Qualitative Sociology, Vol. 25, No. 1, Spring 2002 (© 2002)

Symposium on Methodology in Qualitative Sociology

Introduction: The Methodological Strengths and Dilemmas of Qualitative Sociology

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The articles in this symposium critically reflect upon the methodological strengths and limitations of several diverse yet important works of qualitative sociology, broadly defined: Michael Schwalbe's *Unlocking the Iron Cage: The Men's Movement, Gender Politics, and American Culture* (1996); Paul Willis's *Learning to Labour: How Working Class Kids Get Working Class Jobs* (1977); Perry Anderson's *Lineages of the Absolutist State* (1974); Doug McAdam's *Political Process and the Development of Black Insurgency, 1930–1970* (1982); and Julian McAllister Groves's *Hearts and Minds: The Controversy Over Laboratory Animals* (1997). Among the questions addressed in this symposium are the following: Are the general theoretical or empirical claims of these books persuasive, and are they well supported by the data that are presented by the authors? Are these books persuasive because they adhere to certain methodological rules or standards, if only implicitly? And what are those rules or standards? Or are these books powerful or persuasive despite, or even because of, their lack of methodological rigor, conventionally understood? And would these books have been improved appreciably had they been more methodologically self-conscious or differently designed?

This symposium thus addresses the concern—shared by quantitative social scientists, general readers, and not a few qualitative sociologists themselves—that qualitative sociology lacks methodological rigor and, accordingly, truly reliable or generalizable findings. Some social scientists view qualitative sociology, in no uncertain terms, as methodologically and empirically "soft" and highly subjective, if not completely solipsistic—a characterization that a few qualitative researchers have ironically embraced. At best, according to certain critics, qualitative sociology might generate provisional hypotheses that more rigorous social scientists can then go forth to test and revise, but it cannot itself glean much solid understanding of the social world.

We believe that this view of qualitative sociology is badly mistaken, and the essays in this symposium collectively refute it. Qualitative sociology is not—or need not be—merely literature or navel-gazing, and its findings have proven extraordinarily insightful, persuasive, and influential. At its best, qualitative sociology can be very rigorous and "scientific" indeed. This symposium demonstrates that a significant number of qualitative sociologists, who have not abandoned the idea that qualitative researchers can do scientific or quasi-scientific work as well as quantitative researchers, have produced important and influential research. Qualitative sociology, in short, has some very important things to say about the world beyond the researcher. Accordingly, both quantitative social scientists and those qualitative researchers who have bought into the quantitative critique and embraced subjectivism need to take another look at what qualitative sociology can achieve.

DEFINING QUALITATIVE SOCIOLOGY: HOW "SCIENTIFIC" IS IT?

Grave suspicions about the methodological rigor of qualitative sociology provide the intellectual backdrop to *Designing Social Inquiry: Scientific Inference in Qualitative Research* (1994), a much-discussed methodological text by Harvard political scientists Gary King, Robert Keohane, and Sidney Verba. King, Keohane, and Verba believe that qualitative social scientists need to pursue their research in a more rigorous and scientific manner, which basically means, for them, adhering as much as possible to the standards of *quantitative* research. (Significantly, they do not ask whether quantitative work might be improved by emulating certain features of qualitative research.) King, Keohane, and Verba suggest that quantitative and qualitative research share "the same logic of inference" (1994, p. 3), and they elaborate a number of rules for rigorous, scientific qualitative (and quantitative) research (see Munck 1998). We have extracted and set forth the principal methodological rules of King, Keohane, and Verba in Table 1. (Two of the articles in this symposium explicitly employ King, Keohane, and Verba's text, albeit not uncritically, as a conventional standard for "scientific" qualitative sociology.)

Certainly, judged by King, Keohane, and Verba's rules, much qualitative sociology would be found wanting in various ways, perhaps severely so, a point to which we return below. Of course, just what constitutes qualitative sociology and "its" methodology is notoriously difficult to say. At times the variations among qualitative sociological studies appear to be greater than the similarities. Indeed, a variety of data sources and data-gathering strategies can be classified under the rubric of qualitative sociology: participant-observation, in-depth interviewing, photography and video, document analysis, and archival and historical research. Qualitative studies include ethnographies of groups, places, organizations, or activities; analyses of people's lives and experiences; historical case studies of a wide range of phenomena, including social movements, revolutions, state-building, and other political phenomena; and comparative historical analyses. (King, Keohane, and Verba focus mainly on the latter two types of research, which are more prevalent in political science.)

Table 1. King, Keohane, and Verba's (1994) Basic Rules for "Scientific" Qualitative Research

1. "Construct falsifiable theories" (p. 100). "Choose theories that could be wrong" (p. 19).
2. "Build theories that are internally consistent." "If two or more parts of a theory generate hypotheses that contradict one another, then no evidence from the empirical world can uphold the theory" (p. 105).
3. "Dependent variables should be dependent. A very common mistake is to choose a dependent variable which in fact causes changes in our explanatory variables" [the problem of endogeneity] (pp. 107–108).
4. "Do not select observations based on the dependent variable so that the dependent variable is constant" (p. 108) "Selection should allow for the possibility of at least some variation on the dependent variable" [i.e., do not "sample" or "select on the dependent variable"] (p. 129).
5. "Maximize concreteness." "Choose observable, rather than unobservable, concepts whenever possible." "Abstract, unobserved concepts . . . can be a hindrance to empirical evaluation of theories and hypotheses unless they can be defined in such a way that they, or at least their implications, can be observed and measured" (p. 109).
6. "To make sure a theory is falsifiable, choose one that is capable of generating as many observable implications as possible" (p. 19).
7. "In order better to evaluate a theory, collect data on as many observable implications as possible" (p. 24).
8. "The more evidence we can find in varied contexts, the more powerful our explanation becomes, and the more confidence we and others should have in our conclusions" (p. 30).
9. "State theories in as encompassing ways as feasible." "The theory should be formulated so that it explains as much of the world as possible" (p. 113). "One of the most important achievements of all social science [is] explaining as much as possible with as little as possible" (p. 29).
10. "All data and analyses should, insofar as possible, be replicable" (p. 26).

Many different approaches to theory-building, moreover, are employed in qualitative work, including hypothesis testing, filling gaps and resolving anomalies in theories, and inductive approaches. Additionally, no one theoretical perspective links qualitative research. To make sense of their data, qualitative sociologists employ, among other theoretical frameworks, versions of Marxism (Burawoy 1979; Burawoy et al. 2000; DiFazio 1985; Willis 1977), structuralism (Liebow 1967), historical institutionalism, cultural analysis (Lamont 1992; Willis 1977), Chicago school ecological approaches (Suttles 1968; Kornblum 1974; Venkatesh 2000), and symbolic interactionism (Anderson 1976; Horowitz 1995; Snow and Anderson 1993). At times, it may appear that qualitative sociology is simply a residual category—all sociology, that is, which is *not* quantitative or purely theoretical.

However, despite the many differences in approaches, techniques, and theories in qualitative studies, most of them are similar in their emphasis on capturing or representing in considerable depth or detail what is or was going on in one or a few "cases" of something judged socially significant. Indeed, one link among qualitative studies is their rich descriptions or narratives of cultural, emotional, and social life, sometimes in a comparative framework. Most qualitative studies are generally not about "attitudes," "norms," "roles," or other abstract concepts, but more about what people actually say and do in specific places and institutions, including their interactions with others over time—in other words, how social things (relationships, events, cultures, organizations, and movements) occur or develop in social and temporal context (Morrill and Fine 1997). Rich descriptions and narratives of specific cases, then, are the bridge that connects qualitative sociological studies. Concepts like "attitudes" or "norms," if they are employed at all, are used by analysts more to inform and organize the presentation of social life as it occurs or has occurred.

Generally, then, qualitative sociologists—whether street-corner ethnographers or comparative historical analysts—attempt to remain as close as possible to the actual phenomena that they are trying to understand. They believe that their cases, whatever they may be, have to be understood contextually or holistically, and often with attention to temporal ordering. Cultural and historical specificity matter enormously. By contrast, the type of general "variables" that quantitative social scientists employ are usually not very illuminating of the types of phenomena that interest qualitative sociologists; such variables may facilitate statistical analyses of many cases, but yield only a thin understanding of any particular case—and most people, arguably, care only about particular cases. Qualitative sociologists, moreover, generally look not only for social patterns, regularities, or statistical means, but also for the exceptions or anomalies that tell them (and all of us) something new.

Of course, understood in this way, qualitative sociology would seem to be rather unevenly "scientific." On the one hand, qualitative researchers do tend to eschew abstractions and to "maximize concreteness" when employing concepts (rule 5 in Table 1). And qualitative research can yield exceedingly rich data on the observable implications of particular theories (rule 7), which is in turn necessary for testing, falsifying, and modifying theories and hypotheses (rule 6). On the other hand, qualitative researchers also tend to "sample on the dependent variable" (violating rule 4) and are not always concerned with gathering evidence in "varied contexts" (rule 8) or with generating encompassing theories that explain "as much of the world as possible" (rule 9). Qualitative research is said to suffer from an alleged "small-N problem," failing to examine a sufficient number of cases for building solid generalizations or good theory. Participant-observers, moreover, are often charged with inducing (or even provoking) much of their data, which means that such data may not be replicable (violating rule 10). More generally, participant-observers have been accused of lacking objectivity or critical distance from the groups or institutions in which they insert themselves.

Can anything be done to make qualitative sociology more "scientific"? And should we try? In fact, as the essays in this symposium demonstrate, good qualitative work is not only empirically rich, but is often more methodologically rigorous than might appear. The studies under review in this symposium are generally well designed and, accordingly, are insightful, persuasive, and (in some cases) enormously influential—more so, generally speaking, than quantitative studies of the same topics. There are good reasons, moreover, that these studies do not all adhere scrupulously to certain conventional methodological standards. Consider King, Keohane, and Verba's admonition (rule 9) to formulate theories so that they "explain as much of the world as possible." This rule is fair in principle. But given their commitments to cultural and historical specificity, many qualitative sociologists are simply not interested in this approach to theory. Others quite reasonably doubt whether any particular theory or hypothesis can in fact be especially encompassing in this sense, at least in a nontrivial way. Put differently, the most encompassing theories that are also interesting may in fact illuminate only a small (but hopefully important) corner of the world. And that is no mean achievement. The "small-N problem," to take another example, is no problem at all when only a few major instances exist of the phenomena one wishes to understand (e.g., revolutions, genocides, and racially integrated middle-class neighborhoods in the U.S.). It is difficult *not* to "sample on the dependent variable" if one is interested in such things. This "problem" may also be of minor significance when one is trying to understand how a process evolves over time in a specific setting.

There are also distinct advantages to employing qualitative methods that critics tend to overlook. Close engagement with their cases typically requires qualitative researchers to adapt existing theories or to make new conceptual distinctions or theoretical arguments to accommodate new data. Qualitative research, in other words, may be more conducive to theory-building than is quantitative work. For example, if one examines the ethnographic research that has contributed to our understanding of the barriers and bridges to class mobility and formation in the United States, one sees an incredibly nuanced picture (Horowitz 1997). While the studies are varied in terms of the theories and explanations they develop, their differences are less pronounced than one might imagine from their disparate theoretical approaches. Although structural functionalists tend to see mobility as reasonably feasible for Americans, they also see strong barriers as they watch people struggle to cross boundaries. Conflict theorists, on the other hand, focus more on the barriers, but they also see the people who are able to move around, over, or through such barriers. In most qualitative studies, people work to make places for themselves, for example, by using the culture of organizations to construct their own cultural world (Morrill and Fine 1997). The data in this research take precedence, to some extent, over the theory.

THE TREND TOWARD GREATER METHODOLOGICAL SELF-AWARENESS

Qualitative methods and research have undoubtedly become much more transparent in the last twenty-five years. When anthropologists first went into the field or the Chicago sociologists explored that city during the 1920s (Anderson 1923; Cressey 1932; Wirth 1928), the research product gained legitimacy primarily from the status of the researchers and from the fact that they "had been there." Few talked about how the research was actually conducted, and the researcher never appeared in the finished text. Few books even had methodological appendices. William Foote Whyte's second edition of *Street Corner Society* (1943[1955]), which included an enlarged discussion of his methodology, violated convention. When Laura Bohannan's *Return to Laughter* was published in 1954, an anthropological description of fieldwork, it was published as fiction under a pseudonym (Elenore Bowen). Buford Junker published one of the first qualitative methods texts only in 1960.

Although how people did their research became a topic of discussion in the 1960s, throughout that decade researchers rarely appeared in their books. Participant-observers were viewed as "flies on the wall." Who the researchers were and how they interacted with the people they were observing or interviewing was not a "variable" that needed attention. Qualitative work proceeded like other types of sociological research—it was simply assumed that a social world existed that any good observer could more or less clearly see. The trust of readers was secured by the professional status of the researcher and the amount of time and effort invested in the research. There was, in short, little need to describe the observer or to analyze his or her position relative to the people being studied, changes in relationships over time (Horowitz 1986), issues of power (Sjoberg and Vaughan 1993; Smith 1990, 2001), or even sampling (Gerson and Horowitz 2001). These are all newer questions and concerns. The convention was a silent and hidden observer (Fine 1993). Although the variable position of the researcher—politically, socially and culturally—was certainly known to affect the research *question*, few thought that this variation might affect the particular research methods chosen and the ways of collecting and analyzing data.

There has also been a trend in recent years toward greater methodological self-awareness among historical and comparative historical sociologists. Historical researchers have become sensitized to possible biases in documents and other historical sources (Milligan 1979; Platt 1981; Lustick 1996). Furthermore, a series of texts published in the mid 1980s by Skocpol (1984), Tilly (1984), and Ragin (1987) examined the various logics behind case comparisons, generating ongoing debates about comparative methods and small-N research (Lieberson 1991; Goldthorpe 1991; Mann 1994). As a result of these debates, comparative sociologists have been much more self-conscious about issues of case selection and possible selection bias. There is also greater sensitivity to causal complexity, the "path dependence" of certain processes, and what Ragin (1987) calls "multiple conjunctural causation"—the possibility and even likelihood that certain complex phenomena (e.g., ethnic violence, social movements, and democracy) may be the result of different and perhaps unique causal configurations in different contexts.

In the early 1990s, furthermore, the very concept of the "case" as well as the uses and possibilities of single-case studies were critically interrogated (Ragin and Becker 1992; Feagin, Orum, and Sjoberg 1991; Amenta 1991). Again, the result has been greater self-awareness about which "units of analysis" are appropriate and fruitful to examine and compare. There is also greater awareness that because the cases we study (organizations, movements, temporal processes, and national societies) are nested within larger social and temporal contexts (e.g., the global capitalist "world system," the interstate system, or "waves of protest") they cannot be treated as isolated monads driven or explained by purely endogenous forces (Tilly 1995).

Some of the methodological issues raised by qualitative researchers parallel quantitative approaches, while others are more unique to qualitative work. Ironically, while many quantitative sociologists have viewed qualitative work as "merely" humanistic and without methodological rigor or sophistication, qualitative researchers have been zealous during the last twenty-five years in exploring and questioning virtually all aspects of data gathering, data analysis, and the presentation of research. Few aspects of the research process have not been thoroughly analyzed or deconstructed. Even writing field notes has been a subject of extensive discussion (Emerson, Fretz, and Shaw 1995; Sanjek 1990). The validity of data, especially, has become a major issue for both field researchers and historical sociologists. Qualitative researchers in anthropology and sociology have debated what "data" is and how it can be collected and by whom (Denzin 1994, 1997). Epistemological questions about how researchers can know anything outside themselves have been explored. Debates also evolve over issues of data analysis. In ethnography, debates have erupted about how one can determine the meanings that others attach to their actions, and interviewers have asked whether conversations provide data about what actually happened, what people think happened, or what they wish the interviewer to believe happened.

Other questions that have concerned qualitative sociologists are more political in nature. For whom does the researcher speak? Are there groups whose voices we do not (or cannot) hear? Are our concepts and even the questions we ask ethnocentric or gendered? Years ago, Howard Becker (1967) asked, "Whose side is the researcher on?" More recently, questions have arisen about the writing styles (Van Maanen 1988) and styles of representation (Van Maanen 1995) found in ethnographies.

At every stage of the research process, then, qualitative researchers are now forced to think about methodological issues and choices. The choice of a field site (or sites) must be related to the research question and to theory. Research strategies (participant-observation, in-depth interviewing, photography, archival research, and document collection) must be chosen according to one's research questions—strategies that can change, moreover, as the research proceeds. The choice to do participant-observation in multiple sites or to examine several historical cases needs a sampling justification in terms of the question asked. Why might comparisons among particular groups, for example, be helpful (Morrill 1995)? Interview subjects need to be chosen for a theoretically justifiable reason; random samples are generally not possible, yet comparisons are often wise. Moreover, talking to a few research subjects is generally not sufficient to justify an understanding of social patterns or regularities. However, individual biographies are sometimes justifiable when they illustrate points about theoretically generated ideas or point to hidden links in social phenomena (Becker 1966; Bennett 1981). (For C. Wright

Mills, of course, the ability to link individual biography with social structure was the essence of the "sociological imagination" [1959].)

While qualitative researchers disagree about the extent to which researchers bias data collection, it is possible to be self-reflexive during the data collection process. Researchers must always ask why they are getting the answers they are, or seeing what they see. Of course, researcher bias can sometimes be mitigated through internal comparisons. For example, adopting multiple roles or positions to check on what the observer sees is often critical, as is talking to people in different positions within the setting. An "organizational culture" will probably not look the same to workers and management—in fact, "a" culture might be difficult to find. How the data are to be presented raises still other dilemmas, including questions about how, and how much, authors should write about themselves. There is much debate over how this should be done, but one reasonable strategy is simply to be explicit in the text (so far as possible) about one's position and biases.

SOME KEY CLAIMS AND FINDINGS OF THE SYMPOSIUM

Traditionally, the researcher gathering data to write an ethnography was unobtrusive both as an observer and, later, as a writer. Not only did the reader discover little about the observer from a careful reading of the book, but the assumption was that the observer made little impact on the groups or organizations being studied. Researchers were told to keep their opinions to themselves while gathering the data. "Blending in" was the goal, and it was simply assumed that it did not matter what the researcher actually did to collect data. Although some participant-observer studies were based on research by people involved as full participants in a setting (Becker 1963; Polsky 1969), others were not (Whyte 1943[1955]; Vidich and Bensman 1960). By the 1970s, participant-observers began to realize that who they were influenced the data that they were getting. There were public debates about whether researchers should remain sufficiently autonomous to enable a critical perspective or try to get in far enough to know "what is really going on" in a social setting (Adler and Adler 1987). Some argued (e.g., Jules-Rosette 1976) that one needed to be completely involved as a significant participant, while others saw too much involvement as compromising objectivity (Miller 1952). For those who both wished to maintain "objectivity" or a critical perspective *and* saw that who they were would influence what they saw and what others told them, it became especially critical to carefully analyze their positions and relationships with the observed. Relationships in the field were extremely important, and how different people responded differently to the researcher was a critical piece of evidence in developing understanding (Horowitz 1986). In short, one needed to become self-reflexive in the data collection process.

Despite arguments for full participation as the only way to really know what is going on, it is quite unusual and certainly nontraditional for the researcher to express personal views that appear to violate those of the people being studied. Yet Brooke Harrington, in her contribution to this symposium, argues that Michael Schwalbe's expression of his own views and his retelling of his involvement with his subjects in *Unlocking the Iron Cage: The Men's Movement, Gender Politics, and American Culture* (1996) generally make this work more credible to the reader. Presenting one's self as an active participant who not only recounts what one saw but also talks about how one attempted to publicly reframe discussions is a new strategy, one which raises questions about the researcher's ability to both fit in and to generate a critical account. Schwalbe violated some of the conventional rules about how one does participant-observation and then writes up one's story.

As Harrington argues, Schwalbe takes a critical stance as a member and researcher when he dares to use a feminist critique at a meeting of a men's group that has trouble dealing with the feminist movement. Then, as Harrington points out, Schwalbe is able to adopt a critical stance toward the movement before the men he is researching—although he did so without alienating most of these men—as well as before his readership. Schwalbe does not argue that this violation of convention makes his account superior to others. He claims, conventionally, that his views are better than popular accounts for "sociologically legitimate" reasons: the length of his research, his focus on the regular members rather than the leadership, and the detail that he is able to present. These are all typical assertions of credibility made by ethnographers.

According to Harrington, Schwalbe, an experienced researcher, stretches the boundaries of accepted methodological practice, while also using more conventional techniques. The presentation of his personal views, Schwalbe argues, allows him to demonstrate his critical stance toward the men's movement by showing his readership that he did not agree with all that was said; to explore the variations among the men of the movement by relating how many were hostile to his statements and how many sympathetic; and to better demonstrate both what the members thought important and the meaning of their actions. Schwalbe took a risk and extended traditional participant-observer practices, and he convinced Harrington that the risk was well worth it. As a researcher, he was honest in expressing his personal opinion to those being studied and in telling the reader where he stood and how others reacted to the expression of his views.

In their examination of Paul Willis's ethnography, *Learning to Labour: How Working Class Kids Get Working Class Jobs* (1977), Danielle Bessett and Kate Gualtieri explain how a "mere" case study of a dozen "deviant" English schoolboys has become one of the most influential and highly regarded works in the sociology of education, culture, and class reproduction. Bessett and Gualtieri are not uncritical of the methodological shortcomings of Willis's study, including Willis's failure to employ comparisons as fruitfully as he might have (and as he promised). Willis fails, in technical terms, to explore variation on his dependent variable. Still, Willis succeeds in amassing an incredible amount of powerful and persuasive information about "the lads," their beliefs, and their social context—information cross-checked against various sources of data and available only because of the close relationship that Willis struck up with the lads. The insights of *Learning to Labour* would simply not have been possible if Willis had maintained a distanced, impersonal, or "neutral" relationship with these boys, or if he had tried to explain class reproduction in the industrial town where the lads live through purely quantitative means.

Because of his close relationship with the lads, Willis is also able to see what more "structural" and/or quantitative accounts of educational attainment and class reproduction cannot, namely, the importance of the lads' own agency and creativity. These boys are not dupes of structural forces, but perceptive social agents who actively create a culture of resistance—a culture not without its limitations and unintended consequences, but one that is unusually perceptive and certainly central to understanding the lads' present and future situation. This is an important theoretical point that has influenced a good deal of subsequent sociological research and theory (see, e.g., Giddens 1984). It is also an insight that could only be attained through qualitative research. *Learning to Labour* succeeds because Willis succeeds in getting inside the lads' cultural world.

With Richard Lachmann's article, we leave behind the microcultural focus of Schwalbe and Willis and enter the macrostructural world of Perry Anderson's *Lineages of the Absolutist State* (1974), a classic comparative historical analysis of absolutism and the rise of the bourgeoisie or capitalist class. Anderson's is one of the most influential accounts of the much-debated transition from feudalism to capitalism, a question for which small-N comparative historical analysis is eminently suited since there are relatively few cases—at least in Europe, where capitalism first developed—to consider. Lachmann points to some of the theoretical limitations of Anderson's Marxist perspective, particularly when it comes to explaining how capitalists allegedly overthrew absolutism by means of the great "bourgeois revolutions" in England and France. Yet Lachmann also praises Anderson's methodology of "comparisons within a single social formation" (i.e., absolutism). Although, in one sense, Anderson's sample is relatively small—he focuses on five Western and four Eastern European countries, with an excursus on Japan—this is quite sufficient, Lachmann argues, for building a powerful and parsimonious theory that is applicable to the range of feudal experience. Anderson's comparative approach allows him to convincingly explain important differences between Western and Eastern Europe, differences within Western Europe, and differences (and similarities) between Europe and Japan. Lachmann notes that while Anderson might have been more explicit and self-conscious about his methodology, this would probably not have resulted in a better study since its main weakness (and strength) is Anderson's Marxist *theoretical* perspective.

Doug McAdam's historical analysis of the U.S. Civil Rights Movement in *Political Process and the Development of Black Insurgency, 1930–1970* (1982) is, if anything, even more influential than Anderson's account of absolutism. As

Michael Armato and Neal Caren point out in their contribution to this symposium, McAdam's book did more than any other to establish "political process theory" as the dominant theoretical approach to social movements in the United States. At first glance this might appear rather surprising, since McAdam examines but a single case of a social movement. Why was this book so successful despite its small-N problem? In fact, Armato and Caren point out, McAdam demonstrates that political process theory is far better able than other existing theories to account for various temporal stages of the Civil Rights Movement—its emergence, "heyday," and decline. Indeed, by breaking up the movement into these temporal stages (including the period prior to collective action), McAdam creates several "cases"—and variation on his dependent variable—that confirm his own theory while contradicting elements of other approaches. To do so, McAdam employs a wealth of (replicable) data from various sources to interrogate myriad "observable implications" of his own and other theoretical perspectives.

In the end, Armato and Caren argue, McAdam still has too few cases and observations to fully substantiate his major theoretical claims or his political process "model" of movements. They suggest that the book remains powerful, however, partly because McAdam is so successful at debunking alternative theories of social movements. Political process theory also became influential, they suggest, because its core concept—"political opportunity"—was sufficiently broad (even to the point of vagueness) that other researchers could easily adapt and employ it in their own work (see also Goodwin and Jasper 1999).

Brian Lowe's article also points to conceptual problems in the analysis and presentation of data in Julian McAllister Groves's *Hearts and Minds: The Controversy Over Laboratory Animals* (1997), an empirically rich study of the animal rights movement. Lowe argues that the use of different concepts might have made Groves's book of even greater importance to the sociological community in general and to social movement analysts in particular. Lowe uses the concepts of "moral vocabulary" and "moral resources" to try to make Groves's work more comparable to other studies of social movements; he also argues that these concepts would help to connect the local culture that Groves is studying to that of other social movements and to broader cultural systems. In short, Lowe argues for a reexamination of some of the data that Groves presents with a different conceptual vocabulary.

In fact, the richness of the data in qualitative studies often permits a variety of analyses and interpretations—something that can be quite frustrating to researchers. The choice of specific theories and concepts is obviously critical to the telling of the story—both the tale in its depth and uniqueness and how that tale is linked to other, perhaps generalizable stories. Lowe argues that Groves's story is too unique and not sufficiently generalizable because of the concepts it uses and fails to use; accordingly, the story seems less important than it is. On the other hand, Lowe also argues that the wonderful depth and richness of Groves's story provides the basis for further analysis. The problem is not Groves's small N or a lack of data, but the need for better concepts, a critical aspect of the analysis of all types of data.

OUTSTANDING ISSUES AND INHERENT DILEMMAS

The issue of generalizability is critical to qualitative sociology and to sociology generally. The key trade-off that all social scientists (not just qualitative sociologists) need to weigh has always been and will remain one between depth ("thick," contextualized data) and breadth (large samples of cases). The strength of qualitative research has been to create a deeper and richer picture of what is going on in particular settings, although it has also been able to employ comparisons among a relatively small number of cases to great effect. Qualitative analysts will always be challenged, however, to engage and interest people who do not happen to share an interest in the particular case or cases that they write about. We are also challenged to contribute to general explanations of the particular class of phenomena of which our cases form a part.

How can qualitative studies speak to general sociology, the social sciences, and even broader audiences without losing what qualitative work does best, namely, the development of richly nuanced narratives and analyses? Sociology develops through a process in which researchers engage with, in some fashion or another, what has been said before. We build upon the past. In doing so, we complicate the picture, change it, or fill in some gaps. There are many different ways to do this, including designing our research so as to develop, challenge, or fill in existing concepts or theories as well as developing new concepts or connections. Comparative qualitative work also works well to develop general explanations when there are only a few cases of the phenomena that we are studying.

What qualitative work does rather less well is to develop convincing explanations of large classes of social phenomena—although, as Willis's and McAdam's work demonstrates, even this is not impossible. Class reproduction involves almost everyone, and there have been hundreds of social movements in the United States alone, yet Willis's and McAdam's case studies have greatly enhanced our understanding of these phenomena. Still, an important question that qualitative researchers always need to ask themselves is how their research connects to some larger story. Of course, whether our data and analyses will always speak to that larger story is another question.

Qualitative research has but a few formulas about how to make this connection between the specific and the general. This does not mean, however, that there are no methodological standards available to qualitative researchers or that qualitative studies lack rigor. Traditional standards have evolved from the "fly on the wall" observer and the neutral interviewer to researchers who are finely attuned to how they affect the research process through their presence. The disagreements are over exactly *how*, and how much, researchers are related to the data they have collected. Standards continue to evolve, and all are subject today to intense scrutiny. Although disagreement exists about the extent to which researchers should be involved in what they are studying, and how much they affect what they gather as researchers (because of power differences or characteristics of themselves or those being studied), one traditional methodological standard remains: qualitative research requires long-term involvement that in turn allows access to the rich details and complexities of social life.

The presentation of data and analysis today is quite varied. The days when the shadowy researcher presented some "objective" data are gone. But the boundaries on how to present data today are rather blurred. Some would have us write poetry or perform (Richardson 1999), but most still focus on traditional sociological forms. The researcher today is usually present in the text, and the use of "I" dominates the field of ethnography. Sometimes, that "I" dominates the analysis too, but, most of the time, the work is not about the researcher but about the topic or group that is being studied. The "I" is important to permit the reader to know where the researcher was at the time the data were collected and to explain the role the researcher played. *Unlocking the Iron Cage* (1996), for example, is not about Michael Schwalbe, but about the men's movement. Schwalbe's position and what he said to the group, as Harrington points out, is more important to us for understanding the movement than for understanding Schwalbe.

The risks entailed in pushing ideas and methods forward are an important aspect of research and the production of knowledge. But getting to know one's case or cases (the place, group, organization, or people that one is studying), and knowing that case in considerable depth, is absolutely critical—and something that quantitative research can very rarely approximate. It is essential to demonstrate to readers that one knows one's case well, whether that case is a small group of schoolboys or a vast social movement. Qualitative researchers are able to see social settings in much of their richness—the details, the variations, the ambiguities, the contradictions, and the choices that people or groups make. Of course, useful concepts and theories are also important—useful in that they can extend, contradict, reaffirm, or fill in the work that has gone before. Important work is not strictly bound by the past, but without some links to the past, it is difficult to proceed to the future.

ACKNOWLEDGMENTS

The authors would like to thank Robert Zussman for encouraging this symposium and for his comments on this essay.

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