed by evidence, justifiable, and to accurately represent the phenomena under study. In short, researchers seek *validity* for their work. We assert that qualitative researchers should attend to validity concepts as a part of their interpretive practices. We have previously written and theorized about validity, and, in doing so, we have highlighted and labeled what we consider to be two distinctly different approaches, *transactional* and *transformational* (Cho & Trent, 2006). We define transactional validity in qualitative research as an interactive process occurring among the researcher, the researched, and the collected data, one that is aimed at achieving a relatively higher level of accuracy. Techniques, methods, and/or strategies are employed during the conduct of the inquiry. These techniques, such as member checking and triangulation, are seen as a medium with which to ensure an accurate reflection of reality (or, at least, participants' constructions of reality). Lincoln and Guba's (1985) widely known notion of trustworthiness in "naturalistic inquiry" is grounded in this approach. In seeking trustworthiness, researchers attend to research credibility, transferability, dependability, and confirmability. Validity approaches described by Maxwell (1992) as "descriptive" and "interpretive" also proceed in the usage of transactional processes.

For example, in the write-up of a study on the facilitation of teacher research, one of us (Trent, 2012, p. 44) wrote about the use of transactional processes: "Member checking is asking the members of the population being studied for their reaction to the findings' (Sagor, 2000, p. 136). Interpretations and findings of this research, in draft form, were shared with teachers (for member checking) on multiple occasions throughout the study. Additionally, teachers reviewed and provided feedback on the final draft of this article." This member checking led to changes in some resultant interpretations (called findings in this particular study) and to adaptations of others that shaped these findings in ways that made them both richer and more contextualized.

Alternatively, in transformational approaches, validity is not so much something that can be achieved solely by way of certain techniques. Transformationalists assert that because traditional or positivist inquiry is no longer seen as an absolute

means to truth in the realm of human science, alternative notions of validity should be considered to achieve social justice, deeper understandings, broader visions, and other legitimate aims of qualitative research. In this sense, it is the ameliorative aspects of the research that achieve (or don't achieve) its validity. Validity is determined by the resultant actions prompted by the research endeavor.

Lather (1993), Richardson (1997), and others (e.g., Lenzo, 1995; Scheurich, 1996) propose a transgressive approach to validity that emphasizes a higher degree of self-reflexivity. For example, Lather has proposed a "catalytic validity" described as "the degree to which the research empowers and emancipates the research subjects" (Scheurich, 1996, p. 4). Beverley (2000, p. 556) has proposed "testimonio" as a qualitative research strategy. These first-person narratives find their validity in their ability to raise consciousness and thus provoke political action to remedy problems of oppressed peoples (e.g., poverty, marginality, exploitation).

We, too, have pursued research with transformational aims. In the earlier mentioned study of preservice teachers' experiences learning to teach second-language learners (Cho, Rios, Trent, & Mayfield, 2012), our aims were to empower faculty members, evolve the curriculum, and, ultimately, better serve preservice teachers so that they might better serve English-language learners in their classrooms. As program curricula and activities have changed as a result, we claim a degree of transformational validity for this research.

Important, then, for qualitative researchers throughout the inquiry, but especially when engaged in the process of interpretation, is to determine the type(s) of validity applicable to the study. What are the aims of the study? Providing an "accurate" account of studied phenomena? Empowering participants to take action for themselves and others? The determination of this purpose will, in turn, inform researchers' analysis and interpretation of data. Understanding and attending to the appropriate validity criteria will bolster researcher claims to meaningful findings and assertions.

Regardless of purpose or chosen validity considerations, qualitative research depends on *evidence*. Researchers in different qualitative methodologies rely on different types of evidence to support their claims. Qualitative researchers typically utilize a variety of forms of evidence including texts (written notes, transcripts, images, etc.), audio and video recordings, cultural artifacts, documents related to the inquiry, journal entries, and field notes taken

during observations of social contexts and interactions. “Evidence is essential to justification, and justification takes the form of an argument about the merit(s) of a given claim. It is generally accepted that no evidence is conclusive or unassailable (and hence, no argument is foolproof). Thus, evidence must often be judged for its credibility, and that typically means examining its source and the procedures by which it was produced [thus the need for transparency discussed earlier]” (Schwandt, 2001, p. 82).

Qualitative researchers distinguish evidence from facts. Evidence and facts are similar but not identical. We can often agree on facts, e.g., there is a rock, it is harder than cotton candy. Evidence involves an assertion that some facts are relevant to an argument or claim about a relationship. Since a position in an argument is likely tied to an ideological or even epistemological position, evidence is not completely bound by facts, but it is more problematic and subject to disagreement. (Altheide & Johnson, 2011, p. 586)

Inquirers should make every attempt to link evidence to claims (or findings, interpretations, assertions, conclusions, etc.). There are many strategies for making these connections. *Induction* involves accumulating multiple data points to infer a general conclusion. *Confirmation* entails directly linking evidence to resultant interpretations. *Testability/falsifiability* means illustrating that evidence does not necessarily contradict the claim/interpretation, and so increases the credibility of the claim (Schwandt, 2001). In the “learning to teach second-language learners” study, for example, a study finding (Cho, Rios, Trent, & Mayfield, 2012, p. 77) was that “as a *moral claim*, candidates increasingly [in higher levels of the teacher education program] feel more responsible and committed to ELLs [English language learners].” We supported this finding with a series of data points that included the following preservice teacher response: “It is as much the responsibility of the teacher to help teach second-language learners the English language as it is our responsibility to teach traditional English speakers to read or correctly perform math functions.” Claims supported by evidence allow readers to see for themselves and to both examine researcher assertions in tandem with evidence and to form further interpretations of their own.

Some postmodernists reject the notion that qualitative interpretations are arguments based on evidence. Instead, they argue that qualitative

accounts are not intended to faithfully represent that experience, but instead are designed to evoke some feelings or reactions in the reader of the account (Schwandt, 2001). We argue that, even in these instances where transformational validity concerns take priority over transactional processes, evidence still matters. Did the assertions accomplish the evocative aims? What evidence/arguments were used to evoke these reactions? Does the presented claim correspond with the study’s evidence? Is the account inclusive? In other words, does it attend to all evidence or selectively compartmentalize some data while capitalizing on other evidentiary forms?

Researchers, we argue, should be both transparent and reflexive about these questions and, regardless of research methodology or purpose, should share with readers of the account their evidentiary moves and aims. Altheide and Johnson (2011) call this an “evidentiary narrative” and explain:

Ultimately, evidence is bound up with our identity in a situation. . . . An “evidentiary narrative” emerges from a reconsideration of how knowledge and belief systems in everyday life are tied to epistemic communities that provide perspectives, scenarios, and scripts that reflect symbolic and social moral orders. An “evidentiary narrative” symbolically joins an actor, an audience, a point of view (definition of a situation), assumptions, and a claim about a relationship between two or more phenomena. If any of these factors are not part of the context of meaning for a claim, it will not be honored, and thus, not seen as evidence. (p. 686)

In sum, readers/consumers of a research account deserve to know how evidence was treated and viewed in an inquiry. They want and should be aware of accounts that aim to evoke versus represent, and then they can apply their own criteria (including the potential transferability to their situated context). Renowned ethnographer and qualitative research theorist Harry Wolcott (1990) urges researchers to “let readers ‘see’ for themselves” by providing more detail rather than less and by sharing primary data/evidence to support interpretations. In the end, readers don’t expect perfection. Writer Eric Liu (2010) explains, “we don’t expect flawless interpretation. We expect good faith. We demand honesty.”

Last, in this journey through concepts we assert are pertinent to researchers engaged in interpretive processes, we include attention to the “*literature*.” In discussing “literature,” qualitative researchers

typically mean publications about the prior research conducted on topics aligned with or related to a study. Most often, this research/literature is reviewed and compiled by researchers in a section of the research report titled, “literature review.” It is here we find others’ studies, methods, and theories related to our topics of study, and it is here we hope the assertions and theories that result from our studies will someday reside.

We acknowledge the value of being familiar with research related to topics of study. This familiarity can inform multiple phases of the inquiry process. Understanding the extant knowledge base can inform research questions and topic selection, data collection and analysis plans, and the interpretive process. In what ways do the interpretations from this study correspond with other research conducted on this topic? Do findings/interpretations corroborate, expand, or contradict other researchers’ interpretations of similar phenomena? In any of these scenarios (correspondence, expansion, contradiction), new findings and interpretations from a study add to and deepen the knowledge base, or literature, on a topic of investigation.

For example, in our literature review for the study of student teaching, we quickly determined that the knowledge base and extant theories related to the student teaching experience was immense, but also quickly realized that few if any studies had examined student teaching from the perspective of the K–12 students who had the student teachers. This focus on the literature related to our topic of student teaching prompted us to embark on a study that would fill a gap in this literature: most of the knowledge base focused on the experiences and learning of the student teachers themselves. Our study then, by focusing on the K–12 students’ perspectives, added literature/theories/assertions to a previously untapped area. The “literature” in this area (at least we’d like to think) is now more robust as a result.

In another example, a research team (Trent et al., 2003) focused on institutional diversity efforts, mined the literature, found an appropriate existing (a priori) set of theories/assertions, and then used this existing theoretical framework from the literature as a framework to analyze data; in this case, a variety of institutional activities related to diversity.

Conducting a literature review to explore extant theories on a topic of study can serve a variety of purposes. As evidenced in these examples, consulting the literature/extant theory can reveal gaps in the literature. A literature review might also lead researchers to existing theoretical frameworks that

support analysis and interpretation of their data (as in the use of the a priori framework example). Finally, a review of current theories related to a topic of inquiry might confirm that much theory already exists, but that further study may add to, bolster, and/or elaborate on the current knowledge base.

Guidance for researchers conducting literature reviews is plentiful. Lichtman (2006) suggests researchers conduct a brief literature review, begin research, and then update and modify the literature review as the inquiry unfolds. She suggests reviewing a wide range of related materials (not just scholarly journals) and additionally suggests researchers attend to literature on methodology, not just the topic of study. She also encourages researchers to bracket and write down thoughts on the research topic as they review the literature, and, important for this chapter, she suggests researchers “integrate your literature review throughout your writing rather than using a traditional approach of placing it in a separate chapter [or section]” (p. 105).

We agree that the power of a literature review to provide context for a study can be maximized when this information isn’t compartmentalized apart from a study’s findings. Integrating (or at least revisiting) reviewed literature juxtaposed alongside findings can illustrate how new interpretations add to an evolving story. Eisenhart (1998) expands the traditional conception of the literature review and discusses the concept of an “interpretive review.” By taking this interpretive approach, Eisenhart claims that reviews, alongside related interpretations/findings on a specific topic, have the potential to allow readers to see the studied phenomena in entirely new ways, through new lenses, revealing heretofore unconsidered perspectives. Reviews that offer surprising and enriching perspectives on meanings and circumstances “shake things up, break down boundaries, and cause things (or thinking) to expand” (p. 394). Coupling reviews of this sort with current interpretations will “give us stories that startle us with what we have failed to notice” (p. 395).

In reviews of research studies, it can certainly be important to evaluate the findings in light of established theories and methods [the sorts of things typically included in literature reviews]. However, it also seems important to ask how well the studies disrupt conventional assumptions and help us to reconfigure new, more inclusive, and more promising perspectives on human views and actions. From an interpretivist perspective, it would be most important to review how well methods and findings permit

readers to grasp the sense of unfamiliar perspectives and actions. (Eisenhart, 1998, p. 397)

Conclusion

And so, our journey through qualitative research interpretation and the selected concepts we've treated in this chapter nears an end, an end in the written text, but a hopeful beginning of multiple new conversations among ourselves and in concert with other qualitative researchers. Our aims here have been to circumscribe interpretation in qualitative research; emphasize the importance of interpretation in achieving the aims of the qualitative project; discuss the interactions of methodology, data, and the researcher/self as these concepts and theories intertwine with interpretive processes; describe some concrete ways that qualitative inquirers engage the process of interpretation; and, finally, to provide a framework of interpretive strategies that may serve as a guide for ourselves and other researchers.

In closing, we note that this "travel" framework, construed as a journey to be undertaken by researchers engaged in the interpretive process, is not designed to be rigid or prescriptive, but instead is designed to be a flexible set of concepts that will inform researchers across multiple epistemological, methodological, and theoretical paradigms. We chose the concepts of transparency, reflexivity, analysis, validity, evidence, and literature (TRAVEL) because they are applicable to the infinite journeys undertaken by qualitative researchers who have come before and to those who will come after us. As we journeyed through our interpretations of interpretation, we have discovered new things about ourselves and our work. We hope readers also garner insights that enrich their interpretive excursions. Happy travels to all—*Bon Voyage!*

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Writing Up Qualitative Research

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Abstract

This chapter provides guidelines for writing journal articles based on qualitative approaches. The guidelines are part of the tradition of the Chicago School of Sociology and the author's experience as a writer and reviewer. The guidelines include understanding experiences in context, immersion, interpretations grounded in accounts of informants' lived experiences, and research as action-oriented. The chapter also covers writing articles that report findings based on ethnographies, autoethnographies, performances, poetry, and photography and other graphic media.

Key Words: Writing, qualitative research, qualitative methods, ethnography, journal articles

How researchers write up results for journal publications depends on the purposes of the research and the methodologies they use. Some topics are standard, such as statements about methods and methodologies, but how to represent other topics, like related research and theory, reflexivity, and informants' accounts, may vary. For example, articles based on ethnographic research may be structured differently from writing up research whose purpose is theory development. Journal editors and reviewers often are familiar with variations in style of write-ups, but, when they are not, they may ask for modifications that violate the methodological principles of the research. A common reviewer request is for percentages, which has little meaning in almost all forms of qualitative research because the purpose of the research is to identify patterns of meanings and not distributions of variables. For example, Irvine's (2013) ethnography of the meanings of pets to homeless people shows a variety of meaning without giving the number of participants from which she drew.

Authors sometimes move easily through the review process, but most often they do not, not

only because reviewers might not "get it," but also because authors have left out, underemphasized, or been less than clear about aspects of their research that reviewers and editors believe are important. Working with editors and reviewers frequently results in improved articles.

The purpose of this chapter is to provide guidelines for writing journal articles based on qualitative approaches. My intended audience is composed of researchers, reviewers for journals, and journal editors. Reviewers for funding agencies may also find this chapter useful. I use the terms "journal article" and "research report" as synonyms, even though some journal articles are not reports of research. I have derived the guidelines from ideas associated with the Chicago School of Sociology and my experience as an author and reviewer. Although the Chicago School was, as Becker (1999) wrote, "open to various ways of doing sociology" (p. 10), the ideas in this chapter are part of the tradition, but they are not representative of the entire tradition. Furthermore, the ideas are not fixed but are open-ended because they evolve over time. I have followed the principles of the Chicago School of

Sociology throughout my career, augmented by updates to these ideas, experiments with other traditions, and the sense I make of my own experiences as researcher, author, and reviewer.

The ideas on which I draw include understanding experiences in context, immersion, interpretations grounded in accounts of informants' lived experiences, and research as action-oriented (Bulmer, 1984; Faris, 1967; Gilgun, 1999*a*; 2005*a*; 2012*a*; 2013*b*). To follow these principles, researchers do in-depth studies that take into account the multiple contextual factors that influence meanings and interpretations, seek multiple points of view, and often use multiple methods such as interviews, observations, and document analysis. Researchers do this style of research not only because what they learn is interesting, but because they want to do useful research; that is, research that leads to social actions and even transformations in policies, programs, and interventions. Authors and reviewers pay attention to these principles. Authors convey them in their write-ups, and reviewers look for them as they develop their appraisals.

Excellent writing up of qualitative research matches these principles. In other words, write-ups convey lived experience within multiple contexts, multiple points of view, and analyses that deepen understandings. In addition, if the research is applied, then authors write about how findings may contribute to quality of life. Qualitative researchers from other traditions may follow similar or different guidelines in their write-ups, and I sometimes note other styles of write-ups. Often these variations are related to terminology and not procedures. The reach of the Chicago School of Sociology is wide and deep.

Following these guidelines does not guarantee an easy review process, but this article will be helpful to researchers as they plan and craft their articles and as they respond to reviewers' and editors' comments. After almost thirty years of publishing research based on qualitative approaches, almost as many years as a reviewer, and the editing of three collections of qualitative research reports (Gilgun, Daly, & Handel, 1992; Gilgun & Sussman, 1996; Gilgun & Sands, 2012), I am positioned to offer helpful guidelines, not only to authors but also to reviewers and journal editors.

I begin this chapter with a discussion of general principles and then cover the content of typical sections of research reports. Some of the general material fits into various sections of reports, such as methods and findings. In those cases, I do not

repeat material already covered and assume that my writing is clear enough so that readers know how the general material fits into particular sections of articles.

Although most of this chapter addresses the writing of conventional research reports, I also cover writing articles that report findings through ethnographies, autoethnographies, performances, poetry, and photography and other graphic media. Ethnographies are based on researchers' immersion in the field, where they do extensive observations, interviews, and often document analysis (see Block, 2012). Geertz's (1973) notion of "thick description" is associated with ethnographies. Thick description is characterized by research reports that show the matrix of meanings that researchers identify and attempt to represent in their reports. Autoethnographies are in-depth reflective accounts of individual lives that the narrators themselves write (Ellis, 2009). Ethnographies and autoethnographies involve reflections on meanings, contexts, and other wider influences on individual lives. They are studies of intersections of individual lives and wider cultural themes and practices. Reports of these types of research can look different from conventional research reports in that they appear less formal; the usual sections of methods, literature review, findings, and analysis may have different names; and the sections may be in places that fit the logical flow of the research and not the typical structure of introductory material, methods, results, and discussion. Despite these superficial differences, researchers who write these kinds of articles seek to deepen understandings and hope to move audiences to action through conveying lived experience in context and through multiple points of view. They also typically seek transformations of persons and societies. Links between these forms of research and Chicago School traditions are self-evident.

Some General Principles

Research reports that have these characteristics depend on the quality of the data on which the reports are based, the quality of the analysis, and the skills of researchers in conveying the analysis concisely and with "grab" (Glaser, 1978), which means writing that is vivid and memorable (Gilgun, 2005*b*). Grab brings findings to life. With grab, human experiences jump off the page. Priority is given to the voices of research participants, whom I call informants, with citations and the wisdom of other researchers providing important contextual information. The voices and analyses of researchers

do not dominate (Gilgun, 2005*c*), except in some articles whose purpose is theory development or the presentation of a theory. Researcher analyses often are important, especially in putting forth social action recommendations that stem from the experiences of informants.

A well-done report shows consistency between research traditions and the writing-up of research. For example, reflexivity statements, writing with grab, and copious excerpts from fieldnotes, interviews, and documents of various sorts are consistent with phenomenological approaches whose emphasis is on lived experience and interpretations that informants make of their experiences. Researchers new to qualitative research, however, often mix their traditions without realizing it, which works when the traditions are compatible. When the traditions are not compatible, the write-ups can be confusing and even contradictory (Gilgun, 2005*d*). Some authors may write in distanced, third-person styles while attempting to convey informants' lived experiences. These scholars may, therefore, have difficulty getting their articles accepted. Hopefully, this chapter will facilitate the writing of research reports that show consistency across their many parts and save scholars from rejections of work over which they have taken much care.

Details on These General Principles

In this section, I provide more detail on writing up qualitative research. I begin with a discussion of the need for high-quality data, high-quality analysis, and grab. I then move on to the details of the report, such as the place of prior research and theory, contents of methods sections, organization of findings, and the balance between descriptive material and authors' interpretations. Dilemmas abound. Writing up qualitative research is not for the faint of heart.

High-Quality Data

Since qualitative researchers seek to understand the subjective experiences of research informants in various contexts, high-quality data result in large part from the degree that researchers practice immersion and to the degree that both researchers and informants develop rapport and engage with each other. Through active engagement, informants share their experiences with the kind of detail that brings their experiences to life. How to develop rapport is beyond the scope of this article, but openness and acceptance of whatever informants say are fundamental to engagement. Interviewers do

not have to agree with the values that informants' accounts convey, as when I interview murderers and rapists (Gilgun, 2008), but we do maintain a neutrality that allows the dialogue to continue (Gilgun & Anderson, 2013). The content of interviews is not about us and our preferences, but about understanding informants.

Prolonged engagement can result in quality data. In interview research, prolonged engagement allows for informants' multiple perspectives to emerge, including inconsistencies, contradictions, ambiguities, and ambivalences. In addition, prolonged engagement facilitates the kind of trust needed for informants to share personal, sensitive information in detail, which are the kinds of data that qualitative researchers seek. Prolonged engagement also gives researchers time to reflect on what they are learning and experiencing through the interviews. This provides opportunities to develop new understandings and test new understandings through subsequent research. Their understandings thus deepen and broaden. Informants, too, can reflect, reconsider, and deepen the accounts they share.

Prolonged engagement means in-depth interviews, typically multiple interviews of more than an hour each. As mentioned earlier, time between interviews allows researchers and informants to reflect on the previous interview and prepare for the next. Researchers can do background reading, discuss emerging ideas with others, and formulate pertinent new questions. Informants may retrieve long-forgotten memories and interpretations through interviews. If they have only one interview, they have no opportunity to share with researchers the material that arises after the single interview is concluded.

There are exceptions to multiple interviews as necessary for immersion and high-quality data. When researchers have expertise in interviewing and when the topic is focused, one interview of between ninety minutes to two hours could provide some depth. Even under these conditions, however, more than one interview is ideal. I did a study that involved one ninety-minute interview with perpetrators of child sexual abuse in order to understand the circumstances under which their abusive behaviors became known to law enforcement. Thus, the interview was focused. The interviewees were volunteers who had talked about the topic many times in the course of their involvement in sex abuse treatment programs. They shared their stories with depth and breadth. I, too, was well-prepared. By then, I had had about twenty-five years of experience

interviewing people about personal, sensitive topics. The informants provided accounts not only because the topic was focused, but because they were willing to share and I was willing to listen and to ask questions about their sexually abusive behaviors. With one interview, however, I knew relatively little about their social histories and general worldviews. Thus, I did not have the specifics necessary to place their accounts into context. The material they provided remained valuable and resulted in one publication (Sharma & Gilgun, 2008) and others in planning stages. I prefer two or more interviews because of the importance of contextual data.

In observational studies, prolonged engagement means that researchers do multiple observations over time to obtain the nuances and details that compose human actions. Observational studies often have interview components and also may have document analysis as well. In document analysis, prolonged engagement means researchers base their analyses on an ample storehouse of documents and not just flit in and out of the documents. The quality of document analysis depends on whether the analysis shows multiple perspectives, patterns, and variations within patterns. Ethnographies have these characteristics. Block's (2012) ethnographic research on AIDS orphans in Lesotho, Africa, is an example of a well-done ethnography.

Sample Size

In principle, the size of the sample and the depth of the interview affect whether researchers can claim immersion. The more depth and breadth each case in a study has, the smaller the sample size can be. For example, researchers can engage in immersion through a single in-depth case study when they do multiple interviews and if multiple facets of the case are examined. Case studies are investigations of single units. The case can be composed of an individual, a couple, a family, a group, a nation, or a region. Single case studies are useful in the illustration, development, and testing of theories, as well as in in-depth descriptions.

The more focused the questions, the larger the sample will be. A study on long-term marriage would require a minimum of two or three interviews because the topic is complicated. The sample would include at least ten participants and up to twenty or thirty, depending on the number of interviews, to account for some of the many patterns that are likely to emerge in a study of a topic this complex. In the one-interview study I did of how sexual abuse came to the notice of law enforcement,

one interview was adequate because of the tight focus of the question. Yet, I used a sample size of thirty-two to maximize the possibility of identifying a variety of patterns, which the study accomplished. As mentioned, the one interview, however, did not allow me to contextualize the stories the informants told. Fortunately, I have another large sample that involved multiple, in-depth interviews in which informants discussed multiple contexts over time. This other study was helpful to me in understanding the accounts from the single-interview study.

Recruitment can be difficult. When it is, researchers may not be able to obtain an adequate sample. For example, a sample of seven participants engaging in a single sixty- to ninety-minute interview may not provide enough data on which to base a credible analysis. In a similar vein, articles based on a single or even a few focus groups may not provide enough depth to be informative. Some depth is possible if, in a single-interview study of less than fifteen or twenty interviewees, researchers meet with informants a second time to go over what researchers understand about informants' accounts. This sometimes is called *member-checking*, and it provides additional data on which to base the analysis. In summary, the more depth and breadth to a study, the smaller the sample size can be—even as small as one or two—depending on the questions and the complexity of the cases.

Quality of the Analysis

A quality analysis begins with initial planning of the research and continues until the article is accepted for publication. An excellent research report has *transparency*, meaning the write-up is clear in what researchers did, how they did it, and why. I often tell students they can do almost anything reasonable and ethical, as long as they make a clear account in the write-up.

During planning, some researchers identify those concepts that they can use as sensitizing concepts once in the field. Transparency about the sources of sensitizing concepts characterizes well-done reports. The sources are literature reviews and reflexivity statements. Most researchers, however, have only a limited awareness of the importance of being clear about the sources of sensitizing concepts and other notions that become part of research coding schemes. Sensitizing concepts are notions that researchers identify before beginning their research and that help researchers notice and name social processes that they might not have noticed otherwise (Blumer, 1986). Other researchers wait until data

analysis to begin to identify concepts that they may use as codes and that may also become core concepts that organize findings. Either approach is acceptable and depends on purpose and methodologies.

During data collection, researchers reflect on what they are learning, typically talk to other researchers about their emerging understandings, and read relevant research and theory to enlarge and deepen their understandings. Researchers also keep fieldnotes that are a form of reflection. Based on their various reflections, researchers can reformulate interview and research questions and formulate new ones, do within—and across—case comparisons while in the field, and develop new insights into the meanings of the material.

Also, while in the field, researchers identify promising patterns of meanings and identify tentative core concepts, sometimes called *categories*, which are ideas that organize the copious material that they amass. Once researchers identify tentative core concepts, they seek to test whether they hold up, and, when they do, they further develop the patterns and concepts. Sometimes researchers think they have “struck gold” when they identify a possible core concept or pattern, only to find that the data—or metaphorical vein of gold—peter out (Phyllis Stern, personal communication, November 2002). They then go on to identify and follow-up on other concepts and patterns that show promise of becoming viable.

Core concepts become viable when researchers are able to dimensionalize them (Schatzman, 1991) through selective coding (Corbin & Strauss, 2008). This means that researchers have found data that show the multiple facets of concepts, such as patterns and exceptions to any general patterns. Authors may use other terms to describe what they did, such as thematic analysis. What is important is to describe the processes and produces; and what researchers call them is of less importance.

Core concepts may begin as sensitizing concepts. Researchers sometimes identify, name, and code core concepts through notions that are part of their general stores of knowledge but were not part of the literature review or reflexivity statement. Glaser (1978) called the practice “theoretical sensitivity.” The names researchers choose may be words or phrases informants have used. However derived, core concepts are central to the organization of findings (Gilgun, 2012a).

At some point, data collection stops, but analysis does not. Researchers carry analysis that occurred in the field into the next phases of the research.

Immersion at this point means that researchers read and code transcripts of interviews, observations, and any documentary material they find useful. They carry forward the core concepts they identified in the field. An example of a core concept is “resilience,” which in my own research organized a great deal of interview material. The concept of resilience has been an organizing idea in several of the articles I have written and plan to write (Gilgun, 1996a; 1996b; 2002a; 2002b; 2004a; 2004b; 2005a; 2006, 2008; 2010; Gilgun & Abrams, 2005; Gilgun, Keskinen, Marti, & Rice, 1999; Gilgun, Klein, & Pranis, 2000).

Corbin and Strauss (2008) stated that selective coding helps researchers to decide if a concept can become a core concept, meaning it organizes a great deal of data that have multiple dimensions. An example of dimensionalization is a study of social workers in Australia whose clients were Aboriginal people. The researchers identified several core concepts, among them critical self-awareness (Bennet, Zubrzycki, & Bacon, 2011). The dimensions of critical self-awareness included understanding motivations to work with Aboriginal people, fears of working with Aboriginal people, and personalization and internalization of the anger that some Aboriginal people express.

Like many other researchers, Bennet et al. (2011) were not working within an explicit Chicago School tradition. They therefore do not use terms such as core concepts, dimensionalization, and selective coding. Instead, they described their procedures as thematic analysis, conceptual mapping, and a search for meaning. However, they did use the term “saturation,” which is part of the Chicago School tradition.

A single core concept or multiple related core concepts compose research reports. The Bennet et al. (2011) article, for example, linked multiple core concepts. The authors showed how critical self-awareness leads to meaningful relationships that in turn connect to “acquiring Aboriginal knowledge” (p. 30).

“Grab”

With viable core concepts and rich data, researchers are positioned to present their findings in ways that are memorable and interesting; that is, with “grab” (Glaser, 1978). “Grab” requires compelling descriptive material: excerpts from interviews, field notes, and various types of documents, as well as researchers’ paraphrases of these materials. An example of a research report with grab is Irvine’s

(2013) account of her study of the meanings of pets to homeless people. She provided vivid descriptions of her interactions with the participants and compelling quotes that show what pets mean. Here, an example from Denise's account of her relationship with her cat Ivy:

I have a history with depression up to suicide ideation, and Ivy, I refer to her as my suicide barrier. And I don't say that in any light way. I would say, most days, she's the reason why I keep going.... She is the only source of daily, steady affection and companionship that I have. (p. 19)

These and other quotes, as well as Irvine's well-written, detailed descriptive material, show what grab means.

Grab equates with excellence in writing. Irvine's (2013) article is an example. In terms of the grab of her article, her work is in the Chicago School tradition. She wrote in the first person. She told complete stories in which she quoted extensively from the interviews, described the persons she interviewed and the settings in which she interviewed them, and provided biographical sketches. Robert Park and Ernest Burgess, both of whom trained generations of graduate students in qualitative research at the University of Chicago in the first quarter of the twentieth century, held seminars on the use of literary techniques, such as those used in novels and autobiographies, in writing up research (Bulmer, 1984; Gilgun, 1999*d*; 2012*a*). These educators wanted researchers to report on their "first-hand observation." Park told a class of graduate students to

[g]o and sit in the lounges of the luxury hotels and on the doorsteps of the flophouses; sit on the Gold Coast settees and on the slum shakedowns; sit in the Orchestra Hall and in the Star and Garter Burlesk. In short, gentlemen [sic], go get the seat of your pants dirty. (McKinney, 1966, p. 71)

Park suggested to Pauline Young (1928; 1932) to "think and feel" like the residents of Russian Town, the subject of her dissertation, published in 1932 (Faris, 1967). Irvine's work shows these qualities. She immersed herself in the settings, she conducted in-depth interviews, and she conveyed her first-hand experiences in vivid terms.

The Chicago School also encouraged students to write in the first person. A good example is a report by Dollard (1937), who was concerned about the racial practices of the Southern town where he was doing fieldwork. He said he was afraid that other white people watched as he talked to "Negroes" on

his front porch, when he knew that custom regarding the "proper" place of "Negroes" was at the back door. He wrote

My Negro friend brought still another Negro up on the porch to meet me. Should we shake hands? Would he be insulted if I did not, or would he accept the situation? I kept my hands in pockets and did not do it, a device that was often useful in resolving such a situation. (p. 7)

This description is a portrait of a pivotal moment in Dollard's fieldwork, and it is full of connotations about the racist practices of the time (Gilgun, 1999*d*; 2012*a*).

Irvine (2013) also wrote in the first person. Here's an example:

I met Trish on a cold December day in Boulder. She stood on the median at the exit of a busy shopping center with her Jack Russell Terrier bundled up in a dog bed beside her. She was "flying a sign," or panhandling, with a piece of cardboard neatly lettered in black marker to read, "Sober. Doing the best I can. Please help." (p. 14)

These two excerpts illustrate a methodological point Small (1916) made in his chapter on the first fifty years of sociological research in the United States: namely, the importance of going beyond "technical treatises" and providing first-person "frank judgments" that can help future generations interpret sociology. Without such contexts, "the historical significance of treatises will be misunderstood" (p. 722). Throughout his chapter, Small wrote in the first-person and provided his views—or frank judgments—on the events he narrated. From then until now, research reports in the Chicago tradition are vivid and contextual, conveying to the extent possible what it was like to be persons in situations.

There are many other examples of well-done research reports. Eck's (2013) article on never-married men includes the basic elements that are present in almost all reports based on qualitative methods. It is transparent in its procedures, situated within scholarly traditions, well-organized, vivid, and instructive both for those new to qualitative research and for long-term researchers like me. The other articles I cite in this chapter also show many desirable qualities in research reports.

Research Report Sections

The main sections of standard reports based on qualitative methods are the same as for articles based on other types of methods: Introduction,

Methods, Findings, and Discussion. The American Psychological Association (APA) manual (2009) provides information on what goes into each of these sections. Research reports in sociology journals follow a similar format, although the citation style is slightly different. The American Sociological Association uses first and last names in the reference section, a practice I support. In articles based on qualitative approaches, researchers sometimes change the names of sections, add or omit some, or reorder them. When changes are made, the general guideline is whether the changes make sense and are consistent with the purpose of the research. As Saldaña (2003) pointed out, researchers choose how to present their findings on the basis of credibility, vividness, and persuasive qualities and not for the sake of novelty. Because some articles report findings as fictionalized accounts, poetry, plays, songs, and performances (including plays), it makes sense that the sections on these findings vary from the standard format that I discuss here.

Although there are no rigid rules about how to write journal articles based on qualitative research, much depends on the methodological perspectives, purposes of the research, and the editorial guidelines of particular journals. For example, if researchers want to develop a theory, it is important to be clear from the beginning of the article to state this as the purpose of the research. The entire article should then focus on how the authors developed the theory. Research and theory cited in the literature review should have direct relevance to the substantive area on which the authors theorized. The methods section should explain what the researchers did to develop the theory. The findings section should begin with a statement of the theory that the researchers developed. The rest of the findings section should usually be composed of three parts. The first is composed of excerpts from those data that support the concepts of the theory. This is the grounding of the theory in something clear and concrete. The second is the authors' thinking or interpretation of the meanings of each of the concepts. The third is an analysis of how the theory contributes to what is already known, such as how the findings elaborate on and call into question what is known. Thus, a research report on the development of a theory should contain a lot of scholarship that others have developed.

A report based on narrative principles or one based on an ethnography should contain copious excerpts from interviews, citing less scholarship than an article whose purpose is to develop theory.

However, it is good practice to bring in related research and theory in the results section when this literature helps in interpretation, when findings have connections to other bodies of thought, and when findings are facets of a larger issue. In my now older publication on incest perpetrators (Gilgun, 1995), the editors suggested that I show that when therapists engage in sexual relationships with clients, they are engaging in abuses of power similar to those of incest perpetrators. I was at first indignant that the editors wanted me to do even more work on the article, but I soon was glad they did. It is important to show that incest or any human phenomenon is not isolated from other phenomenon but is part of a larger picture. Doing so fit my purposes, which was to show how to do theory-testing/theory-guided qualitative research. Showing how findings fit into related research and theory is part of this type of research.

Whenever researchers are ready to submit an article for publication, it is wise to read recent issues of journals in which they would like to publish. If they can identify an article whose structure, methodologies, and general purpose are similar to theirs, they could study how those authors presented their material. If, for example, in a report on narrative research, the introductory material is relatively brief, and the findings and discussion sections compose most of the pages, researchers would do well to format their articles in similar ways. I study journals in which I have interest and model much of my own articles after those published in these journals. I make sure, however, that I cover topics that in my judgment are important to cover.

Prior Research and Theory

In my experience, something as simple as the place of prior research and theory can get complicated in the writing of reports based on qualitative research, even when the purpose of the article is primarily descriptive and is not to construct an explicit theory. In general, related research and theory literature can be presented at the beginning of a report as part of a review of pertinent research and theory, in the findings section when prior work helps in the interpretation and analysis of findings, or in the discussion section, where authors may reflect on how their findings add to, undermine, or correct what is known and even add something new.

Readers expect and journal editors typically want articles to begin with literature review, with some exceptions. A perusal of journals that publish qualitative studies shows this. Yet there are exceptions.

Valásquez (2011) began her report on her encounter with scientology with an extended and rather meandering first-person narrative. Her literature review began toward the end of the article. She tailored the review to the report that preceded it. In this article and others, the literature review helped in the interpretation of findings and helped to situate the report in its scholarly contexts. In other articles, the literature review appears in the introductory section. This sets the scholarly context of the research, highlights the significance of topics, and identifies gaps in knowledge. Neither authors nor reviewers should have rigid expectations about where the scholarship of others belongs. It belongs where it makes the most sense and has the most impact.

For many, the placement of literature reviews seems self-evident. Yet, some well-known approaches, such as grounded theory, can set authors up for confusion about where the literature review belongs. This can result in delays in writing up their results. The procedures of grounded theory are open-ended and designed to find new aspects of phenomena—often underresearched—and then develop theories from the findings. At the outset of their work, researchers cannot anticipate what they will find. Therefore, teachers such as Strauss and Glaser advised students not to do literature reviews until they had identified basic social processes that become the focus of the research (Covan, 2007; Glaser & Strauss, 1967).

How, then, do researchers write up research reports when they are doing an open-ended study that, by definition, will culminate in unanticipated findings? Do they write their reports as records on how they proceeded chronologically, or do they follow APA style and the dominant tradition that says the literature review comes first? For the most part, I follow the tradition, as, apparently, do most researchers. However, to structure reports in this way sometimes feels strained and artificial. I would prefer to write a more chronological account, in which I can share with readers the lines of inquiry and procedures I followed. The literature review at the beginning of the report, therefore, would be brief. The methods section is quite detailed in how I went about developing the theory. The findings section would have the three-part format I discussed earlier: statement of the theory, presentations of excerpts that support assertions that certain concepts compose the theory, my interpretation of the meanings of the concepts and the excerpts that support them, and then the use of related research and theory to further develop the theory and to situate it in its scholarly traditions.

In all but one of the research reports that I have published, I did the literature after I had identified findings. The one exception was research I did based on the method of analytic induction, in which researchers can use literature reviews to focus their research from the outset (Gilgun, 1995, 2007). In this research, I used concepts from theories on justice and care to analyze transcripts of interviews I had previously conducted on how perpetrators view child sexual abuse. Even though I was familiar with the transcripts, I found that the concepts of justice and care and their definitions sensitized me to see things in the material that I had not noticed as I did data collection and during previous analyses of the data.

Furthermore, in writing up the results, I brought in research that was not part of the literature review to help me to interpret findings and to show how findings fit with and added to what was already known. I did not place this material in the introductory literature review. Placing related research and theory as parts of the results and discussion sections is common and may be necessary in articles that are reporting on a theory that the authors developed. For descriptive studies whose purpose is not theory-building, such as ethnographies, some findings sections include the addition of research and theory not present in the introductory section. Often, however, authors do not follow this pattern. An example is found in Ahmed (2013), who described how migrants experience settling into a new country. She presents excerpts from interviews and her interpretation of them, including organizing them into a typology, but she does not bring additional research and theory into her interpretations.

Tensions can arise between how much space to give to literature reviews and how much to allot to presentation of informants' accounts/findings (Gilgun, 2005*e*). This happened in the most recent article I co-wrote, which is on mothers' perspectives on the signs of child sexual abuse (Gilgun & Anderson, 2013). We believed the literature review was important because it not only set up our research but summarized a great deal of information that was important to our intended audience of social service professionals. We also wanted to anticipate the expectations of reviewers and the journal editor. Yet, we put much effort into making the literature review as concise as possible in order to have reasonable space for findings. We wrote the literature review before we did data analysis. When we wrote up the results, the first draft was probably three times longer than any journal article could be.

We had written case studies first to be sure that we understood each case in detail. We had wanted to share what the women said in the kind of detail that had helped us deepen our own understandings, so we cut back on the case material. The article was still too long. We decided to exclude the few instances we had in which women knew of the abuse but tried to handle it themselves or did not believe the children when told. We did more summarizing of the literature review. We eliminated many references.

After much effort, we finally had a manuscript that was the required length of twenty-two pages. It included a literature review that set up the research in good form, an adequate accounting of the method, and findings that conveyed with grab the complexities of the signs and lack of signs of child sexual abuse. We wove points made in the literature review into our interpretations, yet we had to leave out important patterns for the sake of space. The editor's decision was a revise and resubmit, which we did. The main recommendation was to elaborate on applications. This was a great suggestion, and we dug deep to think about this. We are pleased with the results. We had to do further reading on topics we had not anticipated at the onset of our project, and we squeezed in a few new citations in the discussion section that related to implications of the research. This additional material greatly enhanced the meanings and usefulness of the research.

There is much more to say about qualitative research and literature reviews. Sometimes researchers get stuck, as I have more than once. I have research that I have not yet published because I have been unable to figure out how to do the multiple literature reviews I think I must show how my theory builds on, adds to, and challenges what is already known. I have written up this research as conference papers, where expectations about literature reviews are more relaxed (Gilgun, 1996c, 1998, 1999c, 2000). One of these papers was on a comprehensive theory of interpersonal violence (Gilgun, 2000). I wanted to write my theory first and then show how the findings contribute to what is already known. Doing so doesn't seem so outlandish today, and I now can imagine writing it up exactly as I would want to. At the same time, I wonder if I would? I really don't know if any journal that would publish a theory of violence would also accept an article that places a literature review after findings. Furthermore, my writing up of the theory would take so many pages that I would not have enough space to do a comprehensive literature review. As of today, the theory I am developing has

links to sixteen or more bodies of literature. No way can I publish a journal-length article that will accommodate that much research and theory!

So, here I am, many years into the development of a comprehensive theory, still reflecting on how to create journal articles out of my analysis. I have published many articles in social media outlets exploring ideas that are the basis for the theory. I have put these articles into collections that are available on the internet (Gilgun, 2012*b*; 2012*c*; 2013*a*). The theory is so complex that writing bits and pieces over the years and having a place to put them have been very helpful.

Finally, some articles may cite few if any related research and theory. This may fit articles whose purpose is to convey lived experience that stands on its own. These articles feature performances, plays, autoethnographies, fictionalized accounts, poetry, and song, among others. Egbe (2013) wrote two poems that she explained were accounts of her experiences of doing research in Nigeria with young smokers. She said she was "dazed by the vast opportunity this method gives a researcher to dig deep into a research problem and be submerged into the world of participants" (p. 353). Her two-page article is composed of two poems and her explanation. The article showed grab, evidence of immersion, experiences in contexts, and multiple perspectives. Her work, therefore, followed well-established guidelines for writing up qualitative research. Egbe not only omitted a literature review, but she did not write about how to use the results of her research, assuming that its uses are self-evident. Obviously, she thought a literature review unnecessary; the reviewers and journal editors agreed with her.

Reflexivity Statements

A growing number of journals encourage researchers to include reflexivity statements in research reports. Researchers may place these in the introductory material of an article, after the literature review and before the methods section; this probably is the most important place to put them because reflexivity statements often influence the focus and design of the research, including the choice of sensitizing concepts and codes. Reflexivity statements may also appear in the methods and findings and methods sections when important. Reflexivity statements are accounts of researchers' experiences with the topic of research; accounts of their expectations regarding informant issues and their relationships to informants, especially in regard to power differentials and other

ethical concerns; and accounts of their reflections on various issues related to possible experiences that informants may have had. They also may include the experience they had while participating in the research (D’Cruz, Gillingham, & Melendez, 2007; Presser, 2005). My article on doing research on violence is an extended reflexivity statement (Gilgun, 2008). There appears to be no standard content for reflexivity statements and no standard places for them to appear. Personal and professional experiences and reflections on power differentials may be the emergent standard. Whatever decisions researchers make about reflexivity statements, they alert audiences to researchers’ perspectives, which can be helpful to readers as they attempt to make sense of research reports.

An example of a reflexivity statement is found in Winter (2010) work. Winter is a practitioner turned researcher who had a previous relationship as a guardian ad litem with the children with whom she later conducted the research that she was reporting. Winter was reflexive about the implications of her prior relationship with these children. I imagine, based on my own experience, that she put only a fraction of her thinking into her article. Not only did she write in her reflexivity statement that she had a prior relationship with the children, but she also wrote about the ethical issues involved.

Ethical issues have a place in reflexivity statements. I have run into ethical questions over the course of my research career. One situation that stands out is the encounter I had with a mother and her eleven-year-old daughter who had participated in my dissertation research on child sexual abuse (Gilgun, 1983). The mother cried and told her daughter how sorry she was that she had been unable to protect her from sexual abuse. The girl was touched but did not seem to know what to do. I suggested that she go stand by her mother. When she got close, the mother and daughter hugged each other and cried. This is a significant event with ethical implications that I included in the findings section of my dissertation and in a subsequent research report (Gilgun, 1984). The ethical issue is, first, whether I should have stepped out of my role as detached researcher and guided the girl to go to her mother, and, second, whether I should have made my blurring of boundaries public by publishing them.

As far as the placement of reflexivity statements, the initial statement has a logical location after the literature review because the reflexivity statement contributes to the development of the research questions, the identification of sensitizing concepts,

the interview schedule, and the overall design of research procedures. Accounts of ongoing reflexivity could be part the findings section and of the discussion section. Reflexivity statements are not a standard part of research reports, but they can contribute to readers’ understandings of the research.

Along with the literature review, reflexivity statements contribute to practical and applied significance statements and may also help to identify gaps in knowledge. Literature reviews and reflexivity statements contain key concepts. The concepts that researchers define at the end of introductory sections typically become codes during analysis, although researchers may not label the concepts as codes either in the introductory section or in the methods section. I am unsure why such labeling has not become routine. When concepts carry the label *code*, this clarifies where codes come from. Without naming codes and stating where they come from, much of analysis is mystified. Many reports read as if the codes appear out of nowhere during analysis. Even Glaser’s (1978) notion of theoretical sensitivity mystifies the origins of codes. How, for example, do researchers become theoretically sensitive? What if researchers are beginning their scholarly careers? How theoretically sensitive are they (Covan, 2007)? What are the implications for the quality of the analysis?

Research Questions, Hypotheses, and Definitions

The final part of the introductory section of a research report is devoted to research questions, hypotheses to be tested (if any), and definitions of core concepts. In general, in qualitative research, hypotheses are statements of relationships between concepts. Theories usually are composed of two or more hypotheses, although, at times, some researchers may use the term *theory* to designate a single hypothesis (Gilgun, 2005*b*). Concepts are extractions from concrete data. Sometimes concepts are called *second-order concepts* and data *first-order concepts*.

Research questions may be absent. In their place are purpose statements that make the focus of the report clear. Hypotheses are rarely present in qualitative research. When they are, the purpose of the research is to test them and typically to develop them more fully. This type of research has in the past been called *analytic induction* (Gilgun, 1995*e*), whereas a more up-to-date version of qualitative hypothesis testing and theory-guided research is called *deductive qualitative analysis* (Gilgun, 2005*d*;

2013). Analytic induction and deductive qualitative analysis are part of the Chicago School tradition.

Methods Section

Most methods sections for reports based on qualitative approaches have the same elements as any other research report. Descriptions of the sample, recruitment, interview schedule, and plans for data analysis are standard. The APA manual provides guidelines (American Psychological Association, 2009) that fit many types of qualitative research reports. However, reports based on autoethnographies, poetry, and performances may have brief or no methods sections. As is clear by now, the report's contents depend on the purposes and methodologies of the research and on the editorial requirements of journals.

Accounts of Methodologies

In writing up qualitative research, methods sections usually contain a brief overview of the research methodology, which is the set of principles that guided the research. The following is an account of the methodology used in a research report on cancer treatment in India:

For this project we drew upon interpretive traditions within qualitative research. This involved us taking an in-depth exploratory approach to data collection, aimed at documenting the subjective and complex experiences of the respondents. Our aim was to achieve a detailed understanding of the varying positions adhered to, and to locate those within a broader spectrum underlying beliefs and/or agendas. (Broom & Doron, 2013, p. 57)

Sometimes, statements of methodology are much more elaborate, but in research reports, such a statement is sufficient, again depending on the editorial policies of particular journals. A few citations, which this article had, round out an adequate statement of methodology.

However, many reports are written in a clear and straightforward way with scant or no account of methodologies. Examples are the work of Eck (2013) and Spermon, Darlington, and Gibney (2013). These kinds of well-done write-ups might eventually be considered generic. Spermon et al. said their study was phenomenological, which sets up assumptions that the report will be primarily descriptive. In actuality, the intent was to develop theory. Such mixing of methodologies may be the wave of the future; in many ways, distinctions between phenomenological studies whose purposes

are descriptive and those whose purposes are to build theory are blurred. Such blurring may have been the case for decades because it is possible and often desirable to build theories based on phenomenological perspectives; that is, in-depth descriptions of lived experience. However, authors are wise to state in one place what their methodologies are and how they put them to use, such as for descriptive purposes or for theory-building.

Description of Sample

Placing descriptions of sample size and the demographics of the sample in the methods sections is typical. As mentioned earlier, evaluation of sample size depends on the depth and breadth of the study. The more depth a study has, the smaller the number of cases can be. The more breadth and the sharper the focus, the larger sample sizes typically are. Samples on which a study is based must provide enough material on which to base a credible article. A sample size of one may be adequate if researchers show their work demonstrates the basic principles of almost all forms of qualitative research: perspectives of persons who participate in the research, researcher immersion into the settings or the life stories of persons interviewed, multiple perspectives, contextual information of various types, and applications. Autoethnographies often have an *n* of one, but joint autoethnographies are possible. Ethnographies may not give a sample size, as was the case in the performance ethnography of Valásquez (2011) who wrote in the first person about her experience with scientology. In her first-person ethnography, Irvine (2013) also did not mention sample size. She said that the narratives she used for the article were from a larger study on the meanings of animals to people who have no homes. She did not describe the usual demographics of age, gender, social class, and ethnicity.

Most articles describe the demographics of the sample. In a recently accepted article (Gilgun & Anderson, 2013), I saw no relevance in mentioning the size of the larger sample from which we drew in order to tell the stories of how mothers responded to their learning that their husbands or life partners had sexually abused their children. We included an exact count of the larger sample because we assumed that it would be the journal's expectations. We also gave particulars of the demographics. Except for social class and ethnicity, we saw little relevance for the other descriptors. These status variables were relevant to us because most of the sample was white and middle or upper class.

This is important because much research on child sexual abuse is done with poor people, and there are stereotypes that poor families and families of color are more likely to experience incest than are white middle and upper class families. Overall, as with some other issues related to writing, the adequacy of the sample description depends on the methodological principles of the research and the journal's editorial policies.

Recruitment

Accounts of recruitment procedures are important because researchers want to show that their work is ethical. Respect for the autonomy or freedom of choice of participants needs to be demonstrated. In addition, often the persons in whose lives we are interested have vulnerabilities. To show that the research procedures have not exploited these vulnerabilities is part of ethical considerations. Most articles have these accounts. Furthermore, when there are accounts of recruitment procedures, it becomes obvious why the sample is not randomly selected. Irvine's (2013) account of recruitment is exemplary. She recruited through veterinary clinics that took care of the pets of homeless persons. She did not approach potential participants herself. Doing so risked making refusals difficult. The staff informed persons of the research and its purposes. If individuals said they were interested, they gave permission for the staff to give their names to researchers. The research interviews took place in the clinics.

The ethics of recruitment revolve around values, such as respect for autonomy, dignity, and worth. Other ethical issues that are important to mention in reports include the use of incentives for participation. Although many human subjects committees now require monetary incentives for participation, this has ethical implications. Irvine (2013) solved this by giving gift cards after the interviews were completed. Reports on ethical issues have a place in methods sections.

Data Collection and Analysis

Accounts of data collection and analysis are part of the methods section. Data collection procedures should be detailed for many reasons. Primary among them is the need for transparency in terms of the ethical standards the researchers followed, as well as the need to allow for replication of the study. Such details also provide guidelines for others who might be interested in using the methods. In addition, there are many different schools of thought and procedures for each of the methods

used with the three general types of data collection: interviews, observations, and documents. It is helpful to state which particular data collection procedures the researchers used. Researchers often provide examples of the kinds of questions asked and procedures used for recording observations and excerpts from documents. Some researchers may omit such an accounting, as with some autoethnographies and articles that turn research material into performances.

How researchers analyzed data is part of the methods sections. As with data collection, there are so many types of analysis that researchers need to describe the particular forms that they used. For figuring out how to report on data analysis, researchers would do well to study articles in journals in which they want to publish. Irvine (2013) used a method of analysis I have never heard of called "personal narrative analysis" (p. 8). She gave enough detail to provide the general idea of what she did and a sufficient number of citations for additional information.

The level of detail can vary. In some sociology journals, for example, researchers may say little about analysis and sometimes little about data collection. This is because the journal editors, reviewers, and those who publish in and read the articles have assumptions that they for the most part take for granted. Even in these journals, however, researchers may want to account for their analytic procedures, especially if they are writing on topics outside of what is usual in such journals.

Other journals require a great deal of detail. In those instances, researchers first decide what they think is essential and then shape their accounts to fit what appears to be usual practice in the journal. The following paragraphs describe data analysis in a recently accepted article on signs of child sexual abuse in families (Gilgun & Anderson, 2013).

DATA ANALYSIS

In the analysis of data, the first author read the transcripts multiple times and coded them for instances related to disclosures of child sexual abuse and associated signs of the abuse, such as how and when the women first learned of the abuse or suspected it was occurring in their families, their responses, and their reflections on the signs of abuse they might have missed, as well as child and perpetrator behaviors that they did not realize were related to child sexual abuse. Their initial and longer term responses and reflections were also coded. The second author independently read and coded

about one-third of the transcripts using this coding scheme to arrive at a 100 percent agreement.

Sources of the codes were our professional experiences in the area of child sexual abuse, the review of research, and the first author's familiarity with the content of the interviews because she had been the interviewer. These codes served as sensitizing concepts, which, as Blumer (1986) explained, are ideas that guide researchers to see aspects of phenomena that they might otherwise not notice. Although altering researchers' ideas to what might be significant serves an obvious useful purpose, sensitizing concepts might also may blind researchers to other aspects of phenomena that might be important. Therefore, we also used negative case analysis, which is a procedure that guides researchers to look for aspects of phenomena that contradict or do not fit with emerging understandings. In this way, researchers are positioned to see patterns, variations within patterns, exceptions, and contradictions in findings (Becker et al., 1961; Bogdan & Biklen, 2007; Cressey, 1953; Lindesmith, 1947).

As we wrote this section, we were aware of the limited space that we had to fill. Yet we were committed to accounting for where our codes came from for reviewers and editors who may be unfamiliar with pre-established codes. As discussed earlier, many reports are written as if codes appear by magic. We decided that, in this report, we would be as clear as possible about where our codes came from. We also reasoned that we would have to call on the authority of well-respected methodologists if reviewers and editors had questions about what we had done. Furthermore, we were aware of the dated nature of the references; we could do nothing about that because there has not been much written recently about pre-established codes. I have written about this quite a bit, but as one of the authors, I not only had to be anonymous during the review process, but I could not be the sole authority.

Generalizability

Many reviewers and editors have questions about the generalizability of the results of qualitative research. Authors themselves sometimes question the generalizability of their own findings. That's why it remains important to provide clear guidelines in research reports about how the authors view the usefulness of their findings. The following ideas may be helpful to authors as they write their reports and to reviewers who are positioned as gatekeepers. The results of qualitative research are not meant to be generalized in a probabilistic sense. But

because dropouts and refusals limit the randomness of samples, most forms of research can't be generalized in a probabilistic sense.

Conversely, as Cronbach (1975) wrote almost forty years ago, the results of any form of research are working hypotheses that must be tested in local settings. Thus, the applicability of qualitative or any other kind of research can be demonstrated only through attempts at application. Do the findings illuminate other situations? Do the results provide researchers, policy makers, and direct practitioners with ideas on how to proceed? Those who apply the research expect to have to adjust findings to fit particular new situations. Many researchers and some journal editors and reviewers know through common sense and everyday experience how to use the results of qualitative research. Our personal lives are extended case studies. What we learn in one situation, we carry over into another. We know we have to test what we have learned in past situations for fit with new situations. If we do not, we impose our ideas on situations that may demand new perspectives. This common practice of applying results to all situations is disrespectful of local conditions and autonomy of persons. We want to avoid such disrespect in how we suggest readers use the results of our research.

Trustworthiness and Authenticity

Pointing out the trustworthiness of procedures and the findings that result from them sometimes are parts of methods sections. Related to trustworthiness are issues of authenticity (Guba & Lincoln, 2005). Both trustworthiness and authenticity arise from immersion, seeking to understand the perspectives of others in context, reflexivity, and seeking multiple points of view. Researchers who have applied these principles will produce reports that are trustworthy and authentic. In addition, the reports will have grab. Extended discussions related to these issues are beyond the scope of this chapter and the scope of research reports as well.

I get more requests for revisions of methods sections, especially for accounts of data collection and analysis, than for any other parts of a manuscript. This is not surprising, given the multiple possible variations. I never know who the reviewers will be and what their expectations are. I rely first on my beliefs about what I want in the procedures section and then I study articles the journal has already publishes. I include what journal editors appear to expect, but I also add information that I think is important, even when it is not part of what I see in methods sections.

Findings Sections

Findings sections in research reports include both descriptive and conceptual material. Descriptive material is composed of researchers' paraphrasing and summarizing of what they found and excerpts from interviews, fieldnotes, and documents. The descriptive material, at its best, is detailed and lively; it not only is informative, it has grab. This material contributes to understandings of human experiences in context. In addition, descriptive material is the basis of researchers' theorizing and it also provides documentation and illustrations of assertions that researchers make.

Conceptual material comprises the analysis and is made up of inferences such as the general statements, concepts, and hypotheses that researchers develop from the material (data). One way to think about the relationship between descriptive and conceptual material is to think of descriptive material as composed of first-order concepts and conceptual material as composed of second-order concepts. Each type depends on the other. Credible conceptual material is based on descriptive material, some of which is contained in the article. Qualitative research yields mountains of data, a fraction of which can be placed into a published article.

As with other sections of research reports, findings sections have many possible variations that depend on the purpose of the research and the methodologies on which the research is based. Thus, the findings can range from heavily descriptive to heavily conceptual. Heavily conceptual research reports arise from research whose purpose is theoretical, in which researchers set out to test, refine, reformulate, or develop theory. Theoretical reports require some descriptive material to show the basis of theoretical statements, but they are often relatively short on descriptive material.

Reports that are primarily descriptive are composed of excerpts from data. Theoretical material appears in often subtle ways, such as in the form of concepts that organize findings. Irvine's (2013) study of homeless people and their pets is largely descriptive, composed of excerpts from the interviews and Irvine's paraphrases and narration of what she did, how, and when. The findings were narrative case studies based on interviews and observations. The details of the narratives were vivid and had the kind of grab that Glaser (1978) recommended. They showed multiples perspectives and variations on what it meant to homeless informants to have pets in their lives. The first three pages were a review of relevant literature and a presentation of method. The last five pages were a discussion of the findings.

As lengthy as the descriptive material is, conceptual material frames the entire report. In the literature review, Irvine introduced notions of positive identity, generativity, and redemption. She used them to analyze her data and organize findings, which were the narrative case studies. She used the concept of redemption as the core or organizing concept, going into some detail about how the research material supports the significance of this idea of pets as redemptive for homeless people.

This analysis is based squarely on the descriptive material. For instance, Irvine wrote that in the stories she presented in her article, "animals provide the vehicle for redemption." She illustrated this point with a quote from one of the narratives and then reminded readers that the narratives "contain variations on the theme" of "*life is better because this animal is in it*" (p. 20; emphasis in original). Readers do not take this on faith because the basis of this general statement in presented multiple times in the case studies. Irvine has much more material on which she based these ideas, but there is not enough room in a journal-length article to show all of her evidence.

An example of an article that is theoretical in purpose and short on descriptive material is found in the work of Cordeau (2012). She developed a grounded theory of the "transition from student to professional nurse" when student nurses work with "mannequins as simulated patients" (p. 90). Based on interviews, observations, and reports that the students wrote on their clinical experiences, the study was composed of about 10 percent descriptive material. This material included excerpts interviews and student reports. In the results section, she used this descriptive material to illustrate and possibly document the grounded theory she constructed. The theory's "core category" was "linking," which had four components, called properties. She documented the properties, primarily with her own thinking about her research material and also with excerpts from interviews, observations, and student reports.

Like Irvine's (2013) study, the purpose of Cordeau's (2012) work was applied where she wanted to build theory that would contribute to the development of clinical expertise in nursing students. She also devoted about one page of her study to applications.

Core Concepts

I've previously provided an extended discussion of core concepts. This section highlights some key

points and illustrates them. Core concepts, often called *core categories*, organize findings. I prefer the term *concept* because *concept* is the term used in discussing theory, such as “concepts are the building blocks of theory,” and theory is one of several possible products of qualitative research. Researchers decide on which concepts are core in the course of analysis. Researchers are ready to write up their reports when they have settled on, named, and dimensionalized one or more core concepts. The terms “core concepts” and “core categories” are associated with grounded theory (Charmaz, 2006; Corbin & Strauss, 2008), but they are useful in other types of qualitative research, such as interpretive phenomenology and narrative analysis. Core concepts both organize findings and, typically, bring together a great deal of information. The term “dimension” means that researchers account for as many aspects of the core concepts as they can in order to show the multiple perspectives and patterns that typically compose concepts.

In reporting on core concepts, I recommend that researchers name them, introduce them, describe them using excerpts from the research material, comment on them, and then situate each of the concepts and their commentaries within their scholarly contexts. As discussed earlier, this shows how the findings fit with what is already known, or add to, force modification of, or refute what is known. Although many researchers, do not situate findings in their scholarly contexts, they usually cover the other topics.

No matter how authors report findings, they should do so with grab. An example of a report exemplary for its grab is the work of Scott (2003) on what it means to be a professional with a physical disability. Scott began her article not with a literature review but with three reviewer comments on other articles she had written. She then stated that the present article was a response to these comments. She followed up with a description of three male students who waited to speak to her after class about her disability and the notion of embodiment that she discussed in class. She brought in related literature throughout the article. Through her own reflections, reports on how others have responded to her, reports on the accounts that three other women with disabilities gave to her as a person with cerebral palsy, and her literature review, Scott not only showed the meanings of disabilities to persons who have them, but also what others say about their own disabilities, what some people who are able-bodied

say about women with disabilities, and how all of this connects to what is known about disabilities and to wide-spread beliefs about disabilities. Her article is full of grab, such as the header that read, “The Day I Became Human.” With the authors’ own experience as the centerpiece, this article exemplifies write-ups that demonstrate the meanings of lived experience in various contexts, immersion, grab, and implications for social action. The analysis she presented as part of her findings is exemplary.

In the production of quality research, no matter the type of write-up, there are no short cuts. Research reports based on poetry, for example, are held to the same standards as any other article: grab, immersion, lived experience in context, and implications for action. In addition, such research reports typically locate themselves within social and human sciences traditions. Furman’s (2007) reflections and analysis of poetry that he wrote over the course of many years provide an example of how poetry can be used in qualitative analysis. This kind of research is a type of document analysis. In performance studies, researchers create a theater production of informant’s accounts of their experiences whose purpose is to transform audiences and move them to action (Saldaña, 2003). The performances are the equivalent of research reports and when they are effective, they have the four characteristics of qualitative research under discussion.

Discussion Sections

In traditional research reports, the discussion section follows the results section. In discussion sections, authors reflect on findings, including what the findings are, how findings contribute to understandings of phenomena of interest, the lines of inquiry the results open up, and implications for policy and practice. Other generic topics to consider are those related to the focus of the journal. For example, if the journal’s focus is related to health, then authors show how findings are related to health.

Discussion sections present the author with opportunities to advocate for how his or her research can be used. The applied purposes of Irvine’s (2013) research come through when she devoted an entire page to make observations about implications. She pointed out how her research contributes to a transformation of images of homeless persons as isolated to images of them as engaged in relationships not only with their pets but with other persons, too. She noted that rehousing homeless persons requires a change in policy that would allow them to have

pets. Furthermore, she said that caring for a pet “can turn things around” (p. 24).

In the discussion section I wrote with Anderson (Gilgun & Anderson, 2013), we addressed methodological issues, such as the probable existence of other patterns in addition to those we identified and the non-random nature of our sample. We also acknowledged the difficulties in working with families in which child sexual abuse has occurred. Since qualitative researchers want to understand lived experiences, we had to prepare ourselves to deal effectively in research areas that are difficult emotionally for us as researchers. Although we may acknowledge the emotional challenges of some topics in reflexivity statements, discussion sections are opportunities for authors to acknowledge the difficulties of using the results we produce. In the article I wrote with Anderson, we made such an acknowledgment, one that we hoped would facilitate more effective practice. We wrote

Practitioners themselves may experience shock, rage, and disgust. The practice of neutrality, in its therapeutic sense, is important in these cases (Gil & Johnson, 1993; Rober, 2011). Neutrality means that practitioners maintain their analytic stances while at the same time they remain attuned not only to service users but also to themselves. When practicing neutrality, service providers regulate their own emotional responses in order to remain emotionally available to service users. Neutrality also means that service providers remain open-minded so that they can hear stories that they may not expect to hear; in other words, to make room for the unexpected (Rober, 2011). Attunement to inner processes is a form of reflection that can facilitate the development of trust between service users and providers. When providers are reflective, they are less likely to tune out, close down, and otherwise stop listening to what services users express. When they listen and hear what service users say, they are more likely to facilitate the best possible outcomes in difficult situations (Weingarten, 2012).

Doing research on lived experience can be difficult for informants and for researchers. Acknowledgment of the implications of these difficulties for users of the research has a place in discussion sections.

Conclusion

In summary, most articles are fairly straightforward in their write-ups: focused literature reviews, reflexivity statements in many cases, clear statements of purpose, clarity about sources of research questions

and/or hypotheses, identification and definition of key concepts, identification of codes the researcher develops from literature reviews and reflexivity statements, succinct accounting of methods, and findings organized logically by core concepts around which the researcher organizes the multiple dimensions of those concepts. Excellent writing makes articles interesting and accessible. Some kinds of write-ups deviate from these components, but they are held to the same standards of immersion, experiences in context, multiple perspectives, and implications for action and other applications. When authors have the good fortune to have a recommendation to revise and resubmit, suggestions for revisions often improve the quality of the article.

The seemingly endless variations that are possible in the write-up of qualitative research makes writing and reviewing manuscripts challenging, especially when compared to traditions in which rigid rules prevail. However, it is important that approaches to qualitative research continue to evolve to meet with our ever-changing understandings of human phenomena. The clarity and transparency of reports are the fundamental guidelines for making judgments about quality. I often tell my students that the guidelines for doing qualitative research are flexible, and what is important is to be clear about what you did, why you did it, and what you came up with.

The notion of grab is central to write-up. Since qualitative research seeks to understand lived experiences, it is logical that findings report on the lived experiences in vivid terms, replete with quotes from data. This is not to undermine the importance of analysis, but grab is possible even in write-ups that require a great deal of analysis. Grab becomes possible because researchers must provide the evidence for the theories and concepts they develop.

When there are questions about priorities related to informants' voices, researchers' interpretations, and prior research, I hope that authors, reviewers, and editors remember that as important as analysis and previous work may be, the voices of informants bring these other important parts of manuscripts to life. Researchers make decisions about whose voices take priority.

There is no one way to respond to these dilemmas. Authors must make their own decisions about what is important to them and then search for journals that will welcome what they want to convey. It's important to consider pushing the boundaries and writing an article in a way that the researcher thinks will best convey his or her findings.

The importance of quality data, quality analysis, and “grab” are foundational. I began this chapter with a discussion of the balance between description and analysis. I then considered core concepts as organizers of findings, the place of literature reviews, styles of presenting methods and methodologies, and the balance between the voices of informants and researchers. I concluded with the many variations in types of reports that result from the various purposes that qualitative research projects can have. There are many different types of qualitative research and many styles of write-ups. This chapter may sensitize readers to enduring issues in the writing of research reports. Like qualitative research itself, there are multiple points of view on how to write up qualitative research.

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Evaluating Qualitative Research

Jeasik Cho *and* Allen Trent**Abstract**

This chapter addresses a wide range of theories and practices related to the evaluation of qualitative research (EQR). First, six categories of EQR are presented: (1) a positivist category, (2) Lincoln and Guba's alternative category, (3) a "subtle-realist" category developed by Hammersley and Atkinson, and Seale, (4) a general EQR category, (5) a category of post-criteriology, and (6) a post-validity category. Second, evaluation strategies for EQR are offered by providing a variety of actual examples. Third, the chapter discusses a path forward for EQR that includes both internal and external elements. The chapter concludes with a holistic view of EQR needed to collectively construct/confront inner and outer challenges to qualitative paradigms in the twenty-first century. Twenty-first century criteria supported include thought-provoking ideas, innovative methodology, performative writing, and global ethics and justice mindedness.

Key Words: Evaluation criteria, validity, checklists, rubrics, politics of evidence, twenty-first century criteria

Quality is elusive, hard to specify, but we often feel we know it when we see it. In this respect research is like art rather than science.

– Seale, 2002, p. 102

Criteria in the 21st century are not one-dimensional.

– Lichtman, 2006, p. 197

We feel exactly the same way that frontier scholars of grounded theory Juliet Corbin and Anselm Strauss (2008) feel regarding the evaluation of qualitative research:

I feel paralyzed, unsure of where to begin, or what to write. As I search the literature, I find that evaluation is necessary but there is little consensus about what that evaluation should consist of. Are we judging for "validity" or would it be better to use terms like "rigor"... "trustworthiness"... or "goodness"... or something called "integrity"... when referring to qualitative evaluation? (p. 297)

Let us select the term *validity*. "Validity has been referred to many ways, including successor validity, catalytic validity, interrogated validity, transgressive validity, imperial validity, simulacra/ironic validity, situated validity, and voluptuous validity" (Altheide & Johnson, 2011, pp. 584–585), and a review of the qualitative literature tells us that there are many more definitions.

Why is it so hard to get started "evaluating" qualitative research? Patton (2002) notes that "some of the confusion that people have in assessing qualitative research stems from thinking it represents a uniform perspective, especially in contrast to

quantitative research. This makes it hard for them to make sense of the competing approaches within qualitative inquiry” (p. 543).

So, to evaluate qualitative research, shall we simply follow Altheide and Johnson’s (2011) lead? In their chapter in the *Sage Handbook of Qualitative Research*, entitled “Reflections on Interpretive Adequacy in Qualitative Research,” their approach was threefold: they updated their well-known article “Criteria for Assessing Interpretive Validity in Qualitative Research” (Altheide & Johnson, 1994), they called their ideas about this job “analytical realism,” and they proposed an “evidentiary narrative” embedded in “a symbolic interactionist perspective” (p. 582) that goes against neo-positivist, scientific, or evidence-based research. We are impressed with their deep philosophical, provocative ideas on developing a new grand quality criterion in response to the current scientific, evidence-based movement that devalues an ideal of qualitative research, but we are more interested in exploring a broader sense of evaluative criteria in qualitative research. We call our approach *evaluating qualitative research* (EQR).

By and large, we are baffled by at least three issues regarding the evaluation of qualitative research: little agreement with the nature of evaluation in qualitative research, a continuous impact of traditional positivist evaluation criteria on qualitative research, and a broad political discourse on the politics of evidence. At least, however, we agree with Schwandt’s (2002) viewpoint that constructing an evaluation lens that involves general and specific accounts of what we might hope to find in a good study is exciting intellectual work. Schwandt’s four general approaches to evaluating qualitative research are to use (1) universal conventional criteria, (2) alternative criteria of trustworthiness and authenticity, (3) pragmatic criteria, and (4) subtle realist criteria of validity and relevance. Although we are impressed with his scheme for a developmental perspective on EQR, our feeling is that this kind of framework is, by itself, something like recreating what has already been deemed disagreeable in this field.

Despite the field’s confusion, disagreements, and our perplexed reaction, our thesis on EQR in this chapter is clear. We express a very simple but meaningful perspective on the evaluation of the processes and products of qualitative research. Our perspective is threefold. First, because we observe that EQR is seen as a relatively cohesive discourse (e.g., a huge number of journal articles and book chapters start with Lincoln and Guba’s [1985]

seminal book, *Naturalistic Inquiry*, and an equally large number of qualitative studies reference and rely on Lincoln and Guba’s [1985] construction of *trustworthiness* criteria), we want to provide “a sketch of EQR” to categorically describe qualitative differences among many different theoretical and practical ideas related to qualitative research evaluation. Second, we provide several evaluation strategies for EQR. And third, we discuss a path forward for EQR that includes both internal and external elements. We conclude this chapter with a beehive metaphor, which gives a holistic view of the kind of EQR needed to collectively construct, collaborate, and confront inner and outer challenges to qualitative paradigms in the twenty-first century.

Evaluation of Qualitative Research: Six Categories

Under the umbrella of qualitative research over the past three decades, the EQR subfield of study has gradually developed in breadth and depth, along with the blossoming of qualitative inquiry adopted in almost all fields of social science. Relatively speaking, EQR is seen as cohesive because Lincoln and Guba’s (1985) discourse on trustworthiness criteria has been accepted as *the* platform for EQR. Even though these trustworthiness criteria are still considered essential in discussing the quality of qualitative research, different discourses are available.

As talk of paradigm has broadened, the platform for EQR has changed as well (Guba & Lincoln, 1989). Those who operate from post-modern and post-structuralist traditions criticize trustworthiness criteria as another version of traditional or foundational approaches (Scheurich, 1996). Defining *validity* is another issue. Some use the terms “criteria” and “validity” interchangeably, drawing on a philosophical and/or evaluation discourse (Creswell, 2006; Schwandt, 2002; Seale, 1999). Others use validity as a broad epistemological concept to justify an ideal of qualitative inquiry (Lather, 1986). As mentioned earlier, such terms as validity, rigor, trustworthiness, goodness, integrity, and so on are interpreted in many different ways by many different people. Lichtman’s (2006, 2009) position on EQR provides a good explanation:

At this point, I caution you to be careful as you review criteria for judging qualitative research.

Several viewpoints are in play. One group contends that we need to return to research that is more scientific, but I believe that is not necessarily the majority viewpoints. Others see the field as still

in a state of flux... The climate of the world of educational research is such that there is increased accountability and standardization and control. The field has become more politicized than it once was... It is not possible, nor is it desirable, to reach any kind of consensus about what standards should be adopted... the field is not unified... reviewers of journals often embrace a kind of generic criteria. Although they review articles in the health field, the points they make are applicable to education... [Although] the issue of judging, quality, and rigor is very much alive... it is clear that the issue of quality is not yet resolved. (2006, pp. 231–232)

Considering the field's disparity, as well as the seeming urgent need for some sort of resolution, our sketch of EQR is categorical in pointing out qualitative differences among many different theoretical and practical ideas. We present six categories of EQR: (1) a positivist category; (2) Lincoln and Guba's alternative category; (3) a "subtle-realist" category developed by Hammersley and Atkinson, and Seale; (4) a general EQR category; (5) a category of post-criteriology; and (6) a post-validity category.

We hope these categories are a useful and meaningful way of sketching a broad view of EQR. We see the six categories as a map that one can use to start making sense of EQR. This sketched map is our own, and others may see the field of EQR differently. We interpret the field of EQR as evolving at present because choosing a set of evaluative criteria in and of itself is socially constructed and politically driven in nature. Therefore, these six categories should not be interpreted as either hierarchical or linear. Simply put, each is a distinctly different category relying on its own specific criteria (Cho & Trent, 2006; Tracy, 2010). We would like our six categories to be seen as providing a holistic perspective, one that continues to evolve but still moves forward, addressing the complex nature of qualitative research and bringing new insights as we collectively draw a broader picture of EQR.

A Positivist Category

Quality in qualitative research is multidimensional. If quality in quantitative research requires accuracy, precision, rightness, or directness, then quality in qualitative research requires context, locality, properness, and indirectness in addition to those required in quantitative research. This is mainly because qualitative research is value-laden or at least value-related. To help readers better understand our first category, we start with four goals or criteria that

are important to consider in the traditional view of EQR. Simply, advocates of this category see qualitative and quantitative research as the same and so use the same criteria, ones based in quantitative research. In a similar vein, mixed-methods scholars identify a series of evaluation criteria necessary for measuring the product and process of mixed methods research (Dellinger & Leech, 2007; Leech, Dellinger, Tanaka, & Brannagan, 2010; Tashakkori & Teddlie, 2003, 2008). Sale and Brazil (2004) present a review of criteria for critically appraising mixed-methods research. In their review, they give a very comprehensive list of literature that identifies criteria for evaluating quantitative and qualitative methods in terms of the four conventional validity goals: *internal validity*, *external validity*, *reliability*, and *objectivity*.

Lincoln and Guba's (1985) Alternative Category

Perhaps the field of EQR would not be as advanced without Lincoln and Guba's (1985) alternative approach to judging qualitative research. This approach is well known and, as noted earlier, is still greatly influencing the discourse on EQR. In addressing the traditional goals or criteria of internal validity, external validity, reliability, and objectivity seen in the first category, Lincoln and Guba propose *credibility*, *transferability*, *dependability*, and *confirmability*, respectively. In Table 32.1, we briefly explain these parallel goals (Thomas & Magilvy, 2011, pp. 152–154).

A "Subtle-Realist" Category

The subtle-realist approach is pragmatic in nature. British scholars Hammersley and Atkinson (1995) and Seale (1999) make a strong case for the necessity of compromise between various extremes. Their philosophical stance in this regard lies between idealism and realism, claiming that neither of them properly addresses the continuing tension of contemporary research, particularly in ethnography. Seale notes, "The widespread appeal of alternative conceptions of research is based upon some fundamental dissatisfactions with the scientific world view" (p. 7). Those who reside in this camp of thought believe that quality in qualitative research is "a somewhat elusive phenomenon that cannot be pre-specified by methodological rules" (p. 7). That is, those concerned with quality in qualitative research don't necessarily "give up on scientific aims as conventionally conceived, but also draw on the insights of postscientific conceptions of

Table 32.1 Lincoln and Guba's alternative criteria for evaluating qualitative research

Traditional	Alternative	Key Points
Internal Validity	Credibility	The elements that allow others to recognize the experiences contained within the study through the interpretation of participants' experiences; checking for the representativeness of the data as a whole; member checking involving returning to the participants to ensure that the interpretations of the researcher are accurate representations of participants' experiences; peer debriefing; prolonged engagement
External Validity	Transferability	The ability to transfer research findings from one group to another; thick description used to provide the reader with detailed contextual information; transfer of understanding is believed to occur if both contexts are similar
Reliability	Dependability	When other research follows the decision trail used by the researcher; having peers participate in the analysis process
Objectivity	Confirmability	Self-critical attitude on the part of the researcher about how one's own preconceptions affect the research

social research” (Seale, p. x). For them, objectivism is seen as “a resource that can be used productively as an attitude of mind by social researchers” (p. 25). Consequently, the discourse on EQR is not fixed but “open to the possibility that conclusions may need to be revised in the light of new evidence” (p. x). A subtle-realist category that is conceptualized in this pragmatic stance is convergent with the following point of view:

Criteriaology is, at root, an impossible project if it is intended to reflect an internally logical line of argument that simultaneously reconciles philosophical and political positions with the great variety of research practices which people may wish to pursue. The challenge appears to be to construct some general account of what we might hope to find in a good study that is, on the one hand, open enough to include this variety, and, on the other hand, not so loosely specified as to be no value in providing guidance. (Seale, 1999, p. 47)

The relationship between claim and evidence is a starting point for the subtle-realist approach to EQR. Triangulating data, in itself, cannot warrant the credibility of a research report; although triangulation is useful to consider, subtle realists argue that “member validation offers a method for testing researcher’s claims by gathering new evidence” (Seale, 1999, p. 71). The quality of qualitative research results from the degree of members’ involvement, whether weak or strong. Thus, openness to the possibility that conclusions may need to be revised in the light of new evidence is determined

by the extent to which members are involved in the closeness between evidences and claims.

A General EQR Category

As Seale (1999) noted, a dilemma exists for EQR: the field needs a set of criteria broad enough to include a variety of qualitative research traditions. The field of qualitative research is broad in history, paradigms, theories, and practices. Each qualitative research tradition has its own rationale for quality considerations (Creswell, 2006). Although discipline-specific criteria for these research traditions are available, a majority of the literature on EQR attempts to provide general criteria or validity applicable to qualitative research generally. These attempts are likely to be encountered in many research articles, some of which will be discussed in the next section of this chapter. We define this attempt as belonging to a general EQR category that proposes evaluative guidelines intended to assist reviewers or committee members in judging the quality of qualitative research of any type. It could be seen as too general for some particular types of qualitative research and perhaps too specific for others.

A Category of Post-Criteriaology

The post-criteriaology category is seen as radical to some extent because those who reside in this category believe that it is neither desirable to use validity or criteria from the conventional positivist standpoint nor even possible to set up predetermined criteria for qualitative research that uncovers complex meaning-making processes.

Is it possible to devise a set of goodness criteria that might apply to an inquiry regardless of the paradigm within which it was conducted? Or is it the case... that goodness criteria are themselves generated from and legitimated by the self-same assumptions that undergrid each inquiry paradigm, and hence are unique to each paradigm? (Guba, 1988, p. 16, cited in Smith, 1990, p. 168)

Smith (1990) reviewed three alternative paradigms and criteria—post-empiricism or post-positivism, constructivism, and critical theory—and found an overall regulative ideal for inquiry: “objectivity, solidarity, and emancipation,” (p. 183) respectively. His criticism is focused on the assumption that “each paradigm has dispensed with the idea of an absolutely authoritative foundation for knowledge. This nonfoundationalism greatly complicates the criteria issue” (p. 183). There are at least three points common to these different perspectives. First, there is no possibility that a mechanical decision-making procedure can be applied to distinguish valid from invalid research. Second, methodology or procedures, in and of themselves, are not sufficient for making decisions about the quality of inquiry. Finally, although only briefly noted earlier, an appeal to consistently successful prediction is not a live option, in that none of the three perspectives has done very well in this area.

A Post-Validity Category

Before explaining this last category, clarifying the difference between a general sense of credibility used in qualitative research and the theoretical sense of validity used in this section is needed. All the earlier five categories of EQR are more or less direct, straightforward, or less abstract in suggesting ways of judging quality or goodness criteria on qualitative research. The post-validity category has its roots in Patti Lather’s (1986) seminal article, “Issues of Validity in Openly Ideological Research: Between a Rock and a Soft Place,” in which she redefines goodness criteria in ways that make evaluation meaningful for value-based research programs such as feminist research, neo-Marxist ethnography, and Freirian empowering research. She argues that for these research programs to be properly assessed, goodness criteria such as triangulation, construct validity, face validity, and catalytic validity must be built into research designs. That is, critical research programs need accurate data credibility, a researcher’s systematized reflexivity, respect for participants’ interpretation on data (called member-checking), and evidence of participants’ consciousness change.

Later, Scheurich’s (1996) article, entitled “The Mask of Validity: A Deconstructive Investigation,” takes Lather’s value-based research programs a step further, arguing that the conventional approach and Lincoln and Guba’s naturalistic approach are fundamentally similar. That is, the general techniques Lincoln and Guba invented have the same orthodox voices that originated in the positivist paradigm. Social transformational research is validated in ways that require a celebration of the play of multiplicity and difference in data collection, analysis, and interpretation. All in all, EQR in this regard is subject to locality or contextuality, in which meaning is de- or reconstructed toward social justice.

Different Strategies for EQR

Here, having reviewed our sketch of the six general categorical approaches to EQR, we present a series of common strategies for qualitative research evaluation. From the many possible, we select five major strategies for EQR that are different in form and content from one another. In the first, scholars develop a list of criteria or checklist that follows a series of research procedures. In the second, a professional organization sets a high level of research standards. In the third, a reviewer is provided with a rubric or scoring guide to review a journal article. In the fourth, an analysis tool is used to evaluate key aspects of the process and the product of qualitative research. And in the last, we include a set of criteria against which art-based research and performance studies are evaluated.

Ten Commandments

How does one evaluate dissertation studies or journal articles? We find the following list a very typical set of criteria (Cobb & Hagemaster, 1987). We’ll call these the *ten evaluative commandments*:

1. Expertise
2. Problem and/or research question
3. Purpose
4. Literature review
5. Context
6. Sample
7. Data collection
8. Data processing and plans for analysis
9. Human subject
10. Importance to the field

To our knowledge, almost all researchers, scholars, and teachers took an introductory class to learn how to conduct research (Ambert, Adler, Adler, & Detzner, 1995; Burns, 1989; Duncan & Harrop, 2006; Elliott, Fischer, & Rennie, 1999; Forchuk & Roberts, 1993;

Greenhalgh, 1997). What students usually learn is that research goes through a process something like problems → questions/purposes → literature review → context/setting → sample/participants → data collection/display/analysis/interpretation → significance of research. Additionally, students learn about the human subject review process. Reviewing a research project in light of typical research procedures and components is common (Popay, Rogers, & Williams, 1998; Yin, 1999). The following review guideline is used in *The Asian Journal of Educational Research and Synergy*, and it highlights a typical research process using *key evaluative criteria* (this journal accepts both quantitative and qualitative research):

General Considerations

1. Importance and interest to the journal's readers
 - What does the paper contribute to the field of education?
 - Is it significant to the target community?
 - Does it present a new and significant contribution to the literature?
 - Is it timely and relevant?
2. Originality of the paper
 - Is the study innovative? Interesting?
3. What were the author(s) trying to accomplish and were they successful?

Specific Considerations

1. Presentation
 - Does the paper present a cohesive argument?
 - What is the basic logic of the presentation?
 - Are the ideas clearly presented?
2. Writing
 - Is the writing concise and easy to follow?
3. Length
 - What portions of the paper should be expanded? Removed? Condensed? Summarized? Combined?
4. Title
 - Is the title informative?
5. Abstract and introduction
 - Do the abstract and introduction accurately reflect the points made in the paper?
6. Literature review
 - Are the cited articles/papers current?
 - Is the literature review comprehensive?
 - Does the literature review contain a coherent argument supported by literature (as opposed to a list of studies)?

7. Methods for studies involving primary data collection

- Does the author provide enough detail of the methodology?
- Are the methods described clearly enough to facilitate replication (where applicable)?
- Is there a sound research methodology?
- Are the methods appropriate?

8. Data presentation

- Could the design be conveyed more easily?
- Are the data clearly presented?
- Can the reported results be verified easily by reference to tables and/or figures?
- Would another form of presentation help?
- Are illustrations instructive?
- Are all tables and figures clearly labeled? Necessary? Well-planned?

9. Analysis and interpretation

- Does the organization of results promote understanding?
- Are the analyses appropriate and logical? Are they described in enough detail?

10. Discussion

- Are the discussion and conclusions made by the author supported by the data?
- Does the writer understand the limitations of his or her work?
- Is there enough breadth and depth in the implications of his or her study?

This detailed guideline is intended to help a reviewer examine a journal article and is similar to the ten evaluative commandments presented earlier. We find two considerations interesting in this guideline: originality and discussion. The discussion part covers conclusion, limitations, and implications, all of which are worth being assessed. The originality part, expressed as *innovative* or *interesting*, is definitely something important for the reviewer to consider. Arguably, those concerned with a general set of criteria are interested in constructing a checklist inherent in logic, specificity, or thoroughness in form and content. In other words, this kind of checklist-type evaluation strategy is appreciated on the grounds that any research can be assessed in a way that follows a linear sense of logic, specificity, and thoroughness. The next is an example of a review checklist by Clive Seale (1999), who wrote a seminal book about evaluating the quality of qualitative research. Seale organizes his major checklist items in terms of introduction (two criteria), methods (five criteria), analysis (six criteria), presentation (six criteria), and ethics (one criterion),

along with an additional thirty-six subcriteria following these major criteria:

Criteria for the evaluation of qualitative research papers

1. Are the methods of the research appropriate to the nature of the question being asked?
2. Is the connection to an existing body of knowledge or theory clear?

Methods

3. Are there clear accounts of the criteria used for the selection of subjects for study and of the data collection and analysis?
4. Is the selection of cases or participants theoretically justified?
5. Does the sensitivity of the methods match the needs of the research questions?
6. Has the relationship between fieldworkers and subjects been considered, and is there evidence that the research was presented and explained to its subjects?
7. Was the data collection and record keeping systematic?

Analysis

8. Is reference made to accepted procedures for analysis?
9. How systematic is the analysis?
10. Is there adequate discussion of how themes, concepts, and categories were derived from the data?
11. Is there adequate discussion of the evidence both for and against the researcher's arguments?
12. Have measures been taken to test the validity of the findings?
13. Have any steps been taken to see whether the analysis would be comprehensible to the participants, if this is possible and relevant?

Presentation

14. Is the research clearly contextualized?
15. Are the data presented systematically?
16. Is a clear distinction made between the data and their interpretation?
17. Is sufficient of the original evidence presented to satisfy the reader of the relationship between the evidence and the conclusions?
18. Is the author's own position clearly stated?
19. Are the results credible and appropriate?

Ethics

20. Have ethical issues been adequately considered?

To elaborate, under the Methods heading, Seale (1999) addresses typical issues related to procedures,

such as the selection of subjects, theoretical sampling, the relationship between fieldworkers and subjects, and systematic ways of data collection and record keeping. Under the heading of Analysis, he points out basic steps to follow: data analysis procedures (reliability); a degree of systematic analysis; adequate discussion of themes, concepts, and categories; negative case analysis; validity; and checking meaning with respondents. Last, the heading of Presentation discusses a synthesis of data that indicates context-specific, systematic data display; proper interpretation; evidence-based conclusion; the researcher's position; and credible results. Some subcriteria are: *Could a quantitative approach have addressed the issue better? To what extent are any definitions or agenda taken for granted, rather than being critically examined or left open? Has reliability been considered, ideally by independent repetition? Has the meaning of their accounts been explored with respondents? Are quotations, fieldnotes, etc. identified in a way which enables the reader to judge the range of evidence used? Have the consequences of the research... been considered?*

Research Standards and Descriptive/Prescriptive Rating Scales

A rigorous attempt to identify a set of general checklist criteria embedded in a linear sense of logic, specificity, and thoroughness is clearly evident in the recent publication of the American Educational Research Association's (AERA) (2006) Standards for Reporting on Empirical Social Science Research. AERA uses the word *standards* and organizes its checklist under two overarching themes, *warrantability* and *transparency*. Table 32.2 is an excerpt of the AERA research standards, showing the great emphasis placed on analysis and interpretation.

The general research standards in the left column deal with reliability, analysis methods, inference, and conclusion. The specific standards for qualitative research in the right column are focused largely on analysis and interpretation; they are strongly geared toward "being transparent" in the process of developing the descriptions, claims, interpretations, evidence that serves as a warrant for each claim, practices used to develop and enhance the warrant for the claims, and interpretive commentary. Presumably, these two core themes, warrantability and transparency, proclaimed by the world's largest educational research association, have significant impact on the qualitative research community in many ways. Warranted claims and transparent procedures could be construed as political in nature

Table 32.2 Standards for reporting on empirical social science research

General Research Standards	Qualitative Standards intended to make the process of analysis transparent for reviewers and readers
5.1. The <i>procedures used for analysis</i> should be precisely and transparently described from the beginning of the study through presentation of the outcomes. Reporting should make clear how the analysis procedures address the research question or problem and lead to the outcomes reported. The relevance of the analysis procedures to the problem formulation should be made clear.	5.11. The <i>process of developing the descriptions, claims, and interpretations</i> should be clearly described and illustrated. The description should make it possible to follow the course of decisions about the pattern descriptions, claims, and interpretations from the beginning to the end of the analysis process. Sufficient detail should be included to make the process transparent and engender confidence that the results are warranted.
5.2. <i>Analytic techniques</i> should be described in sufficient detail to permit understanding of how the data were analyzed and the processes and assumptions underlying specific techniques (e.g., techniques used to undertake content analysis, discourse or text analysis, deliberation analysis, time use analysis, network analysis, or event history analysis).	5.12. The <i>evidence that serves as a warrant for each claim</i> should be presented. The sources of evidence and the strength and variety of evidence supporting each claim should be described. Qualifications and conditions should be specified; significant counter-examples should be reported. Claims should be illustrated with concrete examples (e.g., fieldnote excerpts, interview quotes, or narrative vignettes), and descriptions of the social context in which they occurred should be provided. If a warranted claim entails a generalizing statement (e.g., of typicality), it should be supported with evidence of its relative frequency. Speculations that go beyond the available evidence should be clearly represented as such.
5.3. The analysis and presentation of the outcomes of the analysis should make clear how they <i>support claims or conclusions</i> drawn in the research.	5.13. <i>Practices used to develop and enhance the warrant for the claims</i> should be described, including the search for disconfirming evidence and alternative interpretations of the same evidence. Significant limitations due, for instance, to insufficient or conflicting evidence, should be described.
5.4. Analysis and interpretation should include information about any <i>intended or unintended circumstances</i> that may have significant implications for interpretation of the outcomes, limit their applicability, or compromise their validity. Such circumstances may include, but are not limited to, key actors leaving the site, changes in membership of the group, or withdrawal of access to any part of the study or to people in the study.	5.14. <i>Interpretive commentary</i> should provide a deeper understanding of the claims—how and why the patterns described may have occurred; the social, cultural, or historical contexts in which they occurred; how they relate to one another; how they relate to (support or challenge) theory and findings from previous research; and what alternative claims or counter-claims were considered.
5.5. The <i>presentation of conclusions</i> should (a) provide a statement of how claims and interpretations address the research problem, question, or issue underlying the research; (b) show how the conclusions connect to support, elaborate, or challenge conclusions in earlier scholarship; and (c) emphasize the theoretical, practical, or methodological implications of the study.	

and have been used in recent years in the name of scientific, evidence-based research by political conservatives typically thought to oppose the use and funding of qualitative research (Denzin, 2012). However, many qualitative researchers appear to endorse the word *transparency* as a newly emerging and important criterion in conducting and evaluating qualitative research.

Table 32.3 is a review form for evaluating AERA annual conference proposals. It addresses the research standards alluded to earlier by specifying warrantability and transparency. The evaluation contents or criteria of this review form are aligned with general research procedures, just like those of checklists, but they are much more descriptive and

prescriptive. The form describes what each research component is like (e.g., perspectives or theoretical framework) and, at the same time, it prescribes what must be expected by a reviewer (e.g., evidence, substantiation or warrants for arguments, and scientific significance). Additionally, it gives a 1–5 rating scale. Typically, reviewers are eventually asked to make a decision. To our knowledge, providing written comments is typical, along with stating a decision that falls within one of four judgmental calls: accepted as is, accepted with minor revision, accepted with major revision, or rejected. The AERA proposal review evaluation form has a binary decision rule—accepted or rejected—and includes comments for both writer and division chair.

Table 32.3 AERA annual conference proposal review form

Objectives or purposes	Min (insignificant) 1 2 3 4 5 Max (Critically significant)
Perspective(s) or theoretical framework	Min (Not well executed) 1 2 3 4 5 Max (Well executed)
Methods, techniques, or modes of inquiry	Min (Not well executed) 1 2 3 4 5 Max (Well executed)
Data sources, evidence, objects, or materials	Min (Inappropriate) 1 2 3 4 5 Max (Appropriate)
Results and/or substantiated conclusions or warrants for arguments/point of view	Min (Ungrounded) 1 2 3 4 5 Max (Well grounded)
Scientific or scholarly significance of the study or work	Min (Routine) 1 2 3 4 5 Max (Highly original)
Comments to the program chair (This field is mandatory; you must comment)	
Comments to the author/submitter (This field is mandatory; you must comment)	
Reviewer Recommendation	
Accept ()	
Reject ()	

Evaluative Rubrics

Table 32.4 is a rubric-type review form for the journal *Multicultural Perspectives*. This journal accepts both quantitative and qualitative work, but mostly includes qualitative research articles.

This evaluation rubric reviews journal articles in the context of multiculturalism (race, gender,

ethnicity, etc.); because multicultural education includes several dimensions that deal with *general thematic criteria*, these differ from generally encountered criteria like questions, purposes, literature, analysis, and conclusion. Given a number of different notions of multiculturalism and multicultural education, this journal’s evaluation rubric adopts

Table 32.4 A review form used in the *Journal of Multicultural Perspective*

Rating Dimension	Ex	G	M	W	Comments
Significant Topic					
Clear Purpose and Scope					
Provocative Content (new and thought-provoking)					
Analytical (theoretical, empirical, conceptual, philosophical)					
Organized and Focused					
Clear and Comprehensive					
Conclusions Valid					
Interesting Reading					
Appropriate for <i>Multicultural Perspectives</i>					
Written Comments					

Directions: Place an “X” for each dimension: *Ex* = Excellent; *G* = Good; *M* = Marginal; *W* = Weak. Jot notes in the “comments” section and incorporate these into the narrative.

such general thematic criteria as provocative content (new and thought-provoking) and organized/focused, clear/comprehensive, or interesting reading, along with commonly addressed criteria such as significant topic, clear purpose/scope and methods, and appropriateness to the journal. This review rubric, or general thematic rubric, with its nine dimensions/criteria, not only assists reviewers in evaluating broad ranges of research articles submitted to this interdisciplinary journal, but also seeks a high level of article quality by emphasizing strong qualitative evaluation criteria (e.g., “new and thought-provoking”).

Criteria: By, For, and Of the Readers, Participants, and Investigators

In the matter of evaluating content and form, we have thus far examined a series of criteria set forth in checklists, standards, and rubrics. We would like to draw attention to another, different form of evaluation. If the previous strategies and discussions on determining the inclusion of evaluation criteria are straightforward and directive in terms of what qualitative research is like and how it proceeds, then the argument that Stiles (1999) makes is insightful and relational:

The concept of objectivity is replaced by the concept of *permeability*, the capacity of understanding to be changed by encounters with observations. Investigators argue that we cannot view reality from outside of our own frame of reference. Investigator bias can be reframed as *impermeability*... Good practice in reporting seeks to show readers how understanding has been changed. The traditional goal of truth of statement is replaced by the goal of *understanding by people*. Thus, the validity of an interpretation is always in relation to some people, and criteria for assessing validity depend on who that person is (e.g., reader, investigator, research participant). (p. 99; emphasis in original)

To elaborate, according to Stiles (1999), EQR involves two sets of judgments on quality: *good*

practice criteria and *validity criteria*. Here, we briefly explain the first: judgmental quality criteria. It is likely that all sorts of criteria mentioned in the previous types of evaluation thus far are convergent with what Stiles refers to as good practice criteria in light of the investigator’s choice, sound analytical practices, and disclosures of the investigator’s forestructure. Some example criteria include: “Are research questions clearly stated? Are prolonged and persistent observation made? Did the investigator make a disclosure of his or her orientation or assumptions?” (p. 99). These judgmental criteria and their subcriteria are intended to evaluate the degree of what is generally called “credibility” or claims of truthfulness.

What makes Stiles’s (1999) strategy unique in the matter of EQR is the “validity criteria” (p. 100) that are mainly concerned with who is impacted by the researchers’ interpretations and how the impact of interpretation is utilized and for what purpose. The table of analytic evaluation developed by Stiles is seen in Table 32.5.

The 3x2 grid analysis tool in Table 32.5 involves three different stakeholders and two different purposes of interpretation. For example, if the purpose of interpretation is to determine readers’ agreement with regard to what is found in the research, then one major criterion should be *coherence*, which includes follow-up questions like “Is the interpretation internally consistent? Is it comprehensive?... Does it encompass all of the relevant elements and the relations between elements?” (p. 100). If the purpose of interpretation is to make readers rethink their existing belief system, then they should have *revealing* or *self-evident* learning experiences as they read a text. Subquestions related to this level of evaluation include “Is the interpretation a solution to the concern that motivated the reader’s interest?... Did it produce change or growth in the reader’s perspective? Did it lead to action?” (p. 100).

At the level of evaluation criteria to be applied to research participants, the major criterion is *testimony*, which allows participants to express their voices from

Table 32.5 Types of validity in qualitative research

Impact of interpretation on preconceptions or bias		
Group of people	Fit or agreement	Change or growth
Reader	Coherence	Uncovering; self-evidence
Participants	Testimonial validity	Catalytic validity
Investigators	Consensus; replications	Reflexivity validity

their own perspectives. Follow-up questions are “Did participants indicate that the interpretation accurately described their experience?... Were their reactions to hearing the interpretation consistent with the interpretation’s motifs? Did they reveal fresh and deeper material?” (Stiles, 1999, p. 100). *Catalytic validity*, one of Guba and Lincoln’s (1989) five authenticity criteria, is used if the purpose of interpretation is to empower the participants’ life worlds and to have them “take more control of their lives” (p. 100). This catalytic validity is also more purposefully and critically used in emancipatory social science research (e.g., feminist research, neo-Marxist critical ethnography, and Freirian research). In effect, Lather (1986) radically redefines catalytic validity as indicating “not only... a recognition of the reality-altering impact of the research process itself, but also... the need to consciously channel this impact so that respondents gain self-understanding and, ideally, self-determination through research participation” (p. 67).

Criteria for Art-Based Research and Performance Studies

In recent years, art-based researchers have proposed six evaluative criteria. Barone and Eisner (2012) note with some critical comment on existing inquiry into EQR that “employing a quantitative metric enables one to enumerate or to summarize quantity... Criteria [for arts-based qualitative work] are much more slippery” (Barone & Eisner, 2012, p. 147). With specific art-based evaluative criteria in mind, they propose the following set of criteria:

- *Incisiveness*: The degree to which research gets to the core essence of a social issue; Barone and Eisner (2012) assert that incisive research: “offer[s] the potential for waking the reader up to a strange world that appears new and yet always existed in the shadowy corners of the city that they had never explored on their own” (p. 149)

- *Concision*: The degree to which research occupies the minimal amount of space; “any additional material simply diminishes the capacity of the piece to achieve that purpose, waters down the power of the work, and hence its effectiveness” (pp. 149–150)

- *Coherence*: The creation of a work of arts-based research whose features hang together as a strong form (pp. 150–151)

- *Generativity*: The ways in which the work enables one to see or act on phenomena, even though it represents a kind of case study with an n of only 1 (pp. 151–152)

- *Social significance*: Something that matters, ideas that count, important questions to be raised (p. 153)

- *Evocation and illumination*: Feeling or defamiliarizing an object so that it can be seen in a way that is entirely different from the ways in which customary modes of perception operate (p. 154)

Barone and Eisner (2012) add that these six criteria should be seen as “a cue for perception” (p. 154), one that assists observers or audiences in making a better evaluation of an art product. Therefore, they offer these criteria merely as a starting point for thinking about the appraisal of works of art-based research. Getting locked into criteria that constrain innovation and dampen imagination is undesirable. As with the other scholars mentioned earlier, Barone and Eisner take a deliberative, balanced perspective on EQR. Barone and Eisner assert,

We do not believe that we can have an effective arts based research program without some degree of common reflection over what might be attended to in looking at such work. Thus, in a certain sense, we compromise between, on the one hand, common criteria and, on the other, criteria that are idiosyncratic to the work itself. This may appear a dilemma, but it is a reality. (p. 155)

The compromise alluded to is indeed a reality, one that those involved in qualitative research deal with regularly. Seeing qualitative research as art is not new. But following a “recipe” to produce art-based research is like using a recipe to produce a chocolate cake to a particular standard. The problem is that “the more detailed and prescriptive the recipe, the more likely that the cakes made from that recipe will be indistinguishable from one another” (Barone & Eisner, 2012, p. 155). Eventually, Barone and Eisner “invite you, the readers, to use your own judgment in applying these criteria to the examples of the works of arts based research” (p. 155).

Cho and Trent (2009) suggest validity criteria for assessing performance-related studies. Performance is often viewed as an “object” or the presentation of the results of analysis (Hamera & Conquergood, 2006, p. 420). In this view, qualitative researchers think, plan, select, and show through performance their inquiry findings as the last phase of assignment/research project completion. Although Cho and Trent support this traditional role for performance in qualitative research, they claim that their conceptualization is broader, incorporating performance aspects at all stages of the inquiry process.

They acknowledge the meaning of performance both in and as qualitative research because the boundary between performance and qualitative research blurs as researchers/teachers and students/audience or researchers and reviewers come to see “conducting qualitative research” as an inevitably personal, social, and political performative process. They advocate for in-depth dialogues and scaffolding to support audiences’ and other researchers’ introduction to the possibility of constructing and utilizing performance in/ as qualitative inquiry (Hamera, 2006).

Cho and Trent (2009) offer validity criteria for performance that are critically oriented, culturally responsive, and pedagogically sound. The rubric they construct is not only evaluative but also pedagogical in nature (see Table 32.6). The rubric outlines criteria for all three phases of the performative process: pre-, during, and post-performance.

Pre-performance as imaginative rehearsal is an ongoing textual rehearsal process as the researcher finalizes the analysis and interpretation of the data collected. The focus of imaginative rehearsal is on making the voices of subjects relational and evocative as the researcher constructs texts as scripts. Criteria needed to evaluate this imaginative textual practice involve data sufficiency, level of critical interpretation, and degree of script craftsmanship. The stage of performance-in-use, associated with artistic re/presentation, involves transacting the lived experiences of others with audiences by means of the voices and bodies of the performer(s). One of the main criteria is degree of understandability of the performance being re/presented. With clear delivery in mind, this criterion is one that cautions that some performance is too complex to understand. The post-performance stage is nurtured by a co-reflexive member-checking process among

subjects, performers, and audience. It is important to link artistic re/presentation with degrees of intensive experience and closeness between the performer and the audience. Post-performance is seen as a beginning, not an ending, because the effect of a performance on the performer and the audience may be rearranged as both parties share their understandings with one another. The performer should be very clear about his or her rationale for checking validity: Whose authority? Whose artistic achievements? And, whose evaluative validity is of most importance at this time in this place? Which choices promote the primary aim of attaining a deeper, empathic understanding across participants (both performers and audience members)? These co-constructive validity-seeking questions may help audiences reflect critically, not so much on aesthetics at the surface level as on hidden messages underpinning the performance.

Evaluating Qualitative Research: Politics of Evidence for the Twenty-First Century

The criteria for judging a good account have never been settled and are changing. (Clifford, 1986, p. 9)

The question of whether it is possible to measure the value of qualitative research from the standpoint of conventional evaluation criteria has resurfaced. Those who accept a positivist paradigm assume that reality can be objectively measured. These researchers are reigniting the paradigm wars in ways that repeat old arguments in new form. The new focus of the attack is on the shaky nature of evidence drawn from qualitative research. As has been explored throughout this chapter, much scholarship has been focused on EQR in recent decades. As a consequence, more accurate, meaningful

Table 32.6 Validity criteria designed to guide the development, enactment, and assessment of dialogical performance of possibilities

	Pre-performance	During-performance	Post-performance
Process	<ul style="list-style-type: none"> • Imaginative • Textual rehearsal 	<ul style="list-style-type: none"> • Artistic representation • Situated engagement 	<ul style="list-style-type: none"> • Co-reflexive member checking • Caring/empowered/non violent
Major Criteria	<ul style="list-style-type: none"> • Data-sufficiency • Critical interpretation • Script craftsmanship • Multiple voices • Persuasive • Advocacy 	<ul style="list-style-type: none"> • Aesthetic • Dialogical engagement • Understandability • Improvisational • Empathetic/authentic 	<ul style="list-style-type: none"> • Divergent reactions • Focus on major concrete issues • Generation of possible solutions • Co-construction of further questions • Un/learning about social justice • Promotion of continued conversation and action

ways of evaluating qualitative research have been established.

Despite the evolution of robust evaluation frameworks, work remains on at least two fronts. Internally, as a community of qualitative researchers, we need to continue to focus on the purposes of our scholarly work and the ways we legitimize it both within and outside our fields. This, necessarily, is a never-ending conversation, and one in which all researchers should participate. Externally, we need to continue to focus on appropriate responses to those who diminish the rigorously obtained knowledge that results from naturalistic inquiry. Those who prioritize only randomized, generalizable work with numerical findings (despite the inherent associated problems) ignore a robust knowledge base that, pedagogically, has often more to offer than a statistical analysis of decontextualized “data.” This knowledge base presents in narrative form, as stories, and, as humans and inquirers, it is among our most basic ways of knowing. Unfortunately, as we discuss later in this chapter, those who perpetuate paradigm wars also wield a great deal of power in research and policy communities.

Evaluating Qualitative Research: Moving Forward in Contemporary Contexts

Thus far, our review has illuminated the wide variety of approaches to EQR, including wide-ranging epistemological underpinnings, as well as a broad array of strategies and processes for evaluating qualitative work. Still, despite extant models and frameworks, researchers work in dynamic, always changing contexts—socially, personally, and politically. As noted earlier, there is and always will be a need to continue to examine emergent evaluation prescriptions and proposals and to juxtapose these with contemporary evolutions in context and culture. Richardson’s evolving work on this topic provides a good example. Richardson, in 2000, offered five criteria against which to assess the validity/quality of ethnographic texts:

- *Substantive contribution*: Does this piece contribute to our understanding of social life? Does the writer demonstrate a deeply grounded (if embedded) human-world understanding and perspective? How has this perspective informed the construction of the text?

- *Aesthetic merit*: Does this piece succeed aesthetically? Does the use of creative analytical practices open up the text, invite interpretive

responses? Is the text artistically shaped, satisfying, complex, and not boring?

- *Reflexivity*: How did the author come to write this text? How was the information gathered? Ethical issues? How has the author’s subjectivity, as both a producer and a product of this text, been addressed? Is there adequate self-awareness and self-exposure for the reader to make judgments about the point of view? Do the authors hold themselves accountable to the standards of knowing and telling of the people they have studied?

- *Impact*: Does this affect me? Emotionally? Intellectually? Generate new questions? Move me to write? Move me to try to new research practices? Move me to action?

- *Express a reality*: Does this text embody a fleshed out, embodied sense of lived experience? Does it seem “true” —a credible account of a cultural, social, individual, or communal sense of the “real”? (p. 254)

The evaluative questions listed here concerning ethnographic texts can be applied to judging most qualitative texts. As a journal referee, one must be concerned with the degree of contribution, a sense of aesthetics, the level of a researcher’s reflection, the learning of the reader, and indications of credibility. It appears, however, that Richardson’s criteria have changed. When writing with St. Pierre in 2005, Richardson and St. Pierre exclude the last criterion, *express a reality*. There is no explanation as to why the last criterion about credibility is no longer included in this later version.

It is typical for research methodologists to offer a set of evaluative criteria that are claimed to be relevant and necessary based on their theoretical underpinnings. Many of the scholars highlighted in this chapter have done so, and these criteria sets illustrate that some criteria are commonly used, whereas other criteria are used uniquely, depending on the different purposes and uses of the evaluation. Yet, by looking at the matter of EQR from a broader perspective, we may end up concluding that EQR, like other theoretical constructions in social science, is simultaneously contextual, cultural, and political. When a reviewer evaluates a manuscript, the process is individualistic, and it is hard to describe the multiple influences impacting the reviewer’s perspective. These individualistic and hidden meanings used by a reviewer do not necessarily match neatly with a set of criteria provided by a journal editor, colleague, or conference organizer. Assessment tools in this complex process are used for formality,

convenience, and as a standardized means to ensuring fairness in determining contributors. In the end, it is the reviewer's construction of meaning (or lack of) around the text that matters.

By the same token, an inclusion or exclusion of goodness criterion is socially constructed. The earlier noted discrepancy between Richardson (2000) and Richardson and St. Pierre (2005) serves as an example. The omission must be more than random. The co-authors likely included those criteria on which they agreed and co-constructed understandings, and omitted those on which they did not. A consistent theme across both authors' individual and collaborative work is the joining of art and science in the production of qualitative texts. "Science is one lens, and creative art is another. We see more deeply using two lenses. I want to look through both lenses to see 'a social science art form'—a radically interpretive form of representation" (Richardson & St. Pierre, p. 964). Perhaps the qualitative research community accepts these scholars' social science art form, which is similar to what Lather (1986) refers to as "a new rigor of softness... validity of knowledge in process... an objective subjectivity" (p. 78). A social science art form or an objective subjectivity is something that continues to evolve. A constant deliberation on the inclusion and exclusion of criteria in EQR is necessary to better address the changing nature of knowledge and aesthetics in sociocultural contexts.

Evaluation, Criteria, and Power

Scholars continue the conversation about evaluating research. Tracy (2010) presents a recent proposal for a model to ensure "excellent qualitative research." Tracy's model is a solid synthesis of what has been researched and theorized about in recent history. Alternatively, Lichtman's (2006) review of evaluating qualitative research includes *personal criteria*, which are based on her philosophy and assumptions regarding a good piece of qualitative research. Thus, Lichtman attempts to make her personal philosophy explicit by reflecting on the self, the other, and interactions of the self and other. Lichtman argues that "an understanding of the other does not come about without an understanding of the self and how the self and other connect" (p. 192). Then she goes on, "I believe each is transformed through this research process" (p. 192). In contrast, Tracy takes an objective stance in establishing her model's rationale for education establishment power holders:

In addition to providing a parsimonious pedagogical tool, I hope my conceptualization may aid in

garnering respect for qualitative methods from power holders who know little about our work. Despite the gains of qualitative research in the late 20th century, a methodological conservatism has crept upon social science over the last 10 years... evidenced in governmental and funding agencies' preference for research that is quantitative, experimental, and statistically generalizable... High ranking decision makers—in powerful governmental, funding, and institutional review board positions—are often unprepared and unable to appropriately evaluate qualitative analyses that feature ethnography, case study, and naturalistic data. (Tracy, 2010, pp. 837–838)

With these pedagogical and political purposes in mind, Tracy (2010) provides eight universal hallmarks for high-quality qualitative methods across paradigms, suggesting that each criterion of quality can be approached via a variety of paths and crafts, the combination of which depends on the specific researcher, context, theoretical affiliation, and project. Her eight "big-tent" criteria for excellent qualitative research are listed in Table 32.7.

We'll examine two of these criterion for clarification: "rich rigor" and "meaningful coherence." The nature of rigor is tricky and difficult for evaluators to define. Rigor in qualitative research differs from that in quantitative research. *Rigor* literally means "stiffness," from the Latin word *rigere*, to be stiff, and it implies rigidity, harshness, strict precision, an unyielding quality, or inflexibility. The term *qualitative rigor*, then, is an oxymoron, considering that qualitative research is "a journey of explanation and discovery that does not lead to stiff boundaries" (Thomas & Magilvy, 2011, p. 151). Thus, the word *rigor* involves many dimensions that must be considered. In qualitative research, rigor often refers to the thorough, ethical conduct of a study of a social phenomenon. We argue that all criteria—rigor and numerous others—used (or considered) in evaluating qualitative research are necessary but may not be sufficient. Tracy's (2010) thesis, therefore, is in line with the tricky nature of rigor, which also reflects what Richardson mentioned earlier, a wish to have a social science art form of EQR:

Like all components in this conceptualization—rich rigor is a *necessary but not sufficient* marker of qualitative quality. For qualitative research to be of high quality, it *must* be rigorous. However, a head full of theories and a case full of data does not automatically result in high quality work. Qualitative methodology is as much art as it is effort, piles of data, and time in the field. And just

Table 32.7 Eight “big-tent” criteria for excellent qualitative research

Criteria for quality	Various means, practices, and methods through which to achieve (end goal)
Worthy topic	The topic of the research <ul style="list-style-type: none"> • Relevant • Timely • Significant • Interesting
Rich rigor	The study uses sufficient, abundant, appropriate, and complex <ul style="list-style-type: none"> • Theoretical constructs • Data and time in the field • Sample(s) • Context(s) • Data collection and analysis processes
Sincerity	The study is characterized by <ul style="list-style-type: none"> • Self-reflexivity about subjective values, biases, and inclinations of the researcher(s) • Transparency about the methods and challenges
Credibility	The research is marked by <ul style="list-style-type: none"> • Thick description, concrete detail, explication of tacit (nontextual) knowledge, and showing rather than telling • Triangulation or crystallization • Multivocality • Member reflections
Resonance	The research influences, affects, or moves particular readers or a variety of audiences through <ul style="list-style-type: none"> • Aesthetic, evocative representation • Naturalistic generalizations • Transferable findings
Significant contribution	The research provides a significant contribution <ul style="list-style-type: none"> • Conceptually/theoretically • Practically • Morally • Methodologically • Heuristically
Ethical	The research considers <ul style="list-style-type: none"> • Procedural ethics (such as human subjects) • Situational and culturally specific ethics • Relational ethics • Exiting ethics (leaving the scene and sharing the research)
Meaningful coherence	The study <ul style="list-style-type: none"> • Achieves what it purports to be about • Uses methods and procedures that fit its stated goals • Meaningfully interconnects literature, research questions/foci, findings, and interpretations with each

like following a recipe does not guarantee perfect presentation, or completing a vigorous training plan does not guarantee race-day success, rigor does not guarantee a brilliant final product. That being said, rigor does increase the odds for high quality, and the methodological craft skills developed through rigorous practice transcend any single research project, providing a base of qualitative fitness that may enrich future projects. (Tracy, 2010, p. 841; emphasis in original)

Tracy (2010) uses metaphors of art and recipes to point out that a claim for rigor involves a *closer investigation*. Its promise and limitations coexist. The politics of “being rigorous” is clearly evident in many types of qualitative research. Likewise, techniques to ensure “rigor,” such as advanced statistical analyses, do not guarantee brilliant quantitative research, either. It is the perception of reviewers or assessors that decides what makes research “good research.” All judgment calls involves a complex mix of relative, contextual, political, and/or ethical criteria. In this regard, “tools, frameworks, and criteria are not value free” (Tracy, 2010, p. 838).

Meaningful coherence, Tracy’s final criterion, is accomplished when “the study achieves what it purports to be about, uses methods and procedures that fit its stated goals, and meaningfully interconnects literature, research questions/foci, findings, and interpretations with each” (p. 839). Thus, this criterion is likely to be seen as a summary of overall judgments in a typical evaluation tool.

Tracey’s “big-tent” set of criteria is a synthesis of other scholars’ constructions of existing goodness criteria. These criteria may usefully remind reviewers about a variety of judgmental aspects in their attempts to determine “how good is good enough,” but it is also important to think about the fact that qualitative research “should not be mechanically scored and summed insofar as some issues may be far more important than others in particular studies” (Stiles, 1999, p. 100). In the end, it is necessary to develop some kind of standardized form of evaluative criteria to be used in qualitative research. Such constructions provide us with meaningful evaluation tools or guidelines, aligned with key criteria, which determines the degree of credibility in qualitative research. Yet, is it really possible to develop standardized forms of evaluation applicable to any type of qualitative research? Tracy (2010) thinks it is:

Perhaps the most controversial part of this conceptualization is the notion of universal criteria

for qualitative quality. However, I believe that we need not be so tied to epistemology or ontology (or the philosophy of the world) that we cannot agree on several common end goals of good qualitative research. Qualitative methodologists range across postpositivist, critical, interpretive, and poststructural communities. In contrast, . . . researcher reflexivity is a validity procedure clearly positioned within the critical paradigm where individuals reflect on the social, cultural, and historical forces that shape their interpretation. . . . I would argue instead that researcher reflexivity—like many other practices for goodness—serves as an important means toward sincerity for research in a number of paradigms. Its utility need not be bound only to critical research. (Tracy, 2010, p. 849)

Nonetheless, we find that some prestigious qualitative journals don’t provide these kinds of criteria or guidelines for their reviewers. Instead, reviewers invited by these journals are provided with very general guidelines. Table 32.8 is an example of the *International Journal of Qualitative Studies in Education* (QES) review form.

As Table 32.8 shows, there are no specific criteria to be used in reviewing manuscripts in this prestigious qualitative research journal. Nonetheless, editorial manager Gonzalez (2012) has confidence in this open process: “Reviewers are free to send any comment to the author. We have very strong scholars to agree to review and most of the times, our reviewers are very detailed (without asking them) in their reviews from grammar, to format, to content. . . many of them go and make comments to each section of the manuscript (intro, methodology, results, conclusions)” (personal communication, August 16, 2012). In this review process, what we find is a sense of autonomy, fit, trust, and professional ethics. Reviewers who have expertise

Table 32.8 A review form used in QSE: *The International Journal of Qualitative Studies in Education*

Recommendation () Accept
() Accept with minor revisions
() Accept with major revisions
() Reject and encourage resubmission
() Reject
Would you be willing to review a revision of this manuscript?
() Yes () No
Comments (Confidential Comments to Editors)
Comments to the Author

know what is worth assessing and how good is good enough.

Yet, there are external forces that question not only quality in qualitative research but also its legitimacy. For example, mixed-methods scholars and researchers try not to see themselves as post-positivists in the research paradigms that have been well established over the past several decades (Guba & Lincoln, 1994; Lincoln & Guba, 2000, 2005; Lincoln, Lynham, & Guba, 2011) but instead seek to create their own hybrid epistemology, one that they prioritize over qualitative research. The current neo-conservative initiatives—the National Research Council (NRC) or the Society for Research on Educational Effectiveness (SREE) (see Denzin, 2009, for more detail)—diminish the tradition of qualitative inquiry that values understanding in human science by narrowly defining what research is and how it should be assessed. Denzin (2009) points to the necessity of casting big-tent criteria to evaluate qualitative research in the context of a changing epistemological and political context:

[W]e must expand the size of our tent, indeed we need a bigger tent! We cannot afford to fight with one another. Mixed-methods scholars have carefully studied the many different branches of the poststructural tree... The same cannot be said for the poststructuralists. Nor can we allow the arguments from the SBR [Scientifically Based Research] community to divide us. We must learn from the paradigm conflicts of the 1980s to not over-reach, to not engage in polemics, to not become too self-satisfied. We need to develop and work with our own concepts of science, knowledge and quality inquiry. We need to remind the resurgent postpositivists that their criterion of good work applies only to work within their paradigm, not ours. (pp. 32–33)

As implied here, current discourse on the politics of evidence is mostly a resurrection of old-fashioned epistemological debates, which are initiated from several organizations or councils at the national level in the United States (e.g., NRC, SREE, the Cochrane Clearinghouse, the Campbell Methods Group, or the What Works Clearinghouse). These trends are generally called scientifically based Research (SBR) or evidence-based movement (EBM). The extended discussion goes beyond the scope of this chapter. The main epistemological questions that need to be asked, just as they were forty years ago, are: “Whose science? Whose scientific principles?” (Denzin,

2009, p. 141). Related to the inquiry of this chapter, we ask, “Whose criteria?”

Tracy’s (2010) eight “big-tent” criteria and the QSE’s simple scholarly decision recommendation with its open-ended comments are two extreme approaches within our qualitative research community. Those situated in the positivist epistemology and mixed-method scholars will likely prefer Tracy’s (2010) “big-tent” criteria for excellent qualitative research over the QSE’s simple form. This is not because Tracy’s reconstruction of other scholars’ constructions is absolutely truthful or valid in itself, but because Tracy approaches it procedurally, in terms of a logical flow of what a reviewer needs to do. The beauty of “big-tent” procedural criteria is that it is normative, to the extent that a reviewer should not disregard the work of an author due to a disagreement with the author’s epistemology. This also applies to the other evaluative extreme, such as the QSE’s recommendation sheet with open-ended comments, in which a reviewer has the freedom to make a scholarly judgment. In our opinion, the current debate on the politics of evidence is too heavily focused on ideology while giving too little attention to ethical concerns.

Conclusion

In demonstrating methodological excellence, we need to take care of ourselves in the process of taking care of others. The most successful researchers are willingly self-critical, viewing their own actions through the eyes of others while also maintaining resilience and energy through acute sensitivity to their own well-being. (Tracy, 2010, p. 849)

Lincoln and Guba’s (1985) constructivist criteria to evaluate our qualitative research processes and products started a rich conversation and decades of scholarship designed to hone and refine those criteria initially proposed and to discover increasingly rich and creative ways to address the challenge of evaluation. Lincoln and Guba argue that trustworthiness is always negotiable, not being a matter of final proof whereby readers are forced to accept an account. Therefore, the field of EQR is not an oxymoron. Much has been known about the nature of evaluative criteria in qualitative research. Some propose a general set of criteria, whereas others focus on specific sets of criteria. This chapter identifies six categories of EQR: (1) a positivist category, (2) Lincoln and Guba’s alternative category, (3) a “subtle-realist” category, (4) a general EQR category, (5) a category of post-criteriology, and (6) a

post-validity category. As seen in many strategies or examples of EQR, some make lists of questions about what is commonly expected in assessing the process and product of qualitative research, whereas others select key validity criteria against which an essence of qualitative research is identified, discussed, and evaluated. Still others adopt broad criteria across these different approaches to construct a comprehensive framework.

As evidence in educational research has continued to be more narrowly defined, many qualitative researchers propose clear counterarguments. Efforts will continue in the search for evaluative criteria from inside the qualitative research community, and the field of EQR will continue to grow, theoretically and practically.

What future directions can we expect for EQR in the twenty-first century? As discussed in this chapter, evaluating qualitative research is complex, challenging, and exciting all at the same time. What matters most is accepting this dilemma, celebrating the reality, and creating a holistic storyline (or a common playful intellectual ground) intended to invite those who have diverse backgrounds to bring different evaluative tools toward constructing flexible but firm evaluation theory, policy, and practice. The qualitative research community may do well to pay close attention to Barone and Eisner's (2012) compromise between common and unique criteria. The beginning of this holistic story has been written, and we hope that others jump in to constructively compete in searching for a common ground in evaluating qualitative research. One of authors of this chapter writes (Cho, 2010):

The shape of a hexagon is naturalistic. Beehives, snowflakes, or molecules are some examples that can be found in nature. We like this hexagon shape just because it seems to represent a balance. A triangle implies a sense of absolute stability or a function of geometric equilibrium. A hexagon shows a sense of balance or harmony particularly when it is connected with others. It looks complicated and messy at a distance but patterned and fabricated when closely seen. Imagine that bees constantly move around the surface of beehive. A beehive is constructed in compactly connected hexagon shapes as bees diligently work with beeswax from their bodies. This analogy can lead qualitative researchers to be more creative in their practical engagement with validity. The shape of a hexagon is unique in that it leads to harmony and

balance as it is tightened from, and connected to each other. (p. 4)

EQR is more than a sum of its parts. It goes beyond creating a set of checklists or recipes. Furthermore, it is more than paradigmatic idiosyncrasy. It should be holistic in nature. Our holistic approach to EQR doesn't seek a complete sense of convergence. Instead, it leaves some room, some unknown territory that may never be reached by the researcher. Like a bee that intuitively and holistically dances around and filters pollen into beeswax to construct a hive, a reviewer deeply imbibes both the process and the product of qualitative research to clearly ensure acceptable quality. Twenty-first century criteria that we support include (1) thought-provoking ideas, (2) innovative methodologies, (3) performative writing, and (4) global ethics and justice-mindedness. Riessman's (2008) reflection on truths and cautions is our ending in a new beginning:

I prefer not to think in terms of standards or criteria, and warn students away from the "paradigm warfare" that exists out there in the literature. It can paralyze and...simplify what are complex validation and ethical issues all investigators face.... Narrative truths are always partial—committed and incomplete. (pp. 185, 186)

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Conclusion: Politics
and the Public



The Politics of Research

Michael D. Giardina and Joshua I. Newman

Abstract

This chapter critically interrogates the politics of research currently dominating US higher education, a politics shaped as much by theoretical and methodological questions and debates as it is by prevailing social, cultural, political, and economic forces. The arguments are guided by four primary questions: (1) How and to what extent do the cultural and political priorities of the free-marketized, corporate university impact, direct, or confound the conduct of research?; (2) how and to what extent does politics situate methodologies?; (3) how and to what extent is the research act impinged upon by such particularities as institutional review boards, national funding councils, scholarly journals, and the promotion and tenure process?; and (4) how and where do academics fit into this new research climate? The authors also forward a series of practical recommendations for professors and students alike who seek to actively confront and challenge the academic-industrial complex.

Key Words: Corporate university, neoliberalism, politics of evidence, politics of research, public intellectuals, researcher subjectivity, scholarly publishing, tenure and promotion

[L]ike it or not, all research is political with deep social and moral dimensions. We must face that apparent contradiction and deal with it head on if we are going to be successful researchers whose ultimate goal is to deepen our knowledge of the natural world and humankind, which will ultimately make our planet a more interesting and hospitable home.

— Stephen R. Forrest, *Vice President for Research, University of Michigan, 2010*

Proem

Prior to joining my¹ current university, I taught in a college of media/communication at a major research university in the Midwest, which was home to many leading scholars doing cutting-edge research in the area of cultural studies and interpretive research, as well as to a world-renowned doctoral program. To a person, my faculty colleagues cared deeply about both research and teaching, about the dissemination of impactful scholarship across the disciplines, and the humanistic education

of young people. There was, to be sure, a culture of research at play that sought to critique, challenge, and change the modern world.

When one of the departments in the college—a department which at the time was ranked as the No. 1 undergraduate and No. 3 graduate program in its discipline in the United States, according to a study undertaken by a competitor university and a fact that was proudly listed on the department's website—searched for and hired a new head several years ago, the hire was, unfortunately, emblematic

of a shift in priorities toward a market-focused rather than research- or education-focused curricular turn. The new head—who had a long career as an advertising practitioner and professor alike and who would later be elected as president of a prominent scholarly association (as well as hold the title of interim dean at one point)—made it clear from the start of her tenure the direction she saw for the department (as well as for higher education at a research-extensive university more generally)².

At a welcoming event for incoming undergraduate students, she couched her worldview—and view for the program—in the context of *market relations* and how, as educators, our job was to listen to industry practitioners so that we could best prepare students for prosperous, financially rewarding careers. She later echoed these sentiments in an interview that appeared in a newsletter for the Association for Education in Journalism and Mass Communication (AEJMC, 2010), where she stated in part:

We must look at the changes in our disciplines, changes in the media and changes in our own academic institutions as opportunities. Some very exciting possibilities are surfacing that will help us in the classroom and in our research. *Yet—we must engage with the profession to ensure that the education we provide is relevant and respected.* AEJMC must be part of this charge and provide the means for the continuous exchange of ideas between the practitioners and the professors so that together we all can make meaningful contributions to educating the next generation. (<http://www.aejmc.org/topics/archives/1328>; emphasis ours)

Part of operationalizing this worldview within the structure of the academic unit included industry-immersion days, in which faculty were required to gather several hours away at a Chicago advertising agency to listen to industry executives and managers talk about what they preferred to see from recent college graduates and what kind of course offerings they thought would be most beneficial³; a symposium held on campus that featured numerous industry practitioners, including one from McDonald's who spoke proudly of the brand's "healthy products" and successful campaigns targeting African-American consumers and another who represented the National Agri-Marketing Association, which has honored marketers from such controversial firms as global agriculture and biotechnology giant Monsanto; and significant efforts at fundraising from external donors.⁴ Additionally, three new faculty members

were hired in the intervening period, all of whom had significant industry experience and whose research generally contributed to and reinforced, rather than was critical of and challenged, the status quo of consumer relations, marketing strategies, and representational politics. And, when it came to research output, it was made abundantly clear that only discipline-specific, positivist-oriented journals—rather than those with an interdisciplinary or critical bent—would "matter most" when it came to tenure and promotion.

The market orientation and pedagogical imperatives could not be any clearer: students should not be treated as free human agents but as future wage-earning automatons (who may one day endow the department with scholarships or provide students with access to jobs in industry); faculty members should provide students with the tools to be successful in industry rather than nurture their ability to critically question the dictates of industry (or everyday life more generally); autocratic managerialism is preferred over shared faculty governance; and higher education as a whole should sublimate itself to market forces. "Missing from this model of leadership," however, argues Henry Giroux (2001), "is the recognition that...public intellectuals are more than merely functionaries of the corporate order" (p. 38). It begs the question: How are we as critical scholars to exist, if indeed flourish, in such a context?

* * *

For nearly a decade, we have been writing about qualitative inquiry (see, e.g., Denzin & Giardina, 2006*b*; 2007; 2008; 2009; 2010; 2011; 2012; Denzin, Lincoln, & Giardina, 2006) and interpretive research methods (see, e.g., Denzin & Giardina, 2006*a*; Giardina & Denzin, 2012; Giardina & Newman, 2011), as well as conducting research in, among, through, and with these paradigms (see, e.g., Giardina, 2005; King-White, Newman, & Giardina, 2013; Newman, 2011; Newman & Giardina, 2011). Put differently, we have been writing for most of our academic careers ensconced within the landscape of a post-9/11/01 sociopolitical context, one that has witnessed an already strong free-market status quo explicitly privileged in all quarters of life (especially those related to education, health care, and national security). As such, we acknowledge that our project is necessarily grounded in and contingent on the accelerated developments that occurred in the United States over the past decade.⁵

This chapter takes as its central task unpacking the politics of research as located within this historical present; a present that is governed and shaped as much by theoretical and methodological interest and engagement as it is by social, cultural, political, and economic forces. That is, a shifting landscape that over the past twenty years has become increasingly hostile not only to qualitative inquiry (see Denzin, 2009) but in fact to the very foundations of higher education and democratic thought that once made the American university, as Edward Said (2004) wrote, “the one public space available to real alternative intellectual practices: no other institution like it on such a scale exists anywhere else in the world” (p. 72). But times, as Bob Dylan used to sing, they are a-changing.

Organizationally, in this chapter, we direct attention to the following questions concerning research practices in the contemporary moment:

- How and to what extent do the cultural and political priorities of the free-marketized, corporate university impact, direct, or confound the conduct of research?
- How and to what extent does politics situate methodologies?
- How and to what extent is the research act impinged on by such particularities as institutional review boards (IRBs), national funding councils like the National Science Foundation (NSF) or National Institutes of Health (NIH), scholarly journals, and the promotion and tenure process?

• How and where do we as academics fit into this new paradigm?

To illustrate the convergence, and indeed, interconnectedness of these myriad forces, we offer the visual representation in Figure 33.1.

As you can see, there are (at least) five nodal points directly impinging on the researcher (which we engage with later). However, there is also a moving, if not amorphous, secondary set of philosophical dynamics that hover over and inform this internal matrix—those issues related to cultural and political politics, globalization, neoliberalism, self-reflexivity and researcher subjectivity, and the intersection of race, class, gender, and sexuality. Which is to say, we must look both internally and externally so as to better understand how the *politics* of research affects our *conduct* of research.

The Politics of Research in the Neoliberal University

In the mid-twentieth century, higher education was a public good; now the distinction between public and private is not so clear. (Tuchman, 2009, p. 21)

Undergirding all research in the contemporary moment is the status and consequence of neoliberalism.⁶ In oversimplified terms, our use of the term “neoliberalism” here is in reference to the political economic movement within most developed nation-states (as well as the nationally transcendental brought about by intensified circuits of global

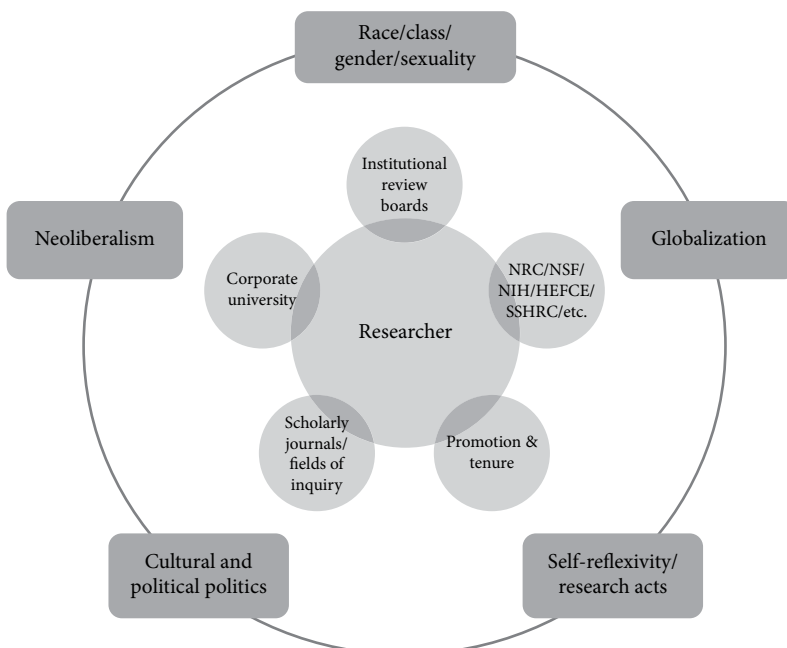


Figure 33.1 Visualizing the politics of research.

interconnectivity) that assumes that only through the freeing of markets and market-based relations can the individual achieve autonomy. According to many globalization and economic theorists, the end of the Cold War (was) brought about this new macroeconomic hegemony—a new world order marshaled by leaders of developed capitalist nation-states (namely Chile under the dictatorship of Augusto Pinochet, and the regimes of Margaret Thatcher in Great Britain, Roger Douglas in New Zealand, Joao Goulart in Brazil, General Suharto in Indonesia, Deng Xiaoping in Communist China, and Ronald Reagan in the United States) who, in the years prior, had supplanted social welfare systems with the *laissez-faire* imperatives of a profit-first, corporate, capitalist free market.

By the time the last few pieces of the Berlin Wall were being carved into souvenir kitsch, a new world order of deregulated, unfettered capital accumulation had emerged—giving rise to an ephemeral boon for a global capitalist class and particularly for those bourgeoisie with investments in the crude oil, biotechnology, finance and banking, digital communication, and mass entertainment sectors (Harvey, 2005; 2007). In most parts of the Global North and Global South, financial markets flourished, speculative capitalism expanded, and income gaps, disparities, and inequalities proliferated at rates that most nodes of the global economy had not theretofore experienced.

Based largely on the “classical” economic theories of Adam Smith and David Ricardo, as revived through neoclassical economists such as Milton Friedman (1962/2002), Ludwig von Mises (1949/2007), and Friedrich von Hayek (1944/2007), these corporate and political intermediaries redesigned a global economy that would “free” various citizenry from the “shackles” of the state, thereby “opening” up the human condition to uninterrupted forms of market exchange. These initiatives included abolition of state regulation of economic activity; opening of national markets for international trade (e.g., the North American Free Trade Agreement [NAFTA]); eradication of tax codes (and particularly those that taxed corporations of upper-class earners); promotion of the interpenetration of capitalist relations into every nuanced social relation; a refocus on individualism and a turn away from social welfare; suppression of central planning; a re-emphasis on individual freedom and a denial of social and economic stratification; and the introduction of new, rationalized systems of “accountability” thusly imposed upon institutions and actors within the public sector.

Under this new world order, social relations were publically rearticulated as *capital* relations, in which “corporations work closely with the neoliberal state to construct the new economy. The neoliberal state focuses not on social welfare for the citizenry as a whole but on enabling individuals as economic actors. To that end, neoliberal states move resources away from social welfare functions toward production functions” (Slaughter & Rhoades, 2004, p. 20). Thus, the public good, unlike at any time in history, became a site of private investment. Public monies were divested from the social welfare apparatuses and reinvested into the market: “The neoliberal attitude toward the state and the market, coupled with the crisis of capital accumulation in the 1970s and 1980s, has led to declines in state spending in, amongst other parts of the former welfare state, higher education” (Canaan & Shumar, 2008, p. 10).

Under this new paradigm, “corporate America” emerged as the dominant teleology of the now global late-century economy. It was, as Giroux (see, e.g., 2004) has repeatedly made clear, a materially consequential confluence of unfettered profitability for the private sector and anti-government and anti-Keynesian governmentalities—a realigned democracy in the service of global commerce. Sheila Slaughter (2006) vividly captures the recourses of the neoliberal turn in this way:

At the core, one might argue that neoliberalism is fundamentally anti-cultural or seeks to deny cultural positionality—reducing human action and interaction to patterns of economic activity. It is a set of political, economic, and social dictums conceived, and installed in the real world by, individuals who have exclusively experienced “freedom” through economic relations; wealthy individuals whose paradigm is located in a sensibility of economic “self-worth.” Particularly noteworthy are the ways in which the neoliberal state: alters the boundaries between public and private sector; shifts public subsidy from welfare functions to entrepreneurial activity; exhibits a preference for commercial solutions to public problems; empowers managers rather than workers; privileges the individual over collectivities when collectivities pursue activities that would constrain capital; and favors secrecy and various schemes of classification of information over public circulation of knowledge and civil liberties. All of these have far reaching consequences for academic freedom as we know it. (p. 2)

This philosophy-turned-policy, as we now know, had considerable effects for schooling and

tertiary education in particular.⁷ As Giroux (1992) reminds us, there were “no disciplines, pedagogies, institutional structures, or forms of scholarship that [we]re untainted by the messy relations of worldly values and interests” (p. 89). Today, the university, like most public institutions, has been radically transformed into a vessel for promulgating heightened commercial activity and extracting new forms of surplus value. What we have witnessed in North and (many parts of) South America, Asia, Europe, and the Pacific Rim is the subordination of “non-market-orientated practices” to create new forms of consumption and labor value. Slaughter and Rhoades (2004) argue that “the neoliberal state,” of what Karl Polanyi (1944/2001) famously predicted as the coming of the “market society,” has “promoted privatization, commercialization, deregulation, and reregulation.... Colleges and universities that pursue an academic capitalist knowledge/learning regime have benefitted from these processes” (p. 21).

What institutional practices have brought about these “benefits”? To a much greater extent than in years past, in these educational settings: *knowledge is treated like a commodity* generated for the purposes of capital accumulation; students are marketed to and socially engaged as “consumers”; pedagogues are managed to be more productive, and less expensive, laborers (whose labor is increasingly deskilled, rationalized, and made more efficient); and the institutional spaces themselves have become more commercialized and spectacularized (e.g., a North American college athletic complex or a Starbucks coffee shop housed inside a campus library). One result, as Simon Critchley recently lamented about the British context (but a lament that applies equally to our argument) is that:

Universities used to be communities; they used to be places where intellectual life really happened. They were also places where avant-garde stuff was happening. And that’s—in England anyway—completely ground to a halt. Universities are largely sold as factories for production of increasingly uninteresting, depressed people wandering around complaining. There’s been a middle-management take-over of our education, and it’s depressing. So universities... have become a kind of pedestrian, provincial university run by bureaucrats. (quoted in Hines, 2012, para. 3).

These, we argue, are the living axioms of our time: the market-centric bases to which our academic work is now bound. In the remainder of this

section, we focus on how this context is filtered throughout three primary dimensions as it pertains to the research act: the production of knowledge, the consumption of knowledge, and the politicization of knowledge.

Producing Knowledge

Many observers have argued that we have in recent years witnessed the rise of the “corporate university” (Giroux, 2007; Johnson, 2008; Rutherford, 2005; Shumar, 1997). In other words, we have seen an increased “marketization and commodification of higher education as indicative of the wider transition of previously public sector institutions as we move from the welfare state to the market state” (Canaan & Shumar, 2008, p. 4). The university campus is now dominated by corporate rhetoric and directives guided by “networking,” “workforce needs,” “cost effectiveness,” “return-on-investment,” and “economies of scale.” Much like a corporation, the university now hires consultants to help control, or “streamline,” variable costs such as faculty wages and “production-related” expenses. As a result, universities have concentrated or even abandoned traditional pedagogical spaces and techniques (i.e., person-to-person classroom teaching) in favor of low-cost options such as online courses, massive lecture auditoriums, and outsourced “practicum” experiences (i.e., internships, which are generally unpaid).

The products that those intellectual workers produce—in this case specialized knowledge—have become increasingly replicable and thus transferrable (Shumar, 1997).⁸ The university as research output center has undergone a quasi-Taylorist revival, whereby the human actions created within its spaces are subjected to an intensified “technocratic rationality” (Althusser, 1971). Academic staff are disciplined by an intensifying regime of assessment of their teaching and research products, what Yvonna S. Lincoln (2011) refers to as “a neoliberal, managerial, technocratic set of means for regulating and normalizing behavior and for inducing conformity within the profession” (p. 370). “Rather than a democratic discourse where all of an institution’s citizens are involved in developing dispositional knowledge,” posit William G. Tierney and Robert A. Rhoads (1995), such assessment protocols have “tried to create a sharper division between managers and workers and to reinforce norms rather than bring them into question” (pp. 109–110). The intended consequence then, as Said (1996) might argue, is “intellectual professionalism” within the professoriate—whereby

actors within an orchestration of scholarly life are encouraged not to rock the proverbial boat, not to stray outside the “accepted paradigms or limits,” and to explicitly work to make oneself marketable, and, above all else, make oneself “uncontroversial and unpolitical and ‘objective’” (p. 55).

Such technocratic rationality “operates under very specific patterns aimed at the increase of production and prediction, the control of the economy, and the regulation of society” (Fernandez-Balboa & Muros, 2006, p. 199). George Ritzer (1998) describes the consequences of these rationalizing processes on what has later been termed “McUniversity” in this manner: “Many students and faculty members are put off by its factory-like atmosphere. They may feel like automatons processed by the bureaucracy and computers or feel like cattle run through a meat processing plant” (Ritzer, 1993, p. 143). Ritzer’s (1993) point is that, much like a franchise link in the McDonald’s restaurant chain, the production processes of the university have come to be defined by the precepts of efficiency, calculability, predictability, and control.

In particular, workers within the contemporary university have incurred increased pressure to publish (or perish) in greater numbers and to subordinate their “teacher selves” to a more operational, if not subsistence, aspect of their working lives. Moreover, in most “research-intensive” and “research-extensive” institutions, it is not a matter of *if* one produces research “outputs,” but what *type* of research. Institutions now often reserve “seed grants,” teaching and research assistantships, or reduction in teaching loads for those teacher-researchers who can promise long-term “yields” in the form of externally lucrative research projects, the development of online “distance-learning” courses, or the founding of research centers driven by private dollars. Which is to say, as producers of knowledge, the working lives of today’s professoriate are ever more influenced by a growing technocratic hegemony from within the university, increasingly rationalized and quantified research expectations, and, as a consequence, the devaluation of their teaching endeavors. Cast as knowledge producers in and of the free market, more scholars in the social sciences have thus turned their focus toward replicable, so-called scientific forms of “evidence” —the sort of evidence market forces and their corporate surrogates are seeking to further expand commercial enterprises (the public–private partnerships at many research universities, with their designated “research parks” and entrepreneurial foci being one such example).

However, we follow Giroux (1983) in calling into question the positivistic, reifying standards by which this neoliberal science is conducted:

the outcome of positivist rationality and its technocratic view of science represents a threat to the notion of subjectivity and critical thinking. The question of essence—the difference between the world as it is and as it could be—is reduced to the merely methodological task of collecting and classifying facts. In this schema, “knowledge relates solely to what is, and to its recurrence” (Horkheimer, 1972). Questions regarding the genesis, development, and normative nature of the conceptual systems that select, organize, and define the facts appear to be outside the concern of positivist rationality. (p. 15)

At its core, what Giroux is elucidating is that if scientific inquiry is molded around market forces, then the generative potentialities of new knowledge formations in and around a topic of inquiry is thus limited to their totemic epistemologies and methods. Or, put differently, “when the market interests totally dominate colleges and universities, their role as public agencies significantly diminishes—as does their capacity to provide venues for the testing of new ideas and the agendas for public action” (Zemsky, 2003, p. B9).

Consuming Knowledge

Just as the “production” of knowledge within the university has succumbed to neoliberalism’s “market-first” imperatives, so too have the practices by which that knowledge is now “consumed.” In his methodical vivisection of the “consumer cultures” of the corporate university, Frank Furedi (2002) makes the following case about higher education in the present tense: “universities exist not simply to educate, but also to sell education in a competitive market to customers who, in the past, were wrongly thought of as just scholars” (pp. 34–35). These days, academic staff members are often required to attend “customer relations” seminars, faculty are encouraged to participate in “annual giving campaigns” (and thus in a post-Fordist twist, consume that which they produce), campus bookshops rarely deal in the seminal texts (but more often in themed merchandise), and the spaces of postmodern consumption now cloak campus greenspaces.

In much the same way Channel One⁹ successfully galvanized an economic partnership with public secondary schools to sell their wares to education’s most captive “audience” (see Graham-Pardus, 2003,

para. 9), universities sell their students (or their profiles) to corporations in the fast food, credit card, and telemarketing industries (to name but a few) (Twitchell, 2004). Perhaps the most disconcerting aspects of corporate university consumerism, however, is that a new generation of college students has arrived at the university well-trained in the practices of education-based consumption: “students now in higher education have gone through a commodified, marketized and stratified education system prior to their entry to higher education, which has inculcated this consumerist position and an instrumentalist approach to learning more generally” (Canaan & Shumar, 2008, p. 7). Writing in the *Chronicle of Higher Education*, Rob Jenkins (2012) provides one glimpse of this consumerist ethos in action, drawn from a comment made by a student in his introductory rhetoric class:

We were talking about the way that social mores and public opinions change over time, and how writing both influences and reflects those changes. But when I broached one particularly controversial topic, a student interjected, “*But that’s just your opinion, and I’m not paying for your opinion.*”... [T]he current emphasis on “customer service” in academe seems to have given some students the impression that they have the right to “purchase” only those ideas that they personally agree with, and that all other ideas or opinions are at best irrelevant and at worst akin to faulty products or unsatisfactory service. (paras. 2, 11, emphasis ours).

It is this commodification of college life—*of the very processes of learning*—that both constitutes and is constitutive of the broader foundations of neoliberal life. The core principle underscores a sense of self-identity that can only be constructed through market-based consumerism: “to be a citizen is to be a consumer, and nothing more. Freedom means freedom to purchase” (Croissant, 2001, p. 1). If, as we have followed others in arguing, the university has become a productive and produced space through which these sensibilities are protracted, celebrated, and often rewarded, then it does so at a frightening social cost.¹⁰ For instance, what are the costs associated with refining our scientific techniques, assumptions, or results to best appeal to consumer markets? Are some constituents willing to pay more for “university validated” knowledge than others? Most consequentially, what lines of discovery, critique, and invention are we bypassing if we only seek to generate a neoliberal knowledge agenda?

We see the consequences of a “customer-first,” market-friendly science as significant—what Giroux (2007) in fact refers to as the marketization of science. We likewise agree with Canaan and Shumar (2008) that “this consumerist position tends to discourage critical thinking and foreclose a more genuine opportunity to have a say in the shaping of knowledge generation” (p. 7). Within such a context, the urgency for reliability, replication, and “evidence” of bodily patterns has created a condition of tautology—whereby the research act constitutes building on, “proving,” or “disproving” prior work, answering the call for “more research in the area,” and so on.

Under such a *modus operandi*, we risk falling victim to the academic-industrial complex, of becoming “merchants of McKnowledge” (Finklestein, 2002). The focus of research design falls not on the epistemological soundness of the project or even the possible benefits for society it might yield, but rather on the researcher’s ability to expand on existing findings toward some tangible end. That end has most often been discursively situated within the confines of “applied research.” Reading for the best of “applied research,” we find a transcontinental body of committed scientists seeking to make a difference by applying their skills, knowledges, and training to real-world problems. A more cynical, or perhaps pragmatic, reading of the emergent realm of “applied science” might suggest a turn toward the re-sourcing of university assets (equipment, human labor, information technology, etc.) to generate new marketable commodity knowledge—knowledge that may or may not solve real-world problems but will most certainly have value in the marketplace based on tautological revelation. In either case, posits Joanne Finkelstein (2002), such inquiry is now overrepresented by “a closed system where meaning becomes a cliché and the epistemological horizon is reduced to unobtrusive banalities” (p. 183). To escape from this tautology, she argues, “meaning must be discoverable in the juxtapositions of improbabilities... As we encounter differences and improbables, we are directed toward fresh ways of thinking and understanding; new ideas and forms emerge” (p. 183).

As just one example, the political calculus of this context has given rise to the emergence of the “self-funded researcher”—the proverbial “Million Dollar Club” member—who while nominally aligned with a university in a professorial role often buys out his or her teaching duties and answers to the dictates of funding agencies rather than their

university constituents. Put into practice, neoliberalism has brought about the age of intellectual excavation—whereby legions of prospectors scour the realms of natural resources, public service, and medicine in hopes of identifying and capitalizing on those natural, cultural, or humanistic fields previously un[der]-commodified.¹¹ These are researchers—well-intentioned health advocates, engineers, or entrepreneurial careerists—who scour the public domain for research “opportunities” that could generate new forms of external research funding, personal accolade, or institutional legitimacy.¹²

Politicizing Knowledge

Quite obviously, the formulations just described are not occurring in a historical vacuum. In point of fact, there is very clearly a political imperative at play as well, one that favors (and in many ways explicitly endorses) these conditions of emergence. In the past decade-plus, sociopolitical actors within the United States have increasingly demonized higher education and the professoriate.¹³ This we have seen manifested in the extreme policy assaults on unions and public workers, which burst forth with some widespread success following the 2010 midterm elections in the United States that resulted in the election of right-wing politicians such as Scott Walker (R-WI), Rick Scott (R-FL), and John Kasich (R-OH), among others. Most notable of this trio was Walker, who introduced a bill to “kill collective bargaining rights for public sector workers” in Wisconsin, one that was met with widespread outrage among state employees and generated a firestorm of debate across the nation (see Kroll, 2011, para. 2). Importantly for us, as *The Nation’s* Chris Hayes (2011) reported, those impacted most directly by Walker’s bill would be teachers (and, by extension, the imperatives of civic education):

Teachers unions are the stewards of preserving public education, which is the core element of our civic life, of the collective democratic enterprise that is these United States. Conservatives have wanted to abolish public education in its current form for a while, and getting rid of the teachers unions is a necessary first step. (MSNBC Live News Broadcast, February 18, 2011)

In their neoliberal vision of education, critical discourse and the free exchange of ideas stands as impediments to the dictates of the market. Or, as Jason Del Gandio (2010) explains:

While most colleges are still nonprofit institutions, their primary function is to serve the neoliberal

enterprise. This happens in at least three ways—by targeting student-consumers, channeling students into corporate careers and contributing to rather than reducing social stratifications. (para. 8)

Such dictates were effectively endorsed by the National Governor’s Association, whose Center for Best Practices suggested that “colleges need to do a better job of aligning their programs with the economic needs of their states” by moving “beyond their traditional emphasis on a broad liberal-arts education to thinking more about skills for specific jobs” through the use of “‘rigorous labor-market data’ to set goals and get more input from local businesses on the skills students need” (Kelderman, 2011, para. 1).¹⁴

In statehouses across the country, legislators have taken this line of thinking to heart. One of the most prominent and indeed problematic “solutions” offered thus far is the “Seven Breakthrough Solutions” reform plan for universities that was developed by the Texas Public Policy Foundation, a free market-based think tank and advocated for by Rick Perry, the Republican Governor of Texas. The plan has drawn harsh rebukes from the academic community for its favoring of teaching over research, measuring productivity and value to the university through a cost-benefit analysis of instructors, and “focusing on the needs of the customers, or students, rather than the faculty” (Ludwig, 2011, para. 3).¹⁵ The deleterious effects of such a plan would be staggering. As John O’Connor (2011) summarizes, the plan marks the first step toward research universities becoming nothing more than diploma mills, encourages heavier teaching loads at the expense of research, and implicitly encourages grade inflation.

It should come as no surprise to us, then, that the 2012 political platform issued by the Republican Party of Texas “blatantly opposed critical thinking in public schools throughout the state” (Weil, 2012, para. 1). The document states in part:

“Knowledge-Based Education—We oppose the teaching of Higher Order Thinking Skills (HOTS) (values clarification), critical thinking skills and similar programs that are simply a relabeling of Outcome-Based Education (OBE) (mastery learning) which focus on behavior modification and have the purpose of challenging the student’s fixed beliefs and undermining parental authority” (quoted in Strauss, 2012, para. 2).¹⁶

Yet this is not an isolated incident. In our own state (Florida), Governor Rick Scott has not only

made public overtures toward the Texas plan just mentioned (see Balona, 2011), he has also made public declarations that should alarm those working in higher education. In October 2011, Scott made public waves by attacking liberal arts degrees such as anthropology as not being of a “vital interest to the state,” seeking instead to shift funding to degrees such as those in science, technology, engineering, and math (the so-called STEM disciplines); or, as Scott put it, funding should “go to degrees where people can get jobs in this state” (Scott, quoted in Anderson, 2011, para. 4–6). Additionally, Scott advocated for “weeding out unproductive professors” and rethinking (if not outright abolishing) faculty tenure (Anderson, 2011, para. 3).¹⁷

However, this market-political rationality cannot be separated from a more cultural-political purview. We recently (2011) saw the state of Arizona embroiled in debates concerning the teaching of Latino Studies in public K–12 schools, where the state’s attorney general went so far as to refer to such programs as “propagandizing and brainwashing” (see Lacey, 2011, para. 3) as he declared the program of the Tucson Unified School District illegal for violating a new state law.¹⁸ Arizona, of course, has been Ground Zero for debates concerning immigration (see, e.g., SB1070). It is thus no surprise that there was enough popular support to dismantle the program but also that its chief opponents have taken aim on the state university system. In a shocking although not surprising statement, Arizona’s superintendent of schools (and also a member of the Arizona Board of Regents), John Hoppenthal, inferred that the university system was to blame for the promotion of diversity at the high school level: “I think that’s where this toxic thing starts from, the universities” (quoted in Planas, 2012, para. 3). In response, Devon Peña, a former chair of the National Association for Chicana and Chicano Studies, pointed out the political reality facing such programs at the university level, noting: “While [Hoppenthal] will not find a sympathetic ear in the faculty ranks, he is on the Board of Regents, and he can certainly hurt the university when it comes to the budget” (quoted in Basu, 2012, para. 12).

In a similar vein, we have seen lesbian, gay, bisexual, and transgender (LGBT) programs or minors targeted with the same politicized rhetoric. In Louisiana, for example, US Representative Jeff Landry (R-New Iberia), embroiled in a tight reelection campaign, called on the University of Louisiana at Lafayette to drop its LGBT minor because, in his view, it fails to “provide an academic benefit to

students or a worthwhile financial return to taxpayers” (Addo, 2012, para. 4). His statement read in part: “I want our young people to be prepared for the workforce and the LGBT minor does not assist them toward their goal. Our neighbors and students should trust that the education dollars they spent at University of Louisiana at Lafayette will be used to further their careers, not a political agenda” (quoted in Alpert, 2012, para. 2).¹⁹

In many ways, this politicized view of higher education—or, at least, one that politicizes the cultural politics embedded within higher education in the service of some other political end such as free-market dogma or vice versa—is in line with that enumerated by the Heartland Institute, a 501(c)(3) think tank whose mission is to “discover, develop, and promote free-market solutions to social and economic problems” (www.heartland.org); specifically, its “Ten Principles of Higher Education Reform” plan put for by Richard Vedder and Matthew Denhart (2011), which takes as a given that “U.S. institutions of higher education are less efficient and decreasingly effective at creating the foundation for [economic] success” (p. 2). Vedder and Denhart identify an array of “problems” to which their solutions are directed, including the rising cost of obtaining a four-year degree, falling productivity among faculty, falling teaching loads among faculty, “obscure” research by faculty, and so on (p. 3). Yet any critical reading of these “problems” would underscore that they are divorced from historical context and circumstance in the best light and outright nonsense in the worst light.

To the contrary, costs are rising because federal and state support of higher education has steadily diminished over the past three decades, causing universities to recoup lost funding by raising tuition and fees; productivity is not easily defined, nor is it constant from discipline to discipline or field to field (i.e., is the geneticist with a \$10 million NSF grant more or less productive than the cultural anthropologist doing fieldwork on disease prevention in Africa? Is it strictly a bibliometric calculation?); teaching loads vary greatly from institution to institution based on a number of factors, including research and service obligations and the mission of the university/college/department (say, research-intensive university to small liberal arts college); and the notion of obscurity of journal publishing, while open to debate, especially among those who would argue that academics should be more engaged with public scholarship, in and of itself is hardly the purview of politicians.

The “solutions” put forth by the Heartland Institute are therefore not surprising; in fact, they would effectively codify the public university as private enterprise. To wit, the solutions include ending government subsidies to higher education (essentially, privatizing higher education); explicitly viewing students as customers to be competed for, so much so that subsidy be given directly to them instead of to the university in the form of vouchers and monetary bonuses for performance; promotion of lower cost alternatives (which, although it goes unstated, likely means online education, including such for-profit centers as the University of Phoenix); emphasizing undergraduate instruction over research and training of graduate students (which is actually referred to in the document as “frivolous activities” [p. 14]); subjecting research to “cost–benefit scrutiny”; eliminating shared faculty governance in favor of top-down managerialism, including as it relates to curricular developments; and, providing the “necessary incentives for faculty and administration to concentrate on making students’ financial investment pay off” (such as merit bonuses for teaching higher numbers of students or allowing “private firms” to teach classes).

At a deeper level, however, the issue is not simply one of market solutions rather than government solutions to a particular problem; it is about the very nature of the public university and its role in a democratic society. Which is to say, these “problems” (and the associated “solutions” on offer) feed into and further perpetuate the negative—if not dangerous—view of higher education proffered by an cadre of politicians, cable news pundits, industry leaders, and, increasingly, the general public: *that its mission is to serve the market and that it is in decline*. Consider that a recent Pew Social Trends poll (“Is college worth it?,” 2011) found that 47 percent of the public reported the main purpose of a college education “is to teach work-related skills and knowledge” whereas just 39 percent said it is to “help a person grow personally and intellectually (the remaining 24 percent viewed both purposes equally). Viewed through such a prism, it is of no surprise that such market-oriented “solutions” are viewed as legitimate by both policy makers and voters alike, as opposed to a controversial course of action.

Thus, it is not a surprise to us that we continue to have a growing chorus of voices on the side of the market rather than on the side of actual democratic education. That is, it is now seen as politically palatable for someone like Governor Pat McCrory

(R-NC) to make public declarations in support of legislation that would allocate funds “not based on how many butts [are] in seats but how many of those butts can get jobs” (quoted in Shin, 2013, para. 6)—and who then followed that up by denigrating gender studies and its place in public education by stating “If you want to take gender studies that’s fine, go to a private school and take it, but I don’t want to subsidize it if that’s not going to get someone a job” (quoted in Shin, 2013, para. 6). The problem, of course, as Bruce Janz (2012) points out in his timely criticism of the Heartland proposal referenced earlier, is that “Knowledge isn’t always about what the market thinks it is.”

To varying degrees, university administrators have come forward to speak out against the austere plans of Perry, Scott, McCrory, and others, as when Arizona State University President Michael M. Crow (2011) wrote in response to Governor Scott’s call for reductions in state appropriations for specific disciplines (i.e., the humanities) so that public universities in his state (Florida) could focus resources on STEM fields:

The notion that we must strip away academic programs not seemingly relevant to workforce development reflects a simplistic and retrograde view of the role of higher education in the American economy.... Curricula expressly tailored in response to the demands of the workforce must be balanced with opportunities for students to develop their capacity for critical thinking, analytical reasoning, creativity, and leadership—all of which we learn from the full spectrum of disciplines associated with a liberal arts education. (para. 2–4)²⁰

Which is all well and good when university administrators are actually on the same page as the professoriate.²¹ Yet this is not always the case, as too many of us are all too well aware. And, more problematically, for too long, too many of us have stood by while these changes were happening before our very eyes; as H. L. Goodall, Jr. (2013) reminds us, “academics haven’t effectively organized as citizens for over thirty years.” He continues,

We are, most of us, afraid to speak out, afraid to act up, and we are afraid to take a contrary stand or do anything that might threaten our jobs, our departments, our budgets and hiring plans, our tenuous hold on an increasingly diminished future. Political action of any kind by faculty on campuses is effectively banned—who among us has not received that annual email “reminder” from a

provost? Yet who among us who see that message as internally and logically inconsistent—after all to forbid the political is itself a political statement that supports only the continuation of the *status quo*—yet who, who among us, has dared to challenge it? And would such a challenge even matter? Would it rally our colleagues? I doubt it. The defeated do not challenge much of anything. The defeated accept what they fear they cannot change. They lack a voice. They become quietly cynical. They protect themselves.

Rather than protecting ourselves through insularity and acquiescence, how might we go about actually protecting ourselves, our students, and our fields of inquiry from such an onslaught? The sections that follow attempt to address such a question.

Confronting the Politics of Research

Educators need to defend what they do as political, support the university as a place to think, and create programs that nurture a culture of questioning. But there is even more at stake here. It needs to be recognized on a broad scale that the very way in which knowledge is selected, pedagogies are defined, social relations are organized, and futures are imagined is always political, though these processes do not have to be politicized in a vulgar or authoritarian way. (Henry A. Giroux, 2009)

It is within and against the earlier described context of the free-marketization of higher education and the deeply politicized terrain in which it is situated that we in the United States reside as researchers. In Figure 33.1 we represented the complex matrix of the politics of research. The previous section was spent addressing neoliberalism and the corporate university. In this section, we turn our attention to specific engagement with and the ramifications of this dynamic. Specifically, we discuss the impact it has had (and continues to have) on the context of research undertaken in its throes. And by that we mean how it has impacted the conduct of our research, the problematics associated with publishing it, and the extent to which it is subsumed within debates concerning promotion and tenure. To be sure, this is a far-reaching, nebulous discussion, one that calls for sustained in-depth inquiry. As such, and limited by page restrictions in this chapter, we submit that we are only scratching the surface of this important debate.

But as we scratch that surface, our starting point should be on the question of evidence. We would argue, in fact, that the politics of research

is inseparable from the politics of evidence—from the political economy of evidence—in the historical present. On this point, Janice M. Morse (2006) is quite clear: the term itself “is an oxymoron. Evidence is something that is concrete and indisputable, whereas politics refers to activities concerned with the acquisition or exercise of authority” (p. 79). Or, as Glenn Lerner (2004) argues, the “political economy of evidence... is not a question of evidence *or* no evidence, but who controls the definition of evidence and which kind is acceptable to whom” (p. 20). This is a view shared by Stuart Murray, Dave Holmes, Amélie Perron, and Geneviève Rail (2007), who posit that “‘truth’ and ‘evidence’ are always overdetermined by the social, historical, and political contexts that lend them their currency and power” (p. 515).

Allow us to briefly review the events of the past decade-plus.²² The methodological conservatism embedded primarily in the educational initiatives of the George W. Bush administration (2000–2008) inscribed (and, to a great extent, codified) narrowly defined governmental regimes of truth. The result was a new “gold standard” for producing knowledge that was “worthwhile having,” one that was based on and privileged quantitative, experimental design studies (Lincoln & Cannella, 2004, p. 7). Specifically, the scientifically based research movement (SBR), first introduced by the US federal government in the Reading Excellence Act of 1999 and later incorporated by the National Research Council (NRC) in its 2001 “Scientific Research in Education” report, created a new and, in fact, hostile political environment for qualitative research in the United States (see Denzin & Giardina, 2006*b*; Denzin, Lincoln, & Giardina, 2006). Embodying what Joe Maxwell (2004) has termed a “re-emergent scientism” and a positivist, so-called evidence-based epistemology, the NRC report moved to define scientific inquiry as being *the same* in all fields, embodying the same general set of principles while acknowledging that different disciplines may have unique features. Under this directive, researchers were (and still are) “encouraged” to employ “rigorous, systematic, and objective methodology to obtain reliable and valid knowledge” (Ryan & Hood, 2006, p. 58). The preferred methodology has well-defined causal models using independent and dependent variables. Causal models are examined in the context of randomized controlled experiments that allow replication and generalization (Ryan & Hood, 2006).

But this “methodological fundamentalism” (Lincoln & Cannella, 2004, p. 7) returns to and

instantiates a much-discredited model of empirical inquiry. That is, the experimental model is ill-suited to

Examining the complex and dynamic contexts of public education in its many forms, sites, and variations, especially considering... subtle social difference produced by gender, race, ethnicity, linguistic status or class. Indeed, multiple kinds of knowledge, produced by multiple epistemologies and methodologies, are not only worth having but also demanded if policy, legislation, and practice are to be sensitive to social needs. (Lincoln & Cannella, 2004, p. 7).

Elizabeth St. Pierre (2004) further reminds us that this “privileging of randomized experimental trials seems to occur in a time warp because educational researchers have acknowledged for decades that there is no single method that can serve as the gold standard for quality science” (p. 133).

Under this framework, qualitative research becomes suspect (if not dismissed outright). “Evidence” from quantitative research is prioritized whereas qualitative, non-numerical, and/or critical theory and constructivist inquiry is marginalized because there are no well-defined variables or causal models. And, as such, the epistemologies of indigenous, critical race, queer, postcolonial, feminist, and postmodern theories are rendered useless, relegated at best to the category of scholarship or commentary, not science (National Research Council, 2001; Ryan & Hood, 2006; St. Pierre, 2004).

There is a great deal at stake in and over these arguments. The demands of this narrow view of evidence raise questions that require serious public discussion, for they celebrate a historical moment when the methods of positivistic science are not being challenged. In valorizing the experimental model, they ignore the many criticisms of experimentalism, developed over four decades ago, involving the inability to adequately treat rival causal factors associated with internal and external validity, as well as the limitations of naïve realism; the erasure of the value-fact-theory distinction; the death of the disinterested observer who has a god’s-eye view of objective reality; the reliance on an ethics of deceptions; and a refusal to consider the contexts of knowledge production or the researcher–subject relationship (Campbell & Stanley, 1963; Howe, 2004; Lincoln & Guba, 2000).

But is this a question of research?
Or a question of politics?
Essentially, both.

Michael L. Silk, Anthony Bush, and David L. Andrews (2010) remind us of the real-world consequences of these internecine struggles, pointing out how

[t]he actions of public and private funding bodies... have made it apparent that the nearer one approaches the *gold standard* of randomized experimental design, the more one is likely to receive funding for doing “objective and *good science*,” and the larger that funding is likely to be. (p. 107; emphases in original)

Moreover, and although it may seem like common sense to say it, the political flavor of the month also influences the topical swing of funding. Writing in *Scientific American*, Eugenie Samuel Reich (2011) points out, for example, that in the years directly following the terrorist attacks of September 11, 2001, the research direction toward national security concerns “had a profound effect on U.S. research in areas as diverse as forensics, biodefense, infectious diseases, public health, cyber security, geology and infrastructure, energy, and nuclear weapons. Even the social sciences have been effected by the emergence of ‘terrorism studies’ and the new emphasis on the threat in the field of risk analysis” (para. 3). Left unsaid is that this swing resulted in a noticeable uptick in short-term thinking and funding for research on issues related to national security (possibly to the detriment of long-term research, such as that on cybersecurity). A 2007 report authored by the Committee on a New Government-University Partnership for Science and Security for the National Academies of Science, titled “Science and Security in a Post-911 World,” noted, in fact, that in the preceding five-year period, there had been “a remarkable increase of funding for bioterrorism-related research, while long-standing research budgets in the life sciences [were] cut or [had] remained stagnant” (p. 89).²³

A similar case can be understood vis-à-vis funding for human embryonic stem cell research during the Bush presidency; specifically, the policy restrictions on federal funding that were adhered to from August 9, 2001 to March 9, 2009 (when it was rescinded by the Obama administration). In brief, the Bush policy barred the National Institutes of Health from funding research on embryonic stem cells beyond the sixty cell lines that were already in existence at the time the policy went into effect. Yet this was not a question of scientific debate or inquiry about the soundness of the medical procedures and so forth, but one of moral disagreement

combined with short-term political posturing. “As ideology,” writes Brandon Keim (2009), “Bush’s restrictions on embryonic stem cell funding were legitimate. They represented a moral objection to the destruction of embryos by people who believe that life begins when sperm meets egg” (n.p.). But it was clearly an incoherent if not self-serving moral objection on Bush’s part, one made visible when revealing the political politics at work. As Michael Kinsey (2006) pointed out in the midst of the debate:

George W. Bush claims to believe [that a microscopic embryo is a human being with the same human rights as you and me], and you have to believe something like that to justify your opposition to stem-cell research. But Bush cannot possibly believe that embryos are full human beings, or he would surely oppose modern fertility procedures that create and destroy many embryos for each baby they bring into the world. Bush does not oppose modern fertility treatments. He even praised them in his anti-stem-cell speech. It’s not a complicated point. If stem-cell research is morally questionable, the procedures used in fertility clinics are worse. You cannot logically outlaw the one and praise the other. And surely logical coherence is a measure of moral sincerity. (paras. 9–10)

Moreover, in President Obama’s (2009) statement lifting the federal ban on stem cell funding, he went out of his way to make clear that politics should not enter the equation:

Promoting science isn’t just about providing resources—it’s also about protecting free and open inquiry. It’s about letting scientists like those who are here today do their jobs, free from manipulation or coercion, and listening to what they tell us, even when it’s inconvenient—especially when it’s inconvenient. It is about ensuring that scientific data is never distorted or concealed to serve a political agenda—and that we make scientific decisions based on facts, not ideology. (“Obama ends stem cell research ban,” 2009, para. 24)

But here is the rub: *we know it still does.*

* * *

If the politics of evidence and the politicized arena in which inquiry is conducted weigh heavily on the minds of researchers during the design and execution of a research project, then the twinned dynamics of publishing that research and subsequently having that research “count” in the tenure

and promotion process weigh heavily during the “writing up” phase. Which is to say, although the pressure to “publish or perish” has always consumed us, now the pressure is increasingly to “publish in specific journals according to specific dictates and misunderstandings of the field or perish under the throes of such regimes.”

Flip through the pages of *The Chronicle of Higher Education*, scan the editorial pages in of some of the larger newspapers in the country, or browse the contents of scholarly journals, and you will likely find numerous debates on the current state of academic publishing, including those about open-access publishing in a digital world and the political economy of the academic publishing industry (see, e.g., Howard, 2012; Koh, 2012; Labi, 2012; Weedon, 2013). In a critical overview of this landscape, Ted Striphas (2010) reminds us that, “The scholarly journal publishing industry of today barely resembles what it was a generation ago. It is both larger—in terms of the sheer number of journals now being produced—and smaller—in terms of the total number of publishers now producing those journals.”²⁴ One negative consequence of this “overproduction” of knowledge—that is, of the oversaturated yet increasingly nichified realm of academic journals—is that “the chances of any given academic journal article getting noticed by one’s colleagues—let alone outside of academe—diminish,” resulting in ever-increasing numbers of unread or uncited studies “whose formal designation as ‘published’ obscures their actual existence as interred” (Striphas, 2010). As a result, we see greater efforts deployed to differentiate between and among journals, to rank them in importance, and so forth. At a time when the importance placed on bibliometric data, such as journal “impact factors,” is at an all-time high, we should be mindful of the grounds on which these debates are and have been taking place.

In his 1997 *British Medical Journal* essay titled “Why the Impact Factor of Journals Should Not Be Used for Evaluating Research,” Per Ottar Seglen outlined a myriad of problems with the use of impact factors, including that “journal impact factors are determined by technicalities unrelated to the scientific qualities of their articles”; “journal impact factors depend on the research field”; “coverage of the database is not complete”; “database has an English language bias”; and “small research fields tend to lack journals with high impact” (Seglen, 1997, pp. 498–499). Even Eugene Garfield (2000), the Chairman Emeritus of Thomson ISI

who devised the tool, explained in his review of the history and meaning of the journal impact factor, “It is one thing to use impact factors to compare journals and quite another to use them to compare authors” (para. 3). Yet even this is a specious argument because comparing journals across categories and fields of inquiry—especially for those conducting inter- and transdisciplinary work—can prove to be rather futile.²⁵

Although we readily admit that the conversation (if not controversy) over journal metrics and impact factors is an important one, and one that we heartily encourage, it is presently outside the scope of this chapter. Rather, in this section, we are more concerned with the de facto policing of knowledge in the pages of these journals and the chilling effects such policing has for qualitative researchers today. If pressed, we think most would agree—even those who may have had poor experiences²⁶—that publishing in any given journal is a generally fair and rewarding if lengthy process, facilitated by and with editors and reviewers who genuinely care about the field and its direction. Within the broader domain of qualitative inquiry, there are certainly ongoing debates to be had over paradigms and methodological engagement and/or disagreement,²⁷ but we have found most of these to be serious discourses aimed at moving the field(s) forward. However, there are always exceptions to the rule—exceptions that highlight if not complicate the sometimes precarious nature of being qualitative researchers, one that is directly tied to the politics of evidence discussed earlier.

Consider the questions raised by Marcelo Diversi and Claudio Moreira (2011). They ask us to consider carefully the terms of the debate and who defines those terms: “Who can speak for whom? Under what power relations? Which bodies continue to determine what constitutes legitimate scholarship?” (p. 230). The answers shouldn’t surprise us, because the song remains the same. As Deborah Ceglowski, Chiara Bacigalupa, and Emery Peck (2011) posit in their article “Aced Out: Censorship of Qualitative Research in the Age of ‘Scientifically Based Research,’” most researchers are—from their earliest days of graduate scholars—“socialized into understanding, accepting, and perpetuating the master narrative” of gold standard scholarship—one that “narrowly specifies and controls acceptable kinds of research, as defined by a limited number of researchers—mostly White and male” (p. 680). It is thus that the institutional structures of the university and the academy increasingly lead

“to the creation of a conventionalized system that judges which inquiries warrant publication or adoption, and ultimately which constructs of truth and knowledge are given public voice to influence educational practice itself” (Randall, Cooper, & Hite, 1999, p. 10). Or, as Silk, Bush, and Andrews (2010) put it, the current context of such evidence-based research is one in which “the training that most doctoral students receive, and in particular the orientation provided in most research design courses, results in the vast majority of students gaining an implicit and explicit understanding of, and comfort with, *foundational* (see Smith & Hodkinson, 2005; Amis & Silk, 2008) beliefs of how to ‘do’ *rigorous research*” (p. 112; emphases in original; for more see also Denzin & Giardina, 2013).

It is within this context that Ceglowski et al. (2011) point to their own experience publishing in the journal *Early Childhood Research Quarterly* (ECRQ), which is considered a top-tier journal in the field of early childhood education,²⁸ and which, according to its own description, “publishes predominantly empirical research (quantitative or qualitative methods).” A cursory engagement with the journal’s contents, however, and especially following passage of the Reading Excellence Act of 1999, shows that qualitative work has almost no home in the journal: Ceglowski et al. note that in the three years immediately following the Reading Excellence Act of 1999, only 4 of 72 manuscript published in the journal were qualitative, a 75 percent decline from the three-year period immediately preceding the Reading Excellence Act. More troubling, however, is the story Ceglowski et al. (2011) share about their experience of having a manuscript reviewed for publication with *ECRQ*.²⁹

To wit, the two external reviews of their original manuscript submission (reviews that the authors share) display no understanding of qualitative paradigms and ask questions concerning reliability, validity, generalizability, measurement, coding, and so on, including a statement questioning why a study should be conducted in the first place if it cannot be generalized. When questioned as to the reviewer selection, the editor essentially acknowledged a better reviewer selection was needed³⁰; the revised and resubmitted manuscript was subsequently rejected as well, with one reviewer even questioning why numbers and percentages weren’t used! The authors then elected to submit the same manuscript to another journal, one that was “purported to be more ‘friendly’ to qualitative research” (p. 683)—it was accepted with positive reviews

and a glowing letter of endorsement from the editor, who noted in part: “The implications of this research are very interesting and take use into a new world of the child” (p. 684).

One manuscript; two different journals; two strikingly different interpretations. And although we only have space to highlight this particular case, it is representative of a larger model of gatekeeping at work, one that skates dangerously close to being more concerned with political negotiation than scholarly inquiry. Take the case of two recent editorials in the highly ranked journal *Qualitative Health Research* (QHR).³¹ In a rather curious editorial, Morse, Coulehan, Thorne, Bottorff, Cheek, and Kuzel (2009) took a stand against “the transformation of data into poetry or free verse” (p. 1035), which, they argued, was becoming ever more common among submissions to the journal.³² They stated in part: “The editors of QHR have considered this trend carefully, and we have made the decision to resist accepting manuscripts of this genre for publication, for the reasons presented below” (p. 1035). The reasons behind their thinking included length of manuscript, significance of contribution, what such an approach does to change the “data,” and that the “presentation of data as verse focuses on the literary device rather than the health research” (p. 1035). Importantly, Morse et al. went out of their way to reject poetry and free verse as being decidedly not “science,” writing instead that “Rendering data into free verse ought potentially to be an alternative approach to dissemination of findings; in other words, not published in scientific journals but brought to people’s attention in other ways at alternate venues, such as poetry reading in one’s local coffee house” (p. 1036). And, to compound the matter, the very next issue of QHR contained a guest editorial by its associate editor, Sally Thorne (2009), which demonized the single-case narrative (p. 1184) and questioned “the credibility and validity of narrative inquiry as a qualitative health research technique” (p. 1185) while privileging a (post-)positivist oriented qualitative paradigm (p. 1184).³³

There are numerous problems with such framing, not the least of which is that it raises questions over the nature of “data” and “science,” privileging (post-)positivism over and against the interpretive realm.³⁴ As James Carey (1989) might say, such a positioning presupposes (and reproduces) an “ever-present desire to maintain a distinction between hard science and soft scholarship” (p. 99). Foisting poetry and other such forms of narrative

inquiry off the pages of scholarly journals and into the “local coffee house” effectively seeks to relitigate the paradigm wars (see Guba, 1990) while obfuscating, if not ignoring, the past twenty years or so of debate and development in this area.

We have been down this road before

Laurel Richardson’s (1993) “Louisa May” example serves us well. “Louisa May’s Story of Her Life” is a narrative poem the author created from an in-depth interview with her title character, “Louisa May,” who was an unwed mother. That is, Richardson transcribed the interview into thirty-six pages of prose text and then “shaped it into a poem/transcript” (Richardson, 1994, p. 140). In so doing, she wrestled with “postmodern issues regarding the nature of ‘data,’ the interview as an interactional event, the representation of lives, and the distribution of sociological knowledge” (p. 140). As she explains:

Louisa May is the speaker in the poem, but I crafted it, using both scientific and poetic criteria. I used only her words, repetitions, phrases, hill-southern rhythms, and narrative strategies, such as multi-syllabic words, embedded dialogues, and conversational asides. My intent was for the poem to stand aesthetically and emotionally...but I also wanted it to be faithful to my sociological understanding of Louisa May’s story of her life... Writing “data” as a poem did two things: first, it changed me, personally, unexpectedly (Richardson, 1992); and second, it exposed the truth—constituting, legitimating, and deeply hidden validating function of the genre, prose. (1993, p. 696)

This form we would recognize as rather established in 2013, what Denzin (2001) has called the “reflexive interview,” one that “is simultaneously a site for conversation, a discursive method, and a communicative format that produces knowledge” (p. 27). And we can now easily find numerous examples of and discussions about this and other postmodern approaches to interviews in the pages of such journals as *Qualitative Inquiry*, *International Review for Qualitative Research*, *Cultural Studies*, *Critical Methodologies*, and *Qualitative Research*.

Yet, when Richardson presented “Louisa May” at the annual Society for the Study of Symbolic Interaction meetings in 1990, it—along with the “skipped line”—was met with hostility from some of those in attendance. Most especially, she was questioned on the “reliability and credibility of the original experience” and the “accuracy” of the story (see Richardson, 1993, p. 699). But beyond

the particularity of Louisa May and her experience as an unwed mother, Richardson's poem was asking questions about that which is taken for granted, about the "obsession of the real," of "grand narratives," and the wider sociological engagement with everyday practices, including the use of interviewing (pp. 700–702). As she reminds us,

Interviewing is a standard sociological technique for acquiring knowledge, for "knowing." Interviews are co-created through the intersection of two subjectivities, the interviewee and the interviewer. In the poem, Louisa May reminds us from the opening line ("The most important thing") to the closing line ("I've talked so much my throat hurts") that she is constructing her life in an interactional context. What we claim to know as sociologists is displayed as constructed knowledge. What happens, then to our authority? our definitive readings?... The poem confronts and threatens sociological epistemology and ontology [and] displays how sociological authority is constructed, and problematizes reliability, validity, and truth. Poetics strips those methodological bogeymen of their power to control and constrain. (1993, p. 704)

And Morse et al. (2009) and Thorne (2009) would appear to want none of that within the pages of their journal!

The real-world implications of such academic politics are clear. In his recent essay on "gated intellectuals" and the role they play in establishing boundaries to protect the status quo and isolate citizens from one another, Giroux (2012, para. 7) reminds us how

higher education is increasingly being walled off from the discourse of public values and the ideals of a substantive democracy at a time when it is most imperative to defend the institution against an onslaught of forces that are as anti-intellectual as they are anti-democratic in nature... it is one of the last strongholds of democratic action and reasoning and one of the most visible targets along with the welfare state. (para. 7)

Although Giroux is specifically referring to gated intellectuals as being public figures like Thomas Friedman or David Brooks who, through their standing and visibility in papers such as the *New York Times* legitimate the harshest realities of neoliberalism, we think it is fair to apply that term to academics as well, for, although their gatekeeping may not reach the level of public influence as those mentioned they nonetheless impact the nature of

research and scholarship on a fairly broad level.³⁵ This is of supreme importance to the case of qualitative researchers, especially if we accept Denzin and Giardina's (2010) assertion about our scholarship and our role as researchers in the present moment: it is not *just* about "method" or "technique." Rather, it is "*about making the world visible in ways that implement the goals of social justice and radical, progressive democracy*" (p. 14, emphasis in original).

Yet the cards are stacked against such a proposition, especially when we consider it in light of the politics of promotion and tenure at the corporate university. If the politics of publishing represents the tip of the iceberg, then the politics of promotion and tenure is the large mass hiding just below the surface. As with our broader discussion about the corporate university, politics is littered throughout our contemporary understanding of tenure. Although the tenure process itself is, more or less, still rather functional to the extent that it is aimed at aiding rather than inhibiting faculty advancement along a particular, if preordained, path,³⁶ the idea of tenure itself is largely under attack, both by external political forces and the internal shift to a neoliberal model of governance.

Consider the following public polling data: In a recent *TIME* magazine poll on the state of public education in the United States ("Americans' views of teacher tenure, merit pay, and other educational reforms," 2010), fully two-thirds (66 percent) of respondents to the question "Do you support or oppose tenure for teachers, the practice of guaranteeing teachers lifetime job security after they have worked for a certain amount of time" in the negative (with 28 percent in favor and 6 percent holding no opinion).³⁷ Likewise, a recent Pew Research Center poll (see Stripling, 2011) found that only 24 percent of the more than 1,000 college presidents surveyed said that, if given a choice, "they would prefer that most faculty at their institution be tenured" whereas roughly 7 in 10 "preferred that faculty be employed on annual or long-term contracts." Additionally, we have seen multiple arguments against tenure appear with increasing regularity within the pages of mainstream media (especially those couched within a market-based rationale). This would include such pieces as Christopher's Beam's (2010) widely discussed *Slate* article "Finishing School: The Case for Getting Rid of Tenure," which presents a cost-benefit argument against tenure; Vedder's (2010) *New York Times* op-ed that repeats well-worn conservative ideological arguments that tenure reduces intellectual diversity via the marginalization of nonliberal faculty; and renowned *Freakonomics*

author (and tenured professor of economics at the University of Chicago) Steve Levitt's (2007) blanket call for tenure to be abolished at all levels.³⁸

Put differently, there isn't a lot of political capital to be gained defending an issue that doesn't register on the broader population (or one that they are actively against); in fact, just the opposite: across the country, states such as Ohio and Florida have moved toward eliminating tenure altogether at the K–12 level. In Florida, for example, Senate Bill 736, otherwise known by its more Orwellian name, the "Race to the Top for Student Success Act," stipulated that all new K–12 teachers hired once the Act went into effect would only be hired on one-year renewable contracts.³⁹ Such action will likely have a deleterious impact on K–12 education, including the recruitment of new teachers into the public school system and the snowball effect on students graduating into college and university programs.

But Florida has its eyes on a bigger prize: eliminating faculty tenure at the university level.⁴⁰ Giroux (2009) refers to this trend within the corporate university as the "casualization of academic labor," noting that as universities increasingly adopt models of corporate governance (discussed earlier), they are aggressively

eliminating tenure positions, increasing part-time and full-time positions without the guarantee of tenure, and attacking faculty unions... At a time when higher education is becoming increasingly vocationalized, the ranks of tenure-track faculty are being drastically depleted in the United States, furthering the loss of faculty as stakeholders.

Currently, only 27% of faculty is either on a tenure track or in a full-time tenure position. (para. 4)

Clearly, this is an untenable situation for the professoriate moving forward, especially in terms of infringement on academic freedom (see Nelson, 2010).

But let us also consider the practical politics of achieving tenure in the first place. Patricia Leavy (2012) quite rightly points out that "the existing tenure and promotion system continues to enforce disciplinary" (para. 4). She continues:

Academics have clear incentives to design small-scale projects that can be completed and published quickly. Moreover, sole authorship is favored over co-authorship and collaboration. Further, peer-reviewed articles and/or monographs are required for tenure and promotion at most, if not all, institutions. By requiring research that produces such limited outcomes, researchers' hands are tied. It is

also clear that journal articles are highly unlikely to reach the public so by privileging this form the entire academic structure discourages scholarship that is truly of value to the public. (para. 4)

Although we might not go so far as to endorse Leavy's argument in toto, we agree that the context her argument contests is one that clearly promotes the professionalization of the professoriate—that promotes positivist social sciences as currently practiced and taught in US higher education. It is a context that the radical historian Howard Zinn (1997) cogently outlined in his essay "The Uses of Scholarship," in which he noted the five rules that "sustain the wasting of knowledge" (pp. 502–507):

1. Carry on "disinterested scholarship."

2. Be objective.

3. Stick to your discipline.

4. To be "scientific" requires neutrality.

5. Scholars must, in order to be "rational," avoid "emotionalism."

Put differently, what Zinn is talking about is "intellectual professionalism" of the kind challenged by Said (1996), who defined it as:

Thinking of your work as an intellectual as something you do for a living, between the hours of nine and five with one eye on the clock, and another cocked at what is considered to be proper, professional behavior—not rocking the boat, not straying outside the accepted paradigms or limits, making yourself marketable and above all presentable, hence uncontroversial and unpolitical and "objective." (p. 55)⁴¹

Taking this line of thought to its natural end, Silk, Bush, and Andrews (2010) contend that such "[p]roper professional behavior—and in our present moment we have to equate *proper* with that which holds the centre, the *gold standard*, EBR [Evidence-Based Research]—represents a threat to our *critical sense*, our ability to be prepared to be self-reflexive to relations of power" (p. 120; emphasizes in original).⁴² *And it is a truly devastating critique of and challenge to our responsibilities as intellectuals.*⁴³

Conclusion

The global community of qualitative researchers is mid-way between two extremes, searching for a new middle, moving in several different directions at the same time. (Denzin, 2009, p. 19)

So here we sit. Our universities grow ever more corporate, ever more market-oriented. Public policy

and public opinion swings ever more against us. Questions of evidence remain in flux. Funding dollars are earmarked for or withheld from certain forms of inquiry for short-term political gain. All of which necessarily trickles down into and through our scholarly outlets (i.e., journals) and the privileging thereof by promotion and tenure committees (as well as merit committees, award committees, and so forth). Taken collectively, some might say that it would be the height of foolishness to be a qualitative researcher in the current moment. We were even told recently, at a grant writing event we attended at our own university, that despite our successes as qualitative researchers,⁴⁴ we should only really consider applying for grants if we were able to have a mixed-methods or quantitative component to them because that's "what counts these days." Not because it might be the best methodological decision in terms of research design, mind you, but because that's what "counts"!

Well excuse us if our politics don't match your methodological fundamentalism.

For it is our politics and the politics of our research endeavors that situate our methods of inquiry—not the other way around. And whereas for some this may be an easy sell, we acknowledge the shaky ground on which many scholars—especially graduate students and newly-minted PhD's—reside. In response to these conditions, Cheek (2007), for one, suggests that we consider how to "work in these spaces rather than being worked over by them" (p. 102). By this, she means that we must go beyond "acknowledging that these spaces exist"—that is, the corporate university, politics, marginalization, and the like—to a critical awareness that we are subjected to them and that they need to be negotiated.

In the spirit of Cheek's (2007) invocation, we offer in closing the following practical recommendations:

1. *We must acknowledge that we are not innocent actors in academia.* We have a moral and sacred responsibility to our communities to change them for the better. How often do we agree to or volunteer to serve on grant adjudicating committees? How often do we agree to volunteer to serve on our university IRB committees? Or promotion and tenure committees? Or any other college-level committee charged with dispersing funding, granting entrance to doctoral students, and so forth? How often have we chosen to run for elected office in our scholarly associations? Or serve on editorial boards or as editors of the journals in

our field(s)? Or join our university faculty unions? How often are our research projects contributing to social change or, failing that, some translatable goal? If you're answering "No" to all of these questions, what are you still waiting for?

2. *We should take every opportunity to broadly communicate our research beyond just the academic journal.* This, of course, does not mean abandoning it altogether or that it is even a realistic option for some, depending on the politics of one's department of college, but it is something that should be strived for and, importantly, *rewarded*.⁴⁵ Such a decision, of course, speaks to the larger topic of our role as researchers in the first place. As Giroux (2012, para. 31) implores us to never forget: "The very notion of being an engaged public intellectual is neither foreign to nor a violation of what it means to be an academic scholar, but central to its very definition."

3. *We must mentor our doctoral students to be cognizant of the politics of research and the context of research into which they are stepping.* It is becoming increasingly frustrating to us when we encounter doctoral students (at conferences, as job candidates, etc.) who seem to be blissfully unaware of the politics of their own profession and/or field and how they situate themselves and have come to be situated in those politics, and who are quite comfortable perpetuating the status quo. If the next generation of researchers is woefully unprepared to face the challenges that lay ahead—or *doesn't want to face them*—then we worry about its future. On this point, we defer to the late Bud Goodall, who asked us to get practical when he remarked: "How well do we train generations of writers in the practicalities of being a writer? About getting a literary agent? Writing literary inquiry? Putting together a blog? Putting together a website? These are things that should be part and parcel of the enterprise that we call academic preparation for the future. Because unless we give our students those tools, unless we cultivate that, it's like throwing someone into a very competitive, highly competitive market without any skill other than that they can write and they want to have a voice, and in this day and age that's just not quite enough. So what do we do? We nurture the young" (Ellis, Bochner, Denzin, Goodall, Pelias, & Richardson, 2008, pp. 330–331).

4. *We must engage with our undergraduate students and degree programs lest they fall victim to the dictates of the corporate university.*⁴⁶ That is, we have an obligation to push back against the

rising demands of the free-market-first approach to higher education that currently holds sway (at least in the United States) over many degree programs. Not only must we continue to advocate for the inclusion of courses that develop students' critical faculties as free human agents, we must simultaneously reject the kind of "banking" education of "receive, memorize, and repeat" that Paulo Freire outlined in *Pedagogy of the Oppressed*—one that sees students as passive objects rather than as active learners. Additionally, and as more and more universities endeavor to bolster their doctoral and master's degree programs—placing ever more importance on graduate credit hours and graduate teaching—we must hold firm that such moves do not come at the expense of undergraduate education (in the form of more doctoral students or visiting [and often underpaid] nontenure-earning lecturers teaching ever-increasing course sizes).

5. *We must engage with and continue to build a community of qualitative researchers.* The work being done by the International Congress of Qualitative Inquiry (<http://www.icqi.org>) is one example of this, and it has nearly tripled in size in the roughly ten years of its existence.⁴⁷ The Congress sponsors dozens of workshops, hundreds of parallel sessions, spotlight sessions, and specialty days (e.g., "A Day in Spanish," "A Day in Qualitative Health Research," and so on) during its annual meetings, and has established a network of collaborating sites throughout the world, an official journal (*International Review of Qualitative Research*), and an annual book series. The critical issues and discussions that have come out of this Congress have, in our view, been at the forefront of contesting the methodological fundamentalism of the last decade. Another example would be the International Institute for Qualitative Methodology at the University of Alberta, Canada (<http://www.iiqm.ualberta.ca/>), which has been pioneering efforts in this area since its founding in 1998. For let us not forget that we don't have all the answers—that the search for the next question, rather than the next answer, is a guiding light of critical scholarship (see, e.g., St. Pierre, 2011), and one that is worth seeking out—and defending.

* * *

It would have been nice to end this chapter with some profound words of wisdom. An uplifting riff about the future of qualitative inquiry, perhaps. A call to arms that seems achievable, within our

grasp. But, to be honest, and for all of the advances we've seen in the past decade, there's still a lot of work to do. Looking forward, we hope we have touched on a number of issues that can be brought into your doctoral seminars, faculty meetings, college committee meetings, and so forth. The stakes are too high to sit passively by while others shape our fields of inquiry. We have a job to do; let's get to it.

Notes

1. Giardina is speaking here.
2. As a curious side note, the candidate to whom the position was initially offered was an advertising executive with no previous experience in higher education save for holding a master's degree. This individual was offered the position despite serious questions raised by numerous research faculty in the College. In the end, the candidate declined the offer and chose to stay in industry.
3. On this point, the consensus was on hands-on or practical courses, such as account planning or quantitative market research skills, as well as proficiency in web design, social media, and Adobe products such as Photoshop and Illustrator.
4. On this last point, Samantha King (2012) rightly points out that "college administrators are now more often hired for their role as fund-raisers and their ability to bridge the world of academe and business than for their intellectual capacities and experience in public service" (p. 77).
5. We further acknowledge that we will speak primarily to the US context. For those interested in the politics of research as germane to the United Kingdom, Australia, and elsewhere, please see the works of Harry Torrance, Julianne Cheek, and Uwe Flick.
6. Portions of this section are reprinted with slight variation from King-White, Newman, and Giardina (2013), as well as drawing on arguments related to neoliberalism in Newman and Giardina (2011). For more on the status and nature of the corporate university see, among others, Eric Gould's (2003) *The University in a Corporate Culture*; Gaye Tuchman's (2009) *Wannabe U: Inside the Corporate University*; Derek Bok's (2003) *Universities in the Marketplace: The Commercialization of Higher Education*; Steven C. Ward's (2012) *Neoliberalism and the Global Restructuring of Knowledge and Education*; and Henry Giroux's (2007) *The University in Chains: Confronting the Military-Industrial-Academic Complex* and (2011) *Education and the Crisis of Public Values: Challenging the Assault on Teachers, Students, and Public Education*.
7. For exhaustive analyses, see Bowles & Gintis, 1976; Kincheloe, 2004; Slaughter & Rhoades, 2004.
8. Shumar (1997) postulates a "new stratification of the workforce" whereby "Part-time faculty face being institutionally invisible and the lack of job security and benefits. Full-time faculty have been stratified into researchers and teachers, and teaching is becoming increasingly stigmatized" (p. 14).
9. Channel One includes a twelve-minute news program for teens that is broadcast via satellite to nearly 10,000 middle schools and high schools in the United States, where it reaches 6 million students. It has been criticized for exposing children to captive advertising, which they are effectively forced to watch as they consume the "news" program.

10. Critics have suggested that the “university is an impersonal market-driven setting; mostly concerned with its own survival, progress and prestige” (Fernandez-Balboa, 2009, p. 148).
11. David Harvey (2005, 2007) often refers to this process as “accumulation by dispossession”; a process by no means unique to education (think oil fields in the Middle East, copper mines in South America, or the privatization of primary and secondary education in post-Katrina New Orleans) in which stagnating regimes of capital accumulation pursue, and commandeer, those materials, services, and relations that were once property of the public (or part of the public good).
12. Within the field of public health, for example, the road most travelled (at least in neoliberal times) seems to lead to the same place: the “obese” body and the plethora of funding opportunities available to those who seek to find a “cure” for the obesity “epidemic.” See King-White, Newman, & Giardina (2013) for more on this topic.
13. Whereas the positivist-oriented recommendations of the 2002 NRC report mentioned later in this chapter (along with such ancillary developments as the US Department of Education’s “What Works Clearinghouse”) came to the fore during the early days of the George W. Bush administration, we would concede that, in the least, it was done with noble if flawed intentions on the part of rigorous academics and policy makers alike (as we had also seen with the bipartisan No Child Left Behind Act of 2001, which passed the Senate by a comfortable 91–8 vote and House by a similar margin of 384–45).
14. This growing institutional acceptance of the corporate university has not gone unnoticed. From movements such as Occupy Cal in California, to the tireless efforts of unions such as the Graduate Employees’ Organization (GEO) at the University of Illinois, to the recent Southern Illinois University–Carbondale faculty strike over collective bargaining agreement talks (not to mention major student protests throughout England during late 2010 against draconian budget cuts in the educational system [see Davis et al., 2010]), students and faculty alike have increasingly stood up to show that the status quo is untenable and must be changed.
15. The seven “solutions” put forward are described in the following terms: “Measure teaching efficiency and effectiveness; publicly recognize and reward extraordinary teachers; split research and teaching budgets to encourage excellence in both; require evidence of teaching skill for tenure; use “results-based” contracts with students to measure quality; put state funding directly in the hands of students; and create results-based accrediting alternatives” (<http://texashighered.com/7-solutions>)
16. Although its communication director, Chris Elam, later indicated that such language “made it into the platform by mistake” (quoted in Lach, 2012, para. 4), he offered the following clarification: “I think the intent is that the Republican Party is opposed to the values clarification method that serves the purpose of challenging students beliefs and undermine parental authority” (ibid, para. 8). The mind reels.
17. During Scott’s time in office, public employee retirement/pension contributions have also been slashed (Dunkelberger, 2012).
18. At the same time, similar programs for Black, Asian, and Native American studies were left untouched.
19. We have also seen the inverse, in which politically laced funding has had the intent of influencing the hiring of faculty with particular ideologies. One recent example is the alleged “strings attached” donations by the Charles G. Koch Charitable Foundation to the Department of Economics at Florida State University for the hiring of two professors who share the Foundation’s ideological standpoint on free-market economics (see Hundley, 2011).
20. At the same time, Naomi Klein (2011) points us to the further economic modalities of the attack on public sector employees: the issue at hand is not a matter of unions versus taxpayers, as Governors Walker, Scott, Kasich, and their acolytes would have it; rather, it serves as a proxy “fight about who is going to pay for the[economic] crisis created by the wealthiest elite in this country... Is it going to be regular working people? Or is it going to be the people who created this crisis?” (MSNBC Live Broadcast hosted by Chris Hayes).
21. Even University of North Carolina President Tom Ross, in brandishing a defense against McCrory’s plan in stating “the University’s value to North Carolina should not be measured by jobs alone” nonetheless couched a defense of his university in the language of the market:

Our three-part mission of teaching, research, and public service requires that we prepare students with the talents and abilities to succeed in the workforce, because talent will be the key to economic growth. (para.)
22. The following four paragraphs are slightly revised and updated from Denzin & Giardina, 2006b.
23. The same report points out that funding for bioterrorism and biodefense-related research funding by the National Institutes of Health increased from \$51 million in fiscal year 2001 to more than \$1.9 billion requested for fiscal year 2007.
24. Striphas (2010) reports that commercial presses (i.e., Taylor & Francis, Elsevier, etc.) account for roughly two-thirds of all of the 20,000 or so scholarly journals on the market today.
25. Consider that the *Sociology of Sport Journal* (SSJ), with which we are both affiliated as associate editor and editorial board member, has a 2011 impact factor of 0.917 and is ranked 17/36 in the “Hospitality, Leisure, Sport, Tourism” category, and 54/137 in the “Sociology” category, with 532 total citations per Thomson Reuters reporting data and period. Compare that to *Qualitative Inquiry*, for example, an interdisciplinary journal in which we have both published. Its 2011 impact factor is 0.839, and is ranked 36/89 in the category “Social Science, Interdisciplinary” with 1,508 citation. Yet numbers can be deceiving. The category in which SSJ is located is a rather arbitrary mix of journals that in fact have very little in common. Although ranked 17 out of 36, it is ranked second among the actual sport-oriented journals listed (i.e., *Journal of Sport & Social Issues*, *International Review for the Sociology of Sport*, *Journal of Sport Management*, etc.). Likewise, *Qualitative Inquiry*, which “provides an interdisciplinary forum for qualitative methodology,” is ranked in a category along with journals that are completely unrelated, such as the *Archives of Sexual Behavior*, *Accident Analysis and Prevention*, and *Public Opinion Quarterly* (all of which have higher impact factors than *Qualitative Inquiry*). Should this be the measure of comparison? It is only when deconstructing the actual category that the astute reader would find that among true “peer” journals in category, only *Journal of Mixed*

Methods Research and *Qualitative Research* are “ranked” higher. The matter becomes even more complicated when considering other journal “ranking” tools, such as the one available through the SCImago Journal & Country Rank portal, which, although similar, does not reproduce the same rankings exactly as does Thomson Reuters.

26. Who among us hasn't had a manuscript sit under review for months and months or received an occasional external review letter from someone who either misunderstood the arguments at hand or put perhaps too little effort into the review? This we accept as par for the current course, however, unfortunate.
27. See, e.g., the debates between Denzin and Hammersley over the role of social justice inquiry in qualitative inquiry.
28. The journal's 2011 impact factor was 1.671, and it was ranked 19/203 in the “Education & Educational Research Category” and 33/67 in the “Developmental Psychology” category.
29. It is important to note that Ceglowski and Bacigalupa are both tenured professors who have made significant contributions to the field of childhood education and policy.
30. We are not insensitive to the demands placed on editors when it comes to getting folks to agree to do reviews. Speaking (MDG) from experience as an associate editor of one journal and the special issue editor of another who is tasked with doing just that, acquiring reviews can be a time-consuming struggle. However, it is inexcusable for an editor to invite reviewers who have no conceptual understanding of the work to be reviewed. In fact, as Ceglowski et al. remind us, it goes against the American Educational Research Association guidelines on reviewer ethics, which state: “Judgments of the adequacy of an inquiry should be made by reviewers who are competent to read the work submitted to them. Editors should strive to select reviewers who are familiar with the research paradigm and who are not so unsympathetic as to preclude disinterested judgment of the merit of the inquiry” (AERA, 2005, p. 8).
31. *QHR* is a “peer-reviewed monthly journal that provides an international, interdisciplinary forum to enhance health care and further the development and understanding of qualitative research in health-care settings.” Its 2011 impact factor was a lofty 2.188, and it was ranked 13/62 in the “Health Policy & Services” category, as well as being the highest-ranked qualitatively oriented journal in the category.
32. We use the word “curious” here given both Morse and Cheek's longstanding service and dedication to qualitative inquiry, including as friends of the International Congress of Qualitative Inquiry.
33. Although we adamantly disagree with Thorne's viewpoint, she is certainly not alone; clearly, there are many in the field who would agree with her position on poetry and narrative. And, in point of fact, her recent book, *Interpretive Description* (2008), is a useful text for better understanding qualitative research within applied settings and disciplines. What we object to is the patronizing tone cast over all such inquiry by her editorial because it has real implications for qualitative scholars rather than as simply philosophical conjecture in the conversation about inquiry.
34. See also the special issue of *Cultural Studies* => *Critical Methodologies* edited by Mirka Koro-Ljungberg and Maggie MacLure (2013) on the theme of “rethinking data.”
35. Cases in point, the *Journal of Advertising* and the *Journal of Sport Management*: it is fair to say that both of these journals are the premier or flagship journals in their respective titular fields of inquiry. The contents of both are largely overrepresented by quantitative research, primarily because those two fields of inquiry are themselves largely overrepresented by quantitative research (e.g., as related to consumer behavior or consumer identification)—a chicken-and-the-egg scenario, perhaps. Additionally, their respective editorial boards mirror this overrepresentation. It is of no surprise, then, that qualitative research that manages to find its way into either of these journals is, by and large, couched with a post-positive or at best quasi-foundational perspective.
36. At our institution, for example, faculty up for both promotion to associate professor with tenure and promotion to full professor are provided a clearly defined, step-by-step explanation of the process; afforded the opportunity to attend workshops; have access to supporting materials to aid in constructing the tenure binder; and move through what is, by and large, a rather transparent, supportive process. We acknowledge that this is not necessarily the case at every university, nor is the outcome of the tenure process always positive in our institution.
37. The poll itself is riddled with inconsistencies between responses, which, taken as a whole, reveals more about the (limited) public understanding of education than on any one individual response. That is, a majority of respondents essentially believed that (a) teachers are underpaid (61 percent), but were (b) against teachers unions (50 percent, with 35 percent in favor and the rest undecided), and were (c) in favor of standardized testing (64 percent), but (d) believed that it is possible “to make changes that would dramatically improve student performance” (90 percent), while generally (e) not willing to pay higher taxes to improve higher education (42 percent against).
38. Faculty tenure is a long-running topic of discussion on the *Freakonomics* blog. Levitt's (2007) article ignited a firestorm on his own blog, as well as in such outlets as the *Chronicle of Higher Education*. For a vociferous defense of tenure, see Nelson (2010).
39. Prior to the Act, Florida K–12 teachers were hired on one-year contracts for the first three years before being awarded “professional service contracts” that were effectively a form of tenure.
40. Not only that, but we now have some individual institutions working toward the same end, such as Wayne State University in Detroit, Michigan (see Abbey-Lambertz, 2012).
41. See also the arguments along these lines in Denzin & Giardina, 2012, especially pp. 19–22.
42. As Denzin stated in reflecting on the state of tenure vis-à-vis qualitative inquiry: “I'm aware of three tenure cases this year where people are being turned back for tenure by campus committees and deans, promotions committees, because they're doing first-person narratives and autoethnography. And they're being turned back by people who don't have a clue about this work and who are passing judgments on this work” (Ellis, Bochner, Denzin, Goodall, Pelias, & Richardson, p. 332). The impetus, then, is on us to make sure this doesn't happen.
43. This is an allusion to Noam Chomsky's famous 1967 article “The Responsibility of Intellectuals.”
44. For example, multiple book awards, successful promotion and tenure cases, founding of a research center at our university that aligns with qualitative methods of inquiry, etc.
45. In our own department, we have been successful in lobbying for publications that appear in mainstream outlets (e.g.,

New York Times, *The Atlantic*, etc.) to “count” in annual reviews and for merit bonuses.

46. We thank the students of Giardina’s Spring 2013 “philosophy of inquiry” doctoral seminar for pushing us on this point: Cole Armstrong, Elizabeth Delia, Mark DiDonato, Don Farr, Jamie Kim, Rhonda Ottley, Pu Haozhou, and Rachel Shields.
47. Full disclosure: I (MDG) am the associate director of the International Congress of Qualitative Inquiry.

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A Brief Statement on the Public and the Future of Qualitative Research

Patricia Leavy

Abstract

Patricia Leavy, editor of the *Oxford Handbook of Qualitative Research*, offers some final comments about the future of qualitative research. Leavy suggests there is a widespread move from a disciplinary to a transdisciplinary research structure in which problems of import are at the center of research practices. Within this context, qualitative researchers are well positioned to advance because of their ability to develop responsive and flexible research designs and present their work in multiple formats. Furthermore, Leavy notes how the broader move toward public scholarship is propelling both the practice of qualitative research and the teaching of qualitative methods.

Key Words: Qualitative research, public scholarship, public intellectual, transdisciplinary, arts-based research, service-learning, teaching qualitative research

The candle is not there to illuminate itself.
 – Nowab Jan-Fishan Khan, 19th century

The contributors of the *Oxford Handbook of Qualitative Research* have done a wonderful job of considering the future directions of the methodological genres they have reviewed. Although I will not replicate their work here, I would like to make some observations about the academic research landscape at this time and the promise it holds for qualitative research.

As noted from the outset of this handbook, qualitative research developed in the context of critique—as an alternative to positivism and thus the target of criticism. Therefore it is not surprising that many qualitative researchers would choose to characterize the present time in relation to that longstanding critique and the “contested” nature of competing research traditions.¹ For example, this perspective has been expressed by Norman K. Denzin and Yvonna S. Lincoln, leaders in the community. Denzin and Lincoln (2012) suggest

that today the qualitative community exists between two extremes. They suggest that, on the one hand exists the evidence-based movement that calls for scientifically based, positivistic research (which is responsible for the surge in mixed-methods research in which at least qualitative researchers can somewhat participate even if their work is at times marginalized). On the other hand, there are calls for critical, transformative, and social justice-oriented research.

Although it is true that the expansion of qualitative research has always garnered pushback, I believe nonetheless that we are in an era of rapid growth, expansion, and innovation.² When considering the “pushback” to the evolution of qualitative practice, bear in mind that a researcher’s methodological work can be categorized in one of three ways: as creating and/or experimenting; as documenting, chronicling, and synthesizing; and as critiquing.

In other words, there are some who do, some who document what others do, and some who serve as critic—and some who take on more than one of these roles at different times, although creation, documentation, and critique remain three different activities. Make of that observation what you like.

As innovation and creativity within the qualitative community has been well documented in this handbook and exclusively so in other texts, and as I choose to focus on the promise and growth and not the critique, I focus my closing observations on positive changes within the academy and their impact on qualitative practice. The broader move in higher education toward public scholarship (making research relevant and accessible to the public) has been a boon to the qualitative research community. This momentum will continue to build. With this in mind, I conclude with a discussion of transdisciplinarity, the move toward public scholarship, and teaching the next generation. Before I do so, I would like to make a brief suggestion about the language we use to talk about research findings.

In this chapter, I write about the changes in the world and the corresponding ever shifting academic landscape and how these shifts are propelling research in new formats to reach diverse audiences. When I talk about the structure of research (from disciplinary to transdisciplinary) and the forms public research takes (popular and expressive), I am really getting at the issue of “shapes.” This is something I have become passionate about in recent years (please see Leavy, 2011, p. 142). Researchers often use the language of form or format to talk about the structure of research reports, but I suggest we turn to the word *shape* instead. The word “shape” speaks to the form of our work, but also to the way that the form *shapes* the content and how that content is received by audiences. Therefore, I suggest we think about representing research finding in terms of “shapes.” As I will stress throughout this chapter, to address different issues successfully and communicate effectively with diverse audiences, we need to be able to think, see, and build in different *shapes* and ultimately to produce knowledge in different shapes—transdisciplinary, artistic, popular shapes. By emphasizing the need to see and create research in different “shapes,” I also hope to highlight the ongoing role of the research community in shaping our knowledge-building and transmission practices.

Transdisciplinarity

The academic landscape has changed dramatically over the past two decades. Whereas during

the past century the academy was dominated by a disciplinary structure, there has been an enormous increase in multidisciplinary, interdisciplinary, and now transdisciplinary approaches to knowledge building. As I have suggested in earlier work, the international research community has entered a new era, one characterized by transdisciplinary research practices (Leavy, 2011). Whereas positivism dominated under a disciplinary structure, with its discrete borders, qualitative approaches to research are thriving in the contemporary transdisciplinary context. Transdisciplinary approaches to research are issue- or problem-centered, meaning that they prioritize the problem at the center of research over discipline-specific concerns, theories, or methods (Leavy, 2011, p. 35). These approaches to research require innovation, emergence, creative thinking, flexibility, and high levels of cross-disciplinary collaboration and integration, all of which are within the purview of qualitative researchers.

To address real-world issues or problems as fully as possible, transdisciplinary approaches to research are holistic and synergistic (see, e.g., Flinterman, Tecler-Mariam-Mesbah, Broerse, & Bunders, 2001; Klein, 2000; Leavy, 2009; Messerli & Messerli, 2008). In fact, some suggest that transdisciplinarity is a response to the demand for holistic approaches to knowledge building (Flinterman et al., 2001). The outcome of this process is an integrated form of knowledge that is larger than the sum of the parts that went into creating it (Flinterman et al., 2001; Hadorn et al., 2008; Horlick-Jones & Sime, 2004; Klein, 2004; Macdonald, 2000; Newell, 2000; Pohl & Hadorn, 2007). Transdisciplinarity also necessitates innovation (Lawrence, 2004; Van Manen, 2001; Russell, Wickson, & Carew, 2008). Researchers are building new conceptual structures, methodological frameworks, theoretical frameworks, and strategies for evaluation (Leavy, 2011). Because these projects are always issue- or problem-centered, those needs supersede disciplinary norms and demand researcher flexibility. As bricoleurs, qualitative researchers have been well positioned to harness the potential of transdisciplinarity. To say that qualitative researchers are engaged in bricolage is more than an act of labeling the work of qualitative researchers and much more so a way of explaining and conceptualizing the qualitative tradition. Under this conception, the qualitative researcher has long been engaging in activities that are needed in a transdisciplinary research context. The changes that are pushing us from a disciplinary to transdisciplinary research structure may

drive up the demand for researchers with qualitative expertise.

A number of extraordinary changes within and beyond the academy are driving transdisciplinarity. It is important to briefly note these changes because they are also indicative of the growth in qualitative research practice. To begin, the social justice movements of the 1960s and 1970s—the women’s movement, the civil rights movement, the gay rights movement—all exposed rampant inequalities. Later, the environmental movement illuminated the dysfunctional relationship between the social and natural worlds. Within the academy, these movements changed who was conducting research, what topics and populations received attention, and what methodologies were employed. Since that time, a range of critical perspectives emerged, including but not limited to feminist, critical race, queer, embodiment, post-modern, post-structural, and Indigenous theories. One of the major outgrowths of these changes has been the establishment and later the normalization of a range of “area studies” that developed in an interdisciplinary and power-attentive context. Importantly, these “area studies” are all “fields forged in critique” (Klein, 2000), a situation that has influenced their guiding theories and corresponding methodological strategies. Disciplines across the academy have felt the effects of critical area studies such as gender studies, black studies, cultural studies, Chicano/Chicana studies, and environmental and sustainability studies. Globalization and technological advancements have also impacted both the subject matters investigated and the way we go about studying them.

Perhaps the most significant changes that have fueled transdisciplinarity are taking place outside of the academy and center on the nature of contemporary problems and the relationship of the public to both those problems and those entrusted with solving them. The problems facing humanity are enormous, complex, and diffuse. Some of the greatest challenges include but are not limited to the environmental crisis, sustainability, health and well-being, poverty, violence, and radically inequitable development on a global scale. No one discipline has or can meet these challenges (Leavy, 2011, p. 49). To effectively address these problems, researchers have sought to develop new ways to do their work. In this regard, transdisciplinarity can be understood as a collaborative method of “responsive problem solving” designed to help us meet the global challenges of our time (Russell et al., 2008, p. 464). Researchers have decided to cross

disciplinary borders, collaborate with nonacademic stakeholders, and build new conceptual frameworks to effectively address these issues. Many qualitative researchers have experience in action, participatory, or community-based research, and their experiences in working with nonacademic stakeholders is of enormous value in transdisciplinary collaborations. Furthermore, qualitative researchers often have experience with flexible research designs and/or the use of emergent methods.

It is worth elaborating on what an evolving or “responsive methodology” (Wickson, Carew, & Russell, 2006) entails because it suits the typical training of many qualitative researchers. As opposed to a set of linear steps researchers may follow, there are periods of cycling back, retesting or requestioning, and making modifications to the design based on new insights (Leavy, 2011; 2012). Therefore, research strategies must be flexible enough to allow for adaptation to new insights (Wickson et al., 2006). Fern Wickson and colleagues define a responsive methodology as “iterative and an ongoing part of the research process...evolving methodology (p. 1051).” Similarly, J. Francisca Flinterman and associates (2001) suggest a “spiral” model of research design that follows the principle of recursiveness. Christian Pohl and colleagues write: “Recursiveness (or iteration) implies foreseeing that project steps may be repeated several times in case of need. The possible limitation or uncertainty of a preliminary result thus becomes a means of targeted learning. Recursiveness is important in all phases of the research process (2007, pp. 22–23).” By applying the principle of recursiveness, researchers avoid rigid research strategies in favor of a dynamic research design that is influenced and strengthened by additional learning. This approach acknowledges that researchers don’t have all of the answers in advance, which is why the research is warranted. Recursiveness can therefore be understood as a way of enacting reflexivity (Pohl et al., 2007), which has long been a priority in the qualitative community. Moreover, this approach to research requires creativity and is best executed when researchers view their work as a craft. In short, these methodological principles are familiar territory to qualitative researchers and far less so to positivist thinkers (who may require more training to adapt to the current landscape). The de-disciplining of the academy, coupled with the need to develop problem-centered approaches to research, has fueled the use and development of qualitative approaches that can

be employed on their own or as a part of multi-method or mixed-methods designs.

A moral imperative drives this work as well. Although there has long been a contingent in the qualitative community who cite social justice as a driver of their work, the moral or ethical imperative pushing transdisciplinarity forward centers on the urgency and scope of the problems at hand and the public's interest in effective, real-world, practical solutions. Researchers have a moral and ethical obligation to use all available tools and to work collectively, as needed, to address pressing social needs and thus function positively within society (Leavy, 2011, p. 51). Richard Ernst has written extensively about researchers' obligation to the public good. He writes: "We academics are obliged to develop wisdom for comprehending the transdisciplinary and transcultural connections that provide clues for solving of the major pending problems. We do not need mere experts knowing everything about very little. Encyclopedic knowledge is better stored in databases. Society is in need of innovative and initiative citizens who are ready to assume responsibility" (2008, p. 129). In short, research should serve public interests in order to be useful beyond the academy (Leavy, 2011).

Public Scholarship

The widespread move toward public scholarship continues to propel the qualitative community. Historically, researchers were charged with the mandate of "publish or perish"; however, the new mantra may be "go public or perish." Researchers are encouraged to engage in dialogue with different audiences as a part of "public communication" (Cannella & Lincoln, 2004; Woo, 2008). The changes that have fueled transdisciplinarity are also pushing the academy to make research more widely accessible. The effects of the social justice movements shed light on the importance of inclusion within knowledge building and dissemination. The nature of contemporary challenges and the consequent push toward "usefulness" as a research benchmark are also at play. The public has become increasingly engaged regarding problems of import, due in part to increases in education as well as to access to information via the internet and other media. Many have lost faith that the major scientific and political institutions are equipped to deal with issues of import and consider academics to be out of touch with reality. The image of the academic sitting in an ivory tower conversing only with his or her colleagues is all too common. In short, both within and beyond the academy, many

are concerned that the research community has neglected their public role (Woo, 2008). It is within this context that there has been a major shift toward public scholarship that aims to be jargon-free, accessible, and relevant. In some contexts, this includes a turn toward participatory methods in which non-academic stakeholders are brought into the research process, but this is not always the case.

Historically, researchers have presented their work at academic conferences to their professional peers and published their findings in articles in peer-reviewed journals or as monographs. The public has had little or no access to this work, which circulates only among an elite few with particular access and highly specialized training. Although there have always been some public intellectuals, now, many scholars are looking to new mediums to more effectively, rapidly, and widely share their research findings, to bring their research into the communities in which they are enmeshed. These alternative mediums and channels include but are not limited to internet postings, photoblogs, and websites; newspaper stories, op-eds, and other media outreach; short stories, novellas, or novels; brochures, newsletters, or informational pamphlets distributed in local organizations, businesses, or community spaces; radio broadcasts or podcasts; theatrical, musical, or dance performances in community or private sector venues; visual art or photography displays in community art galleries or other accessible spaces; and documentary, ethnocinematic, and narrative films.

Often, you will hear that these new forms or "shapes" are simply a result of changing technology. Although it is true that technology is a useful and at times necessary tool in creating research in new formats and distributing it widely, as Phillip Vannini (2012) rightfully points out, technology is merely that—a tool. Public scholarship is increasing because of an awareness of the kinds of problems we are facing, as well as because of shifts in how we think about our role as knowledge producers enmeshed within "real" world contexts, not outside of them. Our views and goals have changed and will continue to do so. So, too, will our skill sets. The turn to "popularize" research is a result of our desire to make our research more accessible and thus valuable (Vannini, 2012). Technology, like art, is a tool through which we may do so.

Qualitative researchers are well positioned to produce research in "popular" forms and distribute them to relevant, multiple audiences. In fact, Vannini (2012) notes that the qualitative research

community has been at the forefront of developing genre-blurring, engaged, and reflexive forms of scholarship that have the potential to reach multiple stakeholders, such as arts-based research. Not only does qualitative research produce knowledge in formats conducive to public scholarship, but qualitative practitioners also are adept at being flexible, innovative, and problem-centered and have therefore been taking advantage of new opportunities to reach the publics they wish to serve.

For students, new researchers, or those now being influenced by transdisciplinarity and moves toward public scholarship, there are challenges to consider. It is important to bear a few things in mind when thinking about popularizing your research findings through a popular (e.g., media- or internet-based) or expressive (e.g., arts-based) medium.

First, presenting your work in different formats and to multiple audiences requires being able to write or speak in different ways, which includes thinking seriously about language, structure, and audience. Katharyne Mitchell (2008) has written extensively about practicing public scholarship and writes the following about her experiences when presenting her work outside of university settings: “I have radically changed my language, including the vocabulary and even grammatical structure of my sentences. But I have retained most, if not all, of the content, and in a number of cases, I have felt that editorial suggestions made my writing crisper and stronger” (p. 2).

Second, you need to learn about the medium you are working with. For instance, if you’re using a popular medium like op-eds, you need to learn to write in that format in order to get your work published. There is a great deal of instruction that can be found for free online. As with anything, practice is needed. If you’re turning to an arts-based approach, as Kip Jones (2012) notes, you can’t simply take your interview transcript and rearrange it on the page to mimic a poem and then call yourself a poet. You need to learn about the craft of poetry. When publishing in new venues, bear in mind that you may need to start small and work your way up (e.g., submitting to a local newspaper to build your credibility before sending your work to *The New York Times*).

Third, there may be a personal cost to producing public scholarship (Mitchell, 2008). When you put your work and ideas out there, you can’t control what you get back, and this may involve emails or letters from those who disagree with you or bad reviews or public critiques of your work. Academic

researchers may pay a price as well, depending on how their colleagues, institution, and tenure policies value their work. You may need to develop a thick skin and stay true to the value system guiding your work. Despite low points and challenges, those who do this work usually claim that the rewards far outweigh the costs (Mitchell, 2008; Zinn, 2008).³

There are other ways that qualitative research is of value to those aiming to engage the public. Some are also using qualitative techniques to develop ways to translate their work into popular media, allowing them to publish in both traditional and popular formats. Here, an outstanding example comes from education researcher Yen Yen Jocelyn Woo (2008) who used qualitative methodologies to translate her research findings into a 105-minute social-realist narrative film titled *Singapore Dreaming*. Woo conducted in-depth interviews with young people in Singapore and New York about their sense of legitimate and illegitimate ways of spending their time, as well as about their sense of desirable and undesirable life paths (2008, p. 322). Woo’s turn to film was based on the following question: “how can we reach new audiences with our work, so that we do not just converse among ourselves but rather increase the relevance of our work toward ameliorative educational goals and achieve greater influence with the public?” (2008, p. 321). *Singapore Dreaming* premiered at the Singapore International Film Festival in 2006, in Singapore’s largest digital theater, and has subsequently had a commercial theatrical release and a television and DVD release, and has been screened in schools, community centers, churches, and other community spaces. In short, through her turn to a popular expressive format, Woo has been able to engage multiple audiences with her research findings. This makes the work relevant, meaningful, and useful. It is a value-added proposition.

A dialectical relationship exists between transdisciplinarity and the move toward public scholarship, with each informing the other. Transdisciplinary collaborations, which prioritize the issue or problem at the center of research over discipline-specific concerns or methodologies, produce research that is often represented in more than one format, thus opening the doorway for nontraditional approaches to representation or experimentation. Furthermore, a desire to represent research findings in alternative forms, for example, in expressive arts-based forms, may push researchers toward cross-pollination. As noted earlier, working in new mediums requires learning about the craft you are adapting, which can help researchers forge cross-disciplinary

collaborations (Leavy, 2009; Saldaña, 2005), which in turn fuels both transdisciplinary training and public scholarship.

Teaching

The push toward public scholarship, coupled with an emphasis on social justice, has also influenced teaching in the social sciences in ways that are advancing qualitative practice. Students in the social sciences are increasingly expected to engage in some kind of service-learning activity that may range from an unpaid internship to volunteer work in a community-based organization. Service learning benefits students in several ways including professional development, forging a community with their peers, kinesthetic learning, engaging with ethics in practice, doing community-oriented or activist work to build a sense of social awareness, and helping students learn research methods in practice (Machtmes et al., 2009). The coursework component of service-learning courses often relies on qualitative practices that may include ethnographic observations, informal interviewing, reflective journaling, and literature reviews (Machtmes et al., 2009). Thereby, the emphasis on service learning, which is inextricably bound to the move toward public scholarship, has the intended and/or unintended consequence of teaching students qualitative principles and practices.

The training of students in human services also requires the teaching of qualitative practice. Many now recognize that qualitative research is an “inherent part of the human services profession” (Goussinky, Reshef, Yanay-Ventura, & Yassour-Borochowitz, 2011, p. 126). Ruhama Goussinky and colleagues suggest that human services students need multidimensional qualitative training because they must actually internalize the nature of qualitative research and in essence “become” qualitative researchers (think and act like qualitative researchers) if they are to be effective in their field.

Certainly, the effects of the justice movement, felt throughout the academy, also influence the teaching of qualitative research in ways that are propelling us forward. The qualitative research community has only recently begun developing materials about how to teach qualitative research (Denzin, 2010). Therefore, qualitative researchers have typically taught their practice as they learned it—by doing. Learning qualitative research through practice has emerged as a fundamental pedagogical strategy (Hurworth, 2008). According to Maureen

Duffy (1995), students should be able to ask themselves: Can I apply what I learned in my life, put theories into practice, and identify practical applications for what I have learned?

Moreover, because qualitative research methods developed with a strong set of social justice aims—such as giving voice to those at the margins—the teaching of qualitative research has typically occurred within a social justice context. In this vein, Denzin notes: “I want to create a space for a dialogue between all pedagogies that link qualitative inquiry with social justice incentives” (2010, p. 51). Denzin further notes that when he began teaching qualitative research in the 1960s he “did not understand that I was teaching students methods for representing social action and making the world visible” (2010, p. 52). In this regard, Duffy (1995) asserts, learning is an “ethical activity.” She suggests students should be able to ask themselves: Can I use this information to help others? What can I integrate into my belief/moral/value system?

Here, we can see how the normalization of teaching qualitative research will further fuel public scholarship and raise up a generation of researchers who understand the ethical or moral substructure of research. Furthermore, because qualitative research is most often taught through a practice model, the mentoring relationship between professors (researchers) and students is paramount. Experienced researchers thus have the opportunity to model transdisciplinarity and public scholarship for their students and train them for the challenges that are inherently a part of these endeavors. Even more so, researchers can model the value system that guides their research choices and what ethical practice looks like in action.

Final Words, for Today...

One could approach the task of suggesting where the field of qualitative research is heading in many different ways. In fact, there are scholars I respect enormously who have written on the subject with a wholly different focus. As I conclude this chapter and contemplate what words, thoughts, and points to leave you with, I am confronted with our relationship (author and reader) and, by extension, the research relationships through which we carry out our work. Most simply, I am left thinking about our roles as researchers and how those roles evolve in the shifting landscape that shapes us as we shape it.

Although traditional approaches to qualitative research remain a staple in the field, there are new opportunities for qualitative researchers as a result of

transdisciplinarity and moves toward public scholarship. This makes it a very exciting time. In this chapter, I have emphasized how transdisciplinary collaboration and public scholarship can assist us in reaching our research objectives and ultimately produce research that is of greater value to the publics we aim to serve. There are personal benefits as well. The current research landscape values connection and engagement, which are fundamental to the feeling of being fully present and “alive.” Coupled with having access to useful research tools, as well as the ability to co-create the new tools needed to properly engage with questions and issues of import, qualitative researchers work in a time in which we may be of real value. Julie Ellison (2008) has remarked that what truly distinguishes public scholarship is its “explicit hopefulness.” I suggest that hope is fortified with equal parts determination and creativity. In reading the collection brought forth in this handbook, that is just how I feel: connected, engaged, armed, and hopeful.

Notes

1. Of course, different research traditions need not be conceptualized as competing but rather as complementary, so here I am speaking about how it has been, not how it should or could be.
2. There is ample documentation of innovation in qualitative research practice. For example, Sharlene Hesse-Biber and I compiled two large edited volumes on “emergent methods” in social research: *Emergent Methods in Social Research* (2006, Sage Publications) and *Handbook of Emergent Methods* (2008, Guilford Press). This explosion in innovative approaches to methodology has naturally drawn criticism as well. For example, some have argued that claims of innovation are greatly overstated and are used to get attention and that “new” approaches like arts-based research are essentially fads (see Travers, 2009; Wiles, Crow, & Pain, 2011). However, if you look at these articles (their highly questionable data collection procedures, the basis for their claims, and their obvious disdain for advances made by creative qualitative researchers), it seems ridiculous to give this work credence. Furthermore, these authors essentially claim that the label “innovative” is frequently used to describe new twists on old methods versus “truly” new methods and thus that the term “innovative” or some such term does not apply. This, too, is an erroneous claim. Adaptations, enhancements, and new ways of approaching methods do in fact make them different. Think about this: when I was in college, I could listen to music while out and about with my Walkman (which played tape cassettes). Are we to say that because that technology existed, the Ipod isn’t an innovative (and, in this example, more effective) way of listening to music? And, of course, there were “inventions” or “adaptions” in between, such as the Discman (which played CDs). Do we really believe these are all essentially the same?
3. For some, public scholarship comes with the territory. For example, Melissa W. Wright (2008) posits that the professional experience of feminist scholars in women’s studies

departments differs—with their work and mere existence constantly judged in the public domain. By positioning themselves as openly feminist scholars, they are necessarily “public” scholars, with many actively speaking, writing, and protesting in public forums.

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