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## THE EVOLUTION OF ETHNOGRAPHIC RESEARCH METHODS: CURIOSITIES AND CONTRADICTIONS IN THE QUALITATIVE RESEARCH LITERATURE

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Mason, Jennifer. *Qualitative Researching*. 2nd ed. Thousand Oaks, CA: Sage. 2002. 224 pp.

Saldaña, Johnny. *Longitudinal Qualitative Research: Analyzing Change Through Time*. Walnut Creek, CA: AltaMira Press. 2003. 244 pp.

Beebe, James. *Rapid Assessment Process: An Introduction*. Walnut Creek, CA: AltaMira Press. 2001. 160 pp.

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de Munck, Victor C. and Elisa J. Sobo, eds. *Using Methods in the Field: A Practical Introduction and Casebook*. Walnut Creek, CA: AltaMira Press. 2001. 288 pp.

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*This article evaluates six recent contributions to the literature on ethnographic research methods. Four concern collecting and analyzing texts, one introduces the use of textual and numerical data, and one illustrates the use of quantitative methods for qualitative data. The first four books reveal that current thinking in qualitative methods continues to privilege inexplicit criteria for data collection and analysis despite recognizing that explicit criteria work best. The last two books show how to effectively integrate qualitative and quantitative data in ways that resolve these contradictions.*

Keywords: Ethnographic methods, qualitative research, quantitative research, data collection and analysis

This article evaluates six recent contributions to the literature on ethnographic research methods. The first four identify themselves as contributions to the literature on qualitative methods. The fifth illustrates the use of a wide variety of quantitative methods appropriate for qualitative research, and the sixth serves as a general introduction to a relatively comprehensive set of ethnographic methods. As a set, these books do two things. First, they introduce the state-of-the-art in qualitative methods. In the process, they illustrate key issues in the struggle to formulate a coherent empirical research method for ethnography. The most salient curiosities and contradictions appear in writings that argue that we should not grant epistemological privilege to specific people who use a narrow range of data and means of analysis that makes it impossible to convincingly rule out plausible alternative explanations, and those who make judgments based solely on inexplicit criteria even while they agree that the application of explicit criteria works best.

## **WHAT IS QUALITATIVE RESEARCH?**

In her Introduction to the second edition of *Qualitative Researching*, Jennifer Mason (2002) makes clear the ambiguities in defining qualitative research. People who do such research come from an extraordinary range of philosophical or theoretical traditions (phenomenology, positivism, semiotics, symbolic interactionism) and scholarly disciplines (anthropology, political science, education, sociology, psychology, nursing, public health), and they call what they do by many different names (grounded theory, ethnomethodology, ethnography, history, discourse analysis). As Mason points out, however, all share the following aim: to understand and (perhaps) explain how a specific set of people experience and interpret their social and cultural environments.

They also privilege the collection and analysis of data in the form of written texts (and, less often, visual media) from informal and semi-formal observations, informal and semistructured interviews, and accounts written by other observers. Whatever others may call their practices, anthropologists understand this aim and means as central to ethnography.

## HOW SHOULD ONE GO ABOUT IT?

Mason offers a reasonably comprehensive perspective. She organizes her text into three broad topics: strategy (Part I, chapters 1–2), data collection (Part II, chapters 3–7), and data analysis (Part III, chapters 8–9). A concluding appendix usefully summarizes by chapters the key questions she addresses in the text.

Mason uses the first two chapters to establish the framework for the rest of her book. Chapter 1 guides the reader through steps that lead to a clear, attainable research focus. Chapter 2 discusses basic issues of research strategy. Parts II and III elaborate these issues. Part II consists of five chapters that discuss sources of data, methods of data collection, interview procedures, observation and participation, visual media and documents, and sampling. Part II concludes with two chapters that deal with coding and building conceptual models, and putting it all together to form a convincing argument.

She organizes the book around an excellent set of questions. Some focus on the practical steps in collecting and analyzing data. What is the appropriate scope of my enquiries? How do I decide how deep I want to investigate a specific topic? How can I deal effectively with nonverbal information collected in the course of interviews? How do I negotiate access and my identity as I collect data? What kind of relationships should I anticipate forming with my research participants? What is the status of my field notes? When should I start coding my notes? When have I finished?

More specialized texts effectively extend the practical questions that researchers should consider when designing a new study. They also provide useful guidance for the actual implementation of research designs. For example, *Longitudinal Qualitative Research*, written by artist–educator Johnny Saldaña (2003), provides an excellent introduction to issues of time and both individual and cultural change in six chapters, an introduction, and a conclusion (plus an appendix that usefully summarizes the central themes). Saldaña asks us to bring into consciousness and think out implications of the time dimensions of research topics that we characteristically ignore. What, for example, constitute pertinent time periods for one or another

topic of enquiry? How do these units differ for different research participants, the investigator, or for research team members? He organizes his book around a useful set of questions that apply to macro-level phenomena as well as to the micro-level changes on which he focuses including what is different from one time period to another? What increases or emerges, and what is cumulative? What kinds of surges or epiphanies occur, and what decreases, ceases, or remains constant? What is idiosyncratic and missing? His second question is when do changes occur through time, which changes interrelate, and in what ways? Third, he asks what contextual and intervening conditions appear to precipitate these changes? Fourth, what are the dynamics of participant changes through time? Which oppose or harmonize with natural human development or constructed social processes and what are participant or conceptual rhythms (phases, stages, cycles, etc.)? Finally, how can one best characterize and explain participant change?

Saldaña illustrates in detail forms of data collection (do not let field notes accumulate, make them cumulative; tell tales of transformation), research design and data management (changing paradigms or methods in midstream while maintaining internal validity), and data analysis (let new codes emerge; collage or network, rather than rank, the interrelated factors that influence change) using text-analysis software.

Similarly, James Beebe's *Rapid Assessment Process* (2001) addresses the practical issues that arise in team-based research. Beebe directly addresses the topic that gives his book its title, but the questions he raises apply, as he notes, to all research carried out in teams, irrespective of project duration. *Rapid Assessment* consists of an introduction, eight chapters, and four appendices (including a glossary). The introduction and chapters 1 and 8 address issues specific to rapid assessment methods. Beebe fills chapters 2 through 7, however, with invaluable technical advice about planning, data collection, data analysis, and managing team-based research projects. He covers several topics. First, he discusses how to construct and administer semistructured interview protocols (your goal is to get people to tell you their stories, not answer your questions). Second, he describes team-based triangulation that takes advantage of perspective differences that reflect variation in disciplinary background (e.g., psychology, agricultural economics, anthropology) as well as group membership (insider–outsider). Third, he instructs on how to handle field notes and fieldwork logs to make iterative data collection and analysis more efficient. Finally, key team management considerations (e.g., daily meetings, leadership skills and responsibilities) are laid out.

As a set and individually, these books advocate systematic, rigorous research, based on active engagement with people who teach the investigator rather than who serve as research subjects. This approach produces explanations—rather than mere descriptions—that identify and argue for explicit empirical generalizations that are accountable to both one’s teachers and one’s readers with respect to their validity and reliability.

Toward this end, the issues most important for researchers to wrestle with consist of fundamental methods theory questions of the kind that Mason uses to organize *Qualitative Researching*. For example, in chapter 1, Mason asks readers to evaluate critically their own theoretical categories of experience, and to make explicit the question they wish to answer and the point of it all. In subsequent chapters, she asks readers to address questions like these: What makes something a piece of data? How can one differentiate self-imposed categories from those used by the people from whom one hopes to learn? What makes your analysis valid? Reliable? Convincing?

## **SORTING THROUGH MUDDLES IN OUR INTELLECTUAL HISTORY**

Paul Atkinson, Amanda Coffey, and Sara Delamont’s *Key Themes in Qualitative Research* (2003) provides an intellectual history of core questions like these. In seven chapters (plus an introduction and a conclusion), they use a series of classic works—for example, by Blanche Geer, Hortense Powdermaker, and Howard Becker—to make the point that we may go about research differently today than we did 20 years ago, and we may engage in endless bickering about one or another issue, but our core questions remain the same. For example, in sorting through one’s own theoretical categories of experience, one must think about the criteria by which one distinguishes the strange from the familiar. Since all speech reflects individually variable life experiences, as well as local and time-specific cultures, what warrant does one have, for example, to think that the meaning an informant attributes to a word or phrase matches one’s own? That you speak the same language? Similarly,

1. What advantages and disadvantages in research accrue to being an outsider or insider in producing valid, reliable findings? Indeed, what constitutes a valid finding? What is the relationship between validity and truth?

2. How do I negotiate access and my identity as I collect data? What level of intimacy is appropriate and what are the advantages and disadvantages of different levels and kinds of intimacy? To what extent should I identify with or advocate for my research participants? How does that change when my research participants are powerless and illiterate? When they are powerful and literate?
3. How do I handle my biases and political leanings? How can I know and assess the impact on my study of unspoken, unconscious agendas I might have? How can I know the equivalent agendas of my research sponsors, colleagues, or informants of different kinds and statuses? By listening too closely to my informants, do I end up giving weight to a tourist-oriented agenda and write a caricature of their lives?
4. To whom do I have what kind of responsibility, with what limits, and under what circumstances? To whom, for example, am I obliged for openness and integrity in my investigation? Does my Institutional Review Board protect human subjects, or (since the IRB's goal is to protect my organization from liability) is that responsibility mine?
5. How does interview content reflect interview context? Should I treat interview content solely as a respondent's experiences? Is my respondent telling me the truth? Or, is that the issue?
6. What weight do I ascribe to what people tell me? To what I see people do? How do I understand what I see people do if the only categories I have to make sense of observations are those which I have accumulated over my life? What is the evidentiary value of words and observations?

Atkinson et al. (2003) usefully sort through much of the muddle in the contemporary literature on qualitative methods. But they beg central questions regarding what may constitute the most important issue: how we might produce findings that are accountable to both one's teachers and one's readers with respect to their validity and reliability. For example, *Key Themes in Qualitative Research* provides a fanciful history of the introduction and use of the language of rigorous research which, with no evidentiary or logical warrant, asks us to believe that methods discourse created the classical criteria of validity and reliability without reference to the practice of research. They attribute special significance to ritualistic methods courses that bear little relationship to the knowledge applied to data collection and analysis. But they do not consider several interrelated possibilities: (1) that those courses were taught by ill-prepared or naive instructors;

(2) that the courses were taken by students without the background to appreciate sophisticated approaches to research; and (3) that more sophisticated understanding of the core issues would have immeasurably improved data collection, analysis, and the validity and reliability of reported findings.

Internal validity confounds saturate text data, for example. *Instrumentation*, for one, contributes to lack of comparability. As one gains experience in the field, one asks better and different questions. Field notes taken at the beginning of a study will not be comparable to those taken later. Similarly what informants report and what they do change in ways that reflect increasing knowledge of both. These changes constitute testing artifacts. Instrumentation and testing confounds may help a study by yielding better data as research progresses; but they also may not. In either event, their presence warns one not to treat as comparable field notes on a particular topic that come from different periods of research, even for short projects.

Other internal validity confounds complicate attempts to interpret data. Field notes contain random errors merely because one asked a specific informant a specific question at a specific time and place. People lie, sometimes reflexively rather than intentionally. People forget. People rationalize what they do. People do things for many reasons. People are not aware of all, or even most of the influences on what they do. People may be completely *unaware* of the most important influences on what they do, particularly when those influences are historical, macro-level phenomena, which cannot be perceived clearly in the minutiae of day-to-day living. What they make conscious varies from one time and place to another. People misjudge the relative importance of their reasons, intentions, or motivations. They change their evaluations. The constantly changing context of fieldwork and social interaction bring into play all of these possibilities. Thus they introduce random errors into one's measurements. Repeated questioning or observation introduces *regression*. Gathering information from other people, or the same people at different times (the next day) and places (next door), almost always yields different findings even when population characteristics do not change. We cannot tell from looking at differences or similarities whether they represent something real (people changed, people living in different neighborhoods think and act differently) or not (the differences are due solely to chance; people have not changed, and people living in different neighborhoods think and act pretty much the same). How reliable are one's data? Low levels of reliability make it hard to see nuanced differences and confound the problem of distinguishing structure from random error.

Increasing age and experience introduce *maturation* confounds. What an informant reports at the beginning of a study may change by the end because he or she has grown older and more experienced and has greater, or different, insight. Death or migration over the course of one's study introduces *mortality* confounds. Everything informants experience over the course of a study, as well as everything informants experienced earlier in their lives, introduces *history* confounds.

The socially constructed nature of cultural phenomena means that any one person who knows about a particular cultural phenomenon participates with other experts in its construction. In short, cultural phenomena inescapably embody *diffusion*—what statisticians call spatial and temporal autocorrelation. In seeking to understand people from the inside looking out, ethnographers aim to characterize accurately spatial and temporal autocorrelation, not correct for it. This poses a confound for explaining cultural variation, however, because intracultural or intercultural variation may merely reflect the structure and communication properties of social networks. The commonality in experience that leads to similarities and differences among informants may merely come from growing up in the same family, or living in the same village. This form of the diffusion confound complicates interpretation when you try to explain intracultural or intercultural variation by reference to variables like “economic opportunities” or “ethnic violence.” Its converse complicates interpretation when one documents and describes intracultural or intercultural variation and confuses similarities that reflect “economic opportunities” or “ethnic violence” for similarities owing to growing up in the same family or living in the same village.

Text data provide insight into the variables that compose cultures—how informants construct things like “families,” “gender,” and “age,” how they identify and discriminate one kind of thing from another, and how they identify, discriminate, and experience things like “power,” “competition,” and “cooperation.” But even highly detailed case-studies of individuals, families, or communities contain no comparisons and, hence, no evidence of relationships among variables or informants. Findings from hours and hours of an interview with a poor Native American woman do not warrant statements about identity formation at the intersection of ethnicity, class, and gender, for example. One must design comparisons into one's study to assess relationships. Selected case studies of people who are both poor and rich, Native American and not, and women and men, provide the necessary comparisons. But the expense of case studies compared to more focused data collection methods means that one cannot collect enough to warrant generalization.

By virtue of the interview and observation formats through which we collect text data, every informant contributes a piece of the cultural puzzle and none contributes to all of them. Informal and semistructured interviews, too, provide no basis for comparison among informants and give only the barest hints about what is common and what is not, what is unusual and what is not. They leave one guessing about what goes with what, about what is really significant, and they provide no warrant for generalization.

## **HUMAN BLUNDERS GENERATE CORE METHODS QUESTIONS**

Should research resolve into the practice of creative writing, or should it constitute an attempt to better understand the phenomenal world? The former goal makes empirical research irrelevant, although those who argue that a single phenomenal world does not exist to be understood do not grasp that any other assumption also leads inescapably to that conclusion. Atkinson et al. (2003), who write from the shared collective agreement among ethnographers that convincing findings come from effectively addressing fundamental issues like “Did I get it right?” and “To whom, if anyone, can I generalize?”, advocate that we search for appropriate criteria. By implication, they invoke the materialist assumption that a single phenomenal world exists, and the (commonplace) observation that some explanations work better than others. But Atkinson et al. provide no guidance for the search for appropriate criteria; indeed, by failing to make explicit and pursue the implications of the materialist assumption, they appear to rule out the possibility.

Mason, too (along with Saldaña and Beebe and, like Atkinson et al., albeit only by implication), invokes the materialist assumption that a single phenomenal world exists, and the (commonplace) observation that some explanations work better than others. *Qualitative Researching* (Mason 2002) devotes an entire chapter (chapter 9) to these issues. Mason makes several vital points. First, investigator judgments pervade research design, data collection, data analysis, and writeup. Second, no one—not the investigator, informants, or anyone else—should unequivocally claim epistemological privilege. Third, making investigator judgments explicit helps both the investigator and the reader find the flaws in a study. Finally, a convincing explanation must rest on a means for ruling out plausible alternatives. However, Mason never tells us why and, because the answers follow as logical implications, she does not tell us how. Moreover, neither she nor, for example, Atkinson et al., recognize that these

observations are more than 100 years old and come from a literature in the philosophy of science. How *does* one produce a convincing explanation, one with findings that are accountable to both one's teachers and one's readers with respect to their validity and reliability? Mason advises investigators to trace the route by which they arrive at interpretations, engage their own biases, and try one's best to read the data from alternative perspectives. This resolves into the weak "Trust me. I did the best I can."

Why should we concern ourselves with issues like investigator judgment, claims of epistemological privilege, study flaws, and means for ruling out plausible alternatives? Because we are not trustworthy. Human understanding of the phenomenal world can never escape personal subjectivity. This means that humans cannot avoid making blunders. The constructs used to formulate questions and behavioral observations contain errors, and the observations made of informant responses and behavior embody at least two sources of error. The first kind of error, measurement error, comes from the means by which one transforms sensory information into intelligible mental constructions (text or numbers or both). The second kind of error consists of either random (sampling) error or systematic (selection bias) error implicit in the subset of all possible times, places, and people, at which an investigator conducts interviews or makes observations. All research findings are subject to the qualification that "I did the best I can." But findings accountable to both one's teachers and one's readers with respect to their validity and reliability rest on explicit statements of the assumptions that guided the research and the criteria used to make judgments, which help everyone find a study's flaws.

### **IRRESPECTIVE OF GOALS OR MEANS, THESE STRICTURES APPLY TO ALL RESEARCH**

Construct error is built into the assumptions one must make to begin and carry out research, whether the research topic consists of elementary particles, cell membrane functions, or culture. One's assumptions contain biases that mirror everything a given person has experienced or not experienced over the life course. Hence, mental constructions consist, at least partly and perhaps largely, of *fantasy*, figments of one's imagination. To distinguish mental constructions that consist largely of fantasy from constructions that consist of less—as Donald Campbell put it, to distinguish between what's there and what you put there—one must assess the construct validity of one's data and findings with reference to whether or not, or the degree to which,

specific mental constructions correspond with specific observations and discriminate those observations from others.

We can do so by reference to inexplicit or explicit criteria. We can evaluate similarities between any two variables or informants, for example, by informal and inexplicit criteria that remain in our head—trust me, here is how I think they are similar or different. Or, we can answer the same question by explicit criteria like those embodied in Pearson’s correlation coefficient. We can evaluate similarities among sets of variables or sets of informants by informal and inexplicit criteria that remain in our head and ask our readers to believe that the mass comparison did not overwhelm the information processing limitations of short-term memory. Or, we can answer the same question by explicit criteria like those embodied in a principal components analysis of similarity coefficients; or, if we wish to use a different set of criteria, by those embodied in one or another form of cluster analysis, or in a multidimensional scaling (MDS) analysis. One way to rule out alternative explanations is to try one’s best to judge whether or not chance best explains an observation, and to try one’s best to read the data from different angles. Or, one can apply the explicit criteria embodied in statistical tests and multiple regression. The former apply explicit criteria to tell you how often one can expect a particular relationship just by chance. The latter applies explicit criteria to tell you that a relationship not only is unlikely due to chance, but that you can still see it after you isolate it from confounding effects.

## **HOW TO BRING RIGOR TO QUALITATIVE RESEARCH**

The muddles surrounding these issues in the literature on qualitative methods come partly from knowledge limitations. In an otherwise excellent text, for example, Mason hopelessly confuses research design with research strategy, and validity, reliability, precision, and accuracy. More importantly, perhaps, they come from (to invoke Richard Shweder’s wonderful expression) magical thinking. Writers on qualitative methods consistently, unnecessarily, and inappropriately impute negative meaning to constructs they associate (usually wrongly) with positivism or science or philosophy of science. Consequently, they never think through the basic issue: the blunders that human subjectivity necessarily makes a part of all research.

Qualitative methods specialists often use the word “reflexivity” to refer to the very old observation that the methods we use influence what we see. Curiously, they privilege text data over an integration

of text and numerical data. They also privilege judgments based solely on inexplicit criteria even while they agree that the application of explicit criteria works best. They thus rule out the possibility of making the structured observations and interviews amenable to comparative (numerical) analysis, which allow one to measure explicitly and control for internal validity confounds, see nuances otherwise missed, evaluate the role of chance, and provide an explicit warrant for generalization.

Victor C. DeMunck and Elisa J. Sobo's *Using Methods in the Field* (2001) illustrates and shows the utility of a host of new methods for the quantitative analysis of qualitative data. The assemblage of these methods for structured data collection and analysis originated in the early years of the American National Science Foundation sponsored Summer Institute on Research Methods in Cultural Anthropology (also known as *methods camp*). A new software package (Anthropac) made them readily accessible. The book consists of 14 chapters (plus a glossary and an appendix with an overly brief history of methods development in anthropology) which explain and illustrate content analysis, freelists, ratings, averages, frequencies, pile sorts, Guttman scaling, triads, decision modeling, consensus analysis, ethnographic sampling, MDS, and quadratic assignment procedures (QAP). Most of the chapters are authored by methods camp graduates.

*Using Methods in the Field* thus provides valuable explanatory material and usefully elaborates methods introduced in Bernard's *Research Methods in Anthropology* (2001). Bernard's textbook, comprising 18 chapters and five appendices, is now in its third edition. It remains by far the most comprehensive introduction to ethnographic research methods. This holds true even for courses that do not introduce forms of numerical analysis. Bernard devotes more than 360 pages to the design, collection, and analysis of text data, compared, for example, to the 200 pages of Mason's *Qualitative Researching*. Particularly useful topics that Bernard covers but Mason does not (or covers only briefly) include competing epistemologies, literature searches, choosing informants, and field note collection, coding, and management. Bernard also shows how experimental design principles may be integrated usefully into ethnographic research designs, discusses structured interviews and observations, extends the kinds of analysis appropriate to texts, and introduces, better than most books, statistics tests standard univariate (e.g., modes, medians, means, standard deviations, bar charts, pie charts, stem-and-leaf plots, box plots), bivariate (e.g., Pearson's correlation coefficient and other measures of similarity, scatterplots, bivariate regression), and multivariate statistical procedures (e.g., similarity matrices, multiple

regression, anova, factor analysis, cluster analysis, and multidimensional scaling).

## RECOMMENDATIONS FOR ETHNOGRAPHIC METHODS COURSES

Ethnographers who restrict themselves to the collection and analysis of text data may tell a story well, but they cannot undertake the comparisons and controls that allow explicit evaluation of construct validity, reliability, and thus the production of findings that convincingly rule out alternative explanations. The value of their stories necessarily remains primarily aesthetic. They remain, in fundamental ways, unaccountable to both one's teachers and one's readers with respect to their validity and reliability.

Instructors who accept the limitations of such research will find Mason's *Qualitative Researching* a good choice for a core text in a one semester course for undergraduates. Because it covers the same ground as Mason's book far more thoroughly, Bernard's *Research Methods in Anthropology* constitutes a better choice, particularly for graduate students. The addition of Johnny Saldaña's *Longitudinal Qualitative Research*, Raymond L. Gorden's *Basic Interviewing Skills* (1998), and Robert M. Emerson, Rachel I. Fretz, and Linda L. Shaw's *Writing Ethnographic Fieldnotes* (1995) would allow an instructor to introduce graduate students to the basics of ethnographic methods very well in a one semester course.

Bernard's *Research Methods in Anthropology* is also the text of choice for any methods course that wishes to incorporate the basics of explicit methods. De Munck and Sobo's *Using Methods in the Field* serves as an excellent supplementary source of illustration and elaboration.

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