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3 The Epistemology of Qualitative Research

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Qualitative and Quantitative

It is rhetorically unavoidable, discussing epistemological questions in social science, to compare qualitative and ethnographic methods with those which are quantitative and survey: to compare, imaginatively, a field study conducted in a community or organization with a survey of that same community or organization undertaken with questionnaires, self-administered or put to people by interviewers who see them once, armed with a printed form to be filled out. The very theme of this conference assumed such a division.

Supposing that the two ways of working are based on different epistemological foundations and justifications leads to asking the question posed to me by the conference's organizers: What's the epistemology of qualitative research? To me, it's an odd question. I'm an intellectual descendant of Robert E. Park, the founder of what has come to be called the Chicago school of sociology. Park was a great advocate of what we now call ethnographic methods. But he was equally a proponent of quantitative methods, particularly ecological ones. I follow him in that, and to me, the similarities between these methods are at least as, and probably more, important and relevant than the differences. In fact, I think that the same epistemological arguments underlie and provide the warrant for both.

How so? Both kinds of research try to see how society works, to describe social reality, to answer specific questions about specific instances of social reality. Some social scientists are interested in very general descriptions, in the form of laws about whole classes of phenomena. Others are more interested in understanding specific cases, how those general statements worked out in this case. But there's a lot of overlap.

The two styles of work do place differing emphasis on the understanding of specific historical or ethnographic cases as opposed to general laws of social interaction. But the two styles also imply one another. Every analysis of a case rests, explicitly or implicitly, on some general laws, and every general law supposes that the investigation of particular cases would show that law at work. Despite the differing emphases, it all ends up with the same sort of understanding, doesn't it?

That kind of ecumenicism clearly won't do, because the issue does not go away. To point to a familiar example, although educational researchers have done perfectly good research in the qualitative style for at least sixty years, they still hold periodic conferences and discussions, like this one, to discuss whether it's legitimate and, if it is, why it is. Surely there must be some real epistemological difference between the methods that accounts for this continuing inability to settle the question.

Some Thoughts About Epistemology

Let's first step back and ask about epistemology as a discipline. How does it see its job? What kinds of questions does it raise? Like many other philosophical disciplines, epistemology has characteristically concerned itself with "oughts" rather than "is's," and settled its questions by reasoning from first principles rather than by empirical investigation. Empirical disciplines, in contrast, have concerned themselves with how things work rather than what they ought to be, and settled their questions empirically.

Some topics of philosophical discussion have turned into areas of empirical inquiry. Scholars once studied biology and physics by reading Aristotle. Politics, another area philosophers once controlled, was likewise an inquiry in which scholars settled questions by reasoning rather than by investigation. We can see some areas of philosophy, among them epistemology, going through this transformation now, giving up preaching about how things should be done and settling for seeing how they are in fact done.

Aesthetics, for instance, has traditionally been the study of how to tell art from nonart and, especially, how to tell great art from ordinary art. Its thrust is negative, concerned primarily with catching undeserving candidates for the honorific title of art and keeping such pretenders out. The sociology of art, the empirical descendant of aesthetics, gives up trying to decide what should and shouldn't be allowed to be called art, and instead describes what gets done under that name. Part of its enterprise is exactly to see how that honorific title—art—is fought over, what actions it justifies, and what users of it can get away with (Becker 1982:131–64).

Epistemology has been a similarly negative discipline, mostly devoted to saying what you shouldn't do if you want your activity to merit the title of science, and to keeping unworthy pretenders from successfully appropriating it. The sociology of science, the empirical descendant of epistemology, gives up trying to decide what should and shouldn't count as science, and tells what people who claim to be doing science do, how the term is fought over, and what people who win the right to use it can get away with (Latour 1987).

So this chapter will not be another sermon on how we ought to do science, and what we shouldn't be doing, and what evils will befall us if we do the forbidden things. Rather, it will talk about how ethnographers have produced credible, believable results, especially those results which have continued to command respect and belief.

Such an enterprise is, to be philosophical, quite Aristotelian, in line with the program of the *Poetics,* which undertook not to legislate how a tragedy ought to be constructed but rather to see what was true of tragedies which successfully evoked pity and terror, producing catharsis. Epistemologists have often pretended to such Aristotelian analysis, but more typically deliver sermons.

Why Do We Think There's a Difference?

Two circumstances seem likely to produce the alleged differences between qualitative and quantitative epistemologists of social science make so much of. One is that the two sorts of methods typically raise somewhat different questions at the level of data, on the way to generalizations about social life. Survey researchers use a variant of the experimental paradigm, looking for numerical differences between two groups of people differing in interesting ways along some dimension of activity or background. They want to find that adolescents whose parents have jobs of a higher socioeconomic status are less likely to engage in delinquency, or more likely, or whatever—a difference from which they will then infer other differences in experience or possibilities that will "explain" the delinquency. The argument consists of an "explanation" of an act based on a logic of difference between groups with different traits (see Abbott 1992).

I don't mean to oversimplify what goes on in such work. The working out of the logic can be, and almost always is, much more complicated than this. Researchers may be concerned with interaction effects, and with the way some variables condition the relations between other variables, in all this striving for a complex picture of the circumstances attending someone's participation in delinquency.

Fieldworkers usually want something quite different: a description of the organization of delinquent activity, a description which makes sense of as much as possible of what they have seen as they observed delinquent youth. Who are the people involved in the act in question? What were their relations before, during, and after the event? What are their relations to the people they victimize? To the police? To the juvenile court? Fieldworkers are likewise interested in the histories of events: How did this start? Then what happened? And then? And how did all that eventually end up in a delinquent act or a delinquent career? And how did this sequence of events depend on the organization of all this other activity?

The argument rests on the interdependence of a lot of more or less proved statements. The point is not to prove, beyond doubt, the existence of particular relationships so much as to describe a system of relationships, to show how things hang together in a web of mutual influence or support or interdependence or what have you, to describe the connections between the specifics the ethnographer knows by virtue of having been there (Diesing 1971). Being there produces a strong belief that the varied events you have seen are all connected, which is not unreasonable since what the fieldworker sees is not variables or factors that need to be "related" but people doing things together in ways that are manifestly connected. After all, it's the same people and it's only our analysis that produces the abstract and discrete variables which then have to be put back together. So fieldwork makes you aware of the constructed character of "variables." (Which is not to say that we should never talk variable talk.)

A second difference which might account for the persistent feeling that the two methods differ epistemologically is that the situations of data gathering present fieldworkers, whether they seek it or not, with a lot of information, whether they want it or not. If you do a survey, you know in advance all the information you can acquire. There may be some surprises in the connections between the items you measure, but there will not be any surprise data, things you didn't ask about but were told anyway. A partial exception to this might be the use of open-ended questions, but even such questions are usually not asked in such a way as to encourage floods of unanticipated data suggesting new variables. In fact, the actual workings of survey organizations discourage interviewers from recording data not asked for on the forms (see Peneff 1988).

In contrast, fieldworkers cannot insulate themselves from data. As long as they are "in the field" they will see and hear things which ought to be entered into their field notes. If they are conscientious or experienced enough to know that they had better, they put it all in, even what they think may be useless, and keep on doing that until they know for sure that they will never use data on certain subjects. They thus allow themselves to become aware of things they had not anticipated which may have a bearing on their subject. They expect to continually add variables and ideas to their models. In some ways, that is the essence of the method.

Many Ethnographies

The variety of things called ethnographic aren't all alike, and in fact may be at odds with each other over epistemological details. In what follows, I will concentrate on the older traditions (for example, participant observation, broadly construed, and unstructured interviewing) rather than the newer, more trendy versions (for example, hermeneutic readings of texts), even though the newer versions are more insistent on the epistemological differences. What I have to say may well be read by some as less than the full defense of what they do that they would make. So be it. I'll leave it to less middle-of-the-road types to say more. (I will, however, talk about ethnographers or fieldworkers somewhat indiscriminately, lumping together people who might prefer to be kept separate.)

A lot of energy is wasted hashing over philosophical details, which often have little or nothing to do with what researchers actually do, so I'll concentrate less on theoretical statements and more on the way researchers work these positions out in practice. What researchers do usually reflects some accommodation to the realities of social life, which affect them as much as any other actor social scientists study, by constraining what they can do. Their activity thus cannot be accounted for or explained fully by referring to philosophical positions (see Platt). In short, I'm describing practical epistemology, how what we do affects the credibility of the propositions we advance. In general, I think that the arguments advanced by qualitative researchers have a good deal of validity, but not in the dogmatic and general way they are often proposed. So I may pause here and there for a few snotty remarks on the excesses ethnographers sometimes fall into.

A few basic questions seem to lie at the heart of the debates about these methods. First, must we take account of the viewpoint of the social actor and, if we must, how do we do it? And how do we deal with the embeddedness of all social action in the world of everyday life? And how thick can we and should we make our descriptions?

The Actor's Point of View: Accuracy

One major point most ethnographers tout as a major epistemological advantage of what they do is that it lets them grasp the point of view of the actor. This satisfies what they regard as a crucial criterion of adequate social science. "Taking the point of view of the other" is a wonderful example of the variety of meanings methodological slogans acquire. For some, it has a kind of religious or ethical significance: if we fail to do that we show disrespect for the people we study. Another tendency goes further, finding fault with social science which "speaks for" others, by giving summaries and interpretations of their point of view. In this view, it is not enough to honor, respect, and allow for the actors' points of view. One must also allow them to express it themselves.

For others, me among them, this is a technical point best analyzed by Herbert Blumer: all social scientists, implicitly or explicitly, attribute a point of view and interpretations to the people whose actions we analyze (Blumer 1969). That is, we *always* describe how they interpret the events they participate in, so the only question is not whether we should, but how accurately we do it. We can find out, not with perfect accuracy, but better than zero, what people think they are doing, what meanings they give to the objects and events and people in their lives and experience. We do that by talking to them, in formal or informal interviews, in quick exchanges while we participate in and observe their ordinary activities, and by watching and listening as they go about their business; we can even do it by giving them questionnaires which let them say what their meanings are or choose between meanings we give them as possibilities. To anticipate a later point, the nearer we get to the conditions in which they actually do attribute meanings to objects and events, the more accurate our descriptions of those meanings are likely to be.

Blumer argued that if we don't find out from people what meanings they are actually giving to things, we will still talk about those meanings. In that case, we will, of necessity, invent them, reasoning that the people we are writing about must have meant this or that, or they would not have done the things they did. But it is inevitably epistemologically dangerous to guess at what could be observed directly. The danger is that we will guess wrong, that what looks reasonable to us will not be what looked reasonable to them. This happens all the time, largely because we are not those people and do not live in their circumstances. We are thus likely to take the easy way and attribute to them what we think we would feel in what we understand to be their circumstances, as when students of teenage behavior look at comparative rates of pregnancy, and the correlates thereof, and decide what the people involved "must have been" thinking in order to behave that way.

The field of drug use, which overlaps the study of adolescence, is rife with such errors of attribution. The most common meaning attributed to drug use is that it is an "escape" from some sort of reality the drug user is said to find oppressive or unbearable. Drug intoxication is conceived as an experience in which all painful and unwanted aspects of reality recede into the background so that they need not be dealt with. The drug user replaces reality with gaudy dreams of splendor and ease, unproblematic pleasures, perverse erotic thrills and fantasies. Reality, of course, is understood to be lurking in the background, ready to kick the user in the ass the second he or she comes down.

This kind of imagery has a long literary history, probably stemming from De Quincey's 1856 *Confessions of an English Opium Eater* (a wonderful nineteenth-century American version is Fitz Hugh Ludlow's 1857 *The Hashish Eater*). These works play on the imagery analyzed in Edward Said's dissection of *orientalia,* the Orient as mysterious other (Said 1978). More up-to-date versions, more science-fictiony, less oriental, and less benign, can be found in such works as William Burroughs's *Naked Lunch* (1966).

Such descriptions of drug use are, as could be and has been found out by generations of researchers who bothered to ask, pure fantasy on the part of the researchers who publish them. The fantasies do not correspond to the experiences of users or of those researchers who have made the experiments themselves. They are concocted out of a kind of willful ignorance.

Misinterpretations of people's experience and meanings are commonplace in studies of delinquency and crime, of sexual behavior, and in general in studies of behavior foreign to the experience and lifestyle of conventional academic researchers. Much of what anthropological and ethnographic studies have brought to the understanding of the problems of adolescence and growing up is the correction of such simple errors of fact, replacing speculation with observation.

But "don't make up what you could find out" hardly requires being dignified as an epistemological or philosophical position. It is really not much different from a more conventional, even positivist, understanding of method (see Lieberson 1992), except in being even more rigorous, requiring the verification of speculations that researchers will not refrain from making. So the first point is that ethnography's epistemology, in its insistence on investigating the viewpoint of those studied, is indeed like that of other social scientists, just more rigorous and complete. (I find it difficult, and don't try very hard, to avoid the irony of insisting that qualitative research is typically more precise and rigorous than survey research, ordinarily thought to have the edge with respect to those criteria.)

One reason many researchers who would agree with this in principle nevertheless avoid investigating actors' viewpoints is that the people we study often do not give stable or consistent meanings to things, people, and events. They change their minds frequently. Worse yet, they are often not sure what things *do* mean; they make vague and woolly interpretations of events and people. It follows from the previous argument that we ought to respect that confusion and inability to be decisive by not giving things a more stable meaning than the people involved do. But that makes the researcher's work more difficult, since it is hard to describe, let alone measure, such a moving target.

An excellent example of the instability of "native" meanings is given in Bruno Latour's (1987) analysis of science. Conventionally, social scientists accord a special status to the knowledge created by scientists, treating it as better than conventional lay knowledge, as being more warranted. Latour notes this paradox: scientists themselves don't always regard science that way. Sometimes they do, treating a result as definitive and "black-boxing" it. But scientists often argue with each other, trying to keep others from putting a result in a black box or, worse yet, opening black boxes everyone thought were shut for good. His rule of method is, we should be as undecided as the actors we study. If they think a conclusion, a finding, or a theory is shaky, controversial, or open to question, then we should, too. And we should do that even if what we are studying is a historical controversy whose outcome we now know, even though the actors involved at the time couldn't. Conversely, if the actors involved think the piece of science involved is beyond question, so should we.

People who write about science prescriptively—epistemologists—could avoid misconstruing the ideas of those they study if they followed the simple rules anthropologists have invented for themselves about fieldwork. It was once thought good enough to visit your tribe for a month or two in the summer and to get all your information from informants interviewed with the help of translators. No one thinks that any more, and now there is a minimum standard—know the native language, stay a year to eighteen months, use some sort of rudimentary sampling techniques. Applied to the study of science, these rules would require that epistemologists learn the native language fully, not just the high church version trotted out on formal occasions but the language of daily work as well, not just the views of "eminent scientists" and those who speak for the science, but of the ordinary scientists who actually do the work. Which is what Latour and the other students of "shop-floor practice" in science have done (and what Diesing [1971], an unusual epistemologist, did), and many other sociologists of science did not.

Epistemologically, then, qualitative methods insist that we should not invent the viewpoint of the actor, and should only attribute to actors ideas about the world they actually hold, if we want to understand their actions, reasons, and motives.

The Everyday World: Making Room for the Unanticipated

A second point, similar to the emphasis on learning and understanding the meanings people give to their world and experiences instead of making them up, is an emphasis on the everyday world, everyday life, the *quotidian.* This catch phrase appears frequently in ethnographic writing, often referring to the ideas of Alfred Schutz. In Schutz's writings (see Schutz 1962), and in the elaborations of those ideas common among ethnomethodologists, the everyday world typically refers to the taken-for-granted understandings people share which make concerted action possible. In this, the idea resembles the notion of culture one finds in Redfield—"shared understandings made manifest in act and artifact" (1941:132)—and the similar emphasis on shared meanings in Meadian (George Herbert Mead, that is) thought as interpreted by Blumer.

The general idea is that we act in the world on the basis of assumptions we never inspect but just act on, secure in the belief that when we do, others will react as we expect them to. A version of this is the assumption that things look to me as they would look to you if you were standing where I am standing. In this view, "everyday understandings" refers not so much to the understandings involved, say, in the analysis of a kinship system—that this is the way one must behave to one's mother's brother's daughter, for instance—but to the deep epistemological beliefs that undergird all such shared ideas, the meta-analyses and ontologies we are not ordinarily aware of that make social life possible.

Much theoretical effort has been expended on this concept. I favor a simpler, less controversial, more workaday interpretation, either as an alternative or simply as a complement to these deep theoretical meanings. This is the notion of the everyday world as the world people actually act in every day, the ordinary world in which the things we are interested in understanding actually go on. As opposed to what? As opposed to the simpler, less expensive, less time-consuming world the social scientist constructs in order to gather data efficiently, in which survey questionnaires are filled out and official documents consulted as proxies for observation of the activities and events those documents refer to.

Most ethnographers think they are getting closer to the real thing than that, by virtue of observing behavior *in situ* or at least letting people tell about what happened to them in their own words. Clearly, whenever a social scientist is present, the situation is not just what it would have been without the social scientist. I suppose this applies even when no one knows that the social scientist is a social scientist doing a study. Another member of a cult who believes flying saucers from other planets are about to land is, after all, one more member the cult would not have had otherwise and, if the cult is small, that increase in numbers might affect what the observer is there to study.

But given that the situation is never exactly what it would have been otherwise, there are degrees of interference and influence. Ethnographers pride themselves on seeing and hearing, more or less, what people would have done and said had the observers not been there. One reason for supposing this to be true is that ethnographers observe people when all the constraints of their ordinary social situation are operative. Consider this comparatively. We typically assure people to whom we give a questionnaire or who we interview that no one will ever know what they have said to us, or which alternatives on the questionnaire they have chosen. (If we can't make that assurance, we usually worry about the validity of the results.) This insulates the people interviewed from the consequences they would suffer if others knew their opinions. The insulation helps us discover people's private thoughts, the things they keep from their fellows, which is often what we want to know.

But we should not jump from the expression of a private thought to the conclusion that that thought determines the person's actions in the situation to which it might be relevant. When we watch someone as they work in their usual work setting or go to a political meeting in their neighborhood or have dinner with their family—when we watch people do things in the places they usually do them with the people they usually do them with—we cannot insulate them from the consequences of their actions. On the contrary, they have to take the rap for what they do, just as they ordinarily do in everyday life. An example: when I was observing college undergraduates, I sometimes went to classes with them. On one occasion, an instructor announced a surprise quiz for which the student I was accompanying that day, a goof-off, was totally unprepared. Sitting nearby, I could easily see him leaning over and copying answers from someone he hoped knew more than he did. He was embarrassed by my seeing him, but the embarrassment didn't stop him copying, because the consequences of failing the test (this was at a time when flunking out of school could lead to being drafted, and maybe being killed in combat) were a lot worse than my potentially lowered opinion of him. He apologized and made excuses later, but he did it. What would he have said about cheating on a questionnaire or in an interview, out of the actual situation that had forced him to that expedient?

Our opinions or actions are not always regarded as inconsequential by people we study. Social scientists who study schools and social agencies regularly find that the personnel of those organizations think of research as some version of the institutional evaluations they are constantly subject

to, and take measures to manipulate what will be discovered. Sometimes the people we find it easiest to interview are on the outs with their local society or culture, hoping to escape and looking to the ethnographer for help. But although these exceptions to the general point always need to be evaluated carefully, ethnographers typically make this a major epistemological point: when they talk about what people do they are talking about what they saw them do under the conditions in which they usually do it, rather than making inferences from a more remote indicator such as the answer to a question given in the privacy of a conversation with a stranger. They are seeing the "real world" of everyday life, not some version of it created at their urging and for their benefit, and this version, they think, deserves to be treated as having greater truth value than the potentially less accurate versions produced by other methods, whatever the offsetting advantages of efficiency and decreased expense.

A consequence of finding out about the details of everyday life is that many events and actions turn out to have mundane explanations seldom accounted for in our theories. A student in a fieldwork class I taught in Kansas City studied letter carriers. Under my prodding, he tried to find out what sorts of routes the carriers preferred, which parts of town they chose to work in when they had a chance to make a choice. Having done his research, he invited his fellow students to guess the answer and, budding social scientists that they were, their guesses centered on social class: the carriers would prefer middle-class areas because they were safer; the carriers would prefer working-class areas because the inhabitants would be on fewer mailing lists and thus there would be less mail to carry; and so on. All these clever, reasonable guesses were wrong. What the carriers he talked to preferred (and this is not to say that other carriers elsewhere might not have different preferences and reasons for them) were neighborhoods that were flat. Kansas City is hilly and the carriers preferred not to climb up and down as they moved from street to street. This is not an explanation that would make sense from a "stratification" point of view; a follower of Bourdieu, for instance, might not think to include such an item in a survey. But that was the reason the carriers gave, a homely reason waiting to be discovered by someone who left room for it to come out.

Full Description, Thick Description: Watching the Margins

Ethnographers pride themselves on providing dense, detailed descriptions of social life, the kind Geertz (1974) has taught us to recognize as "thick." Their pride often implies that the fuller the description, the better, with no limit suggested. At an extreme, ethnographers talking of reproducing the "lived experience" of others.

There is something wrong with this on the face of it. The object of any description is not to reproduce the object completely—why bother when we have the object already?—but rather to pick out its relevant aspects, details which can be abstracted from the totality of details that make it up so that we can answer some questions we have. Social scientists, for instance, usually concentrate on what can be described in words and numbers, and thus leave out all those aspects of reality that use other senses, what can be seen and heard and smelled. (How many monographs deal with the smell of what is being studied, even when that is a necessary and interesting component—and when isn't it? [cf. Becker 1986:121–35].)

Ethnographers usually hail "advances" in method which allow the inclusion of greater amounts of detail: photographs, audio recording, video recording. These advances never move us very far toward the goal of full description; the full reality is still a long way away. Even when we set up a video camera, it sits in one place at a time, and some things cannot be seen from that vantage point; adding more cameras does not alter the argument. Even such a small technical matter as the focal length of the camera's lens makes a big difference: a long lens provides close-up detail, but loses the context a wide-angle lens provides.

So full description is a will-o'-the-wisp. But, that said, a fuller description is preferable to, and epistemologically more satisfying than, a skimpy description. Why? Because, as with the argument about the actor's point of view, it lets us talk with more assurance about things than if we have to make them up—and, to repeat, few social scientists are sufficiently disciplined to refrain from inventing interpretations and details they have not, in one way or another, observed themselves. Take a simple example. We want to know if parents' occupations affect the job choices adolescents make. We can ask them to write down the parents' occupations on a line in a questionnaire; we can copy what the parents have written down somewhere, perhaps on a school record; or we can go to where the parents work and verify by our own observation that this one teaches school, that one drives a bus, the other one writes copy in an advertising agency.

Is one of these better than another? Having the children write it down on a form is better because it is cheap and efficient. Copying it from a record the parents made might be better because the parents have better knowledge of what they do and better language with which to express it than the children do. Seeing for ourselves would still be open to question—maybe they are just working there this week—but it leaves less room for slippage. We don't have to worry about the child's ignorance or the parents' desire to inflate their status. Epistemologically, I think, the observation which requires less inference and fewer assumptions is more likely to be accurate, although the accuracy so produced might not be worth bothering with.

A better goal than "thickness"—one fieldworkers usually aim for—is "breadth": trying to find out something about every topic the research touches on, even tangentially. We want to know something about the neighborhood the juveniles we study live in, and the schools they go to, and the police stations and jails they spend time in, and dozens of other things. Fieldworkers pick up a lot of incidental information on such matters in the course of their participation or lengthy interviewing but, like quantitative researchers, they often use "available data" to get some idea about them. They usually do that, however, with more than the usual skepticism.

It is time to mention, briefly, the well-known issue of "official statistics" or, put more generally, the necessity of looking into such questions as why records are kept, who keeps them, and how those facts affect what's in them. (None of this is news to historians, who would think of this simply as a matter of seeing what criticisms the sources they use have to be subjected to.) As Bittner and Garfinkel (1967) told us years ago, organizations don't keep records so that social scientists can have data but, rather, for their own purposes. This is obvious in the case of adolescents, where we know that school attendance records are "managed" in order to maximize state payments; behavioral records slanted to justify actions taken toward "difficult" kids; and test scores manipulated to justify tracking and sorting. Similarly, police records are kept for police purposes, not for researchers' hypothesis testing.

Ethnographers therefore typically treat data gathered by officials and others as data about what those people did: police statistics as data about how police keep records and what they do with them, data about school testing as data about what schools and testers do rather than about student traits, and so on. That means that ethnographers are typically very irreverent, and this makes trouble.

It makes trouble where other people don't share the irreverence but take the institution seriously on its own terms. Qualitative researchers are often, though not necessarily, in a kind of antagonistic relationship to sources of official data, who don't like to be treated as objects of study but want to be believed (I have discussed this elsewhere under the heading of the "hierarchy of credibility" [Becker 1967]).

Coda

There's not much more to say. Practitioners of qualitative and quantitative methods may seem to have different philosophies of science, but they really just work in different situations and ask different questions. The politics of social science can seduce us into magnifying the differences. But it needn't, and shouldn't.

Further Thoughts

After the foregoing had been discussed at the conference, some people felt that there were still unresolved questions that I ought to have dealt with. The questions were ones that are often raised and my answers to them are not really "answers," but rather responses which discuss the social settings in which such questions are asked rather more than the questioners may have anticipated.

One question had to do with how one might combine what are sometimes called the two modalities, the qualitative and quantitative approaches to social research. There is a little literature on this question, which generally ends up suggesting a division of labor, in which qualitative research generates hypotheses and quantitative research tests them. This question is invariably raised, and this solution proposed, by quantitative researchers, who seem to find it an immense problem, and never by qualitative researchers, who often just go ahead and do it, not seeing any great problem, in that following the lead of Robert E. Park, as I suggested above.

Well, why don't qualitative researchers think it's a problem? They don't think it's a problem because they focus on questions to be answered rather than on procedures to be followed. The logic of this is laid out in enormous detail in a book that is not about sociology at all, George Polya's *Mathematics and Plausible Reasoning* (1954), in which he shows how one combines information of all kinds in assessing the reasonableness of a conclusion or idea.

And how do researchers actually go about combining these different kinds of data? This is not an easy matter to summarize briefly, because qualitative researchers have been doing this for a very long time, and there are many examples of it being done in many parts of the literature. Thomas Kuhn (1970) noted that scientists learn their trade not by following abstract procedural recipes but rather by examining exemplars of work in their field commonly regarded as well done. The best way to see how data of these various kinds can be combined is to examine how they were combined in exemplary works. This was obviously too large a task for the conference paper.

But I will cite three well-known works and suggest that analysis of the methods used in them and in other such works be undertaken by those who want to see the answer to the question. Horace Cayton and St. Clair Drake's *Black Metropolis* (1945) is a monumental study of the black areas of the south side of Chicago in the late thirties. It contains data of every kind imaginable, some statistical, some observational, all pointed toward answering questions about the organization of that community. *Boys in White* (1961), the study of medical students several of us conducted in the 1950s, relied on observation and unstructured interviews to generate data, but presented the results both in an ethnographic form and in simple tables which were, somewhat to the surprise of qualitative zealots, "quantitative," though we did not use any tests of significance, the differences we pointed to being gross enough to make such tests an unnecessary frill. Jane Mercer's *Labeling the Mentally Retarded* (1973) is the nearest of these three to the standard combination often recommended; she used community surveys, official records of several kinds, as well as unstructured interviews, to arrive at her conclusions about the social character of mental retardation.

A second question dealt with validity, noting that my paper did not speak to that question, but instead talked (following the lead of Polya, already referred to) about credibility. Do I really think that that's all there is to it, simply making a believable case? Isn't there something else involved, namely, the degree to which one has measured or observed the phenomenon one claims to be dealing with, as opposed to whether two observers would reach the same result, which was one of the ways some people interpreted my analysis of credibility?

We come here to a difference that is really a matter not of logic or scientific practice but of professional organization, community, and culture. The professional community in which quantitative work is done (and I believe this is more true in psychology than in sociology) insists on asking questions about reliability and validity, and makes acceptable answers to those questions the touchstone of good work. But there are other professional communities for whose workers those are not the major questions. Qualitative researchers, especially in sociology and anthropology, are more likely to be concerned with the kinds of questions I raised in the body of my paper: whether data are accurate, in the sense of being based on close observation of what is being talked about or only on remote indicators; whether data are precise, in the sense of being close to the thing discussed and thus being ready to take account of matters not anticipated in the original formulation of the problem; whether an analysis is full or broad, in the sense of being based on knowledge about a wide range of matters that impinge on the question under study, rather than just a relatively few variables. The paper contains a number of relevant examples of these criteria.

Ordinarily, scholarly communities do not wander into each other's territory, and so do not have to answer to each other's criteria. Operating within the paradigm accepted in their community, social scientists do what their colleagues find acceptable, knowing that they will have to answer to their community for failures to adhere to those standards. When, however, two (at least two, maybe more) scholarly communities meet, as they did in this conference, the question arises as to whose language the discussions will be conducted in, and what standards will be invoked. It is my observation over the years that quantitative researchers always want to know what answers qualitative researchers have to *their* questions about validity and reliability and hypothesis testing. They do not discuss how they might answer the questions qualitative researchers raise about accuracy and precision and breadth. In other words, they want to assimilate what others do to their way of doing business and make those other ways answer their questions. They want the discussion to go on in their language and the standards of qualitative work translated into the language they already use.

That desire—can I say "insistence"?—presumes a status differential: *A* can call *B* to account for not answering *A*'s questions properly, but *B* has no such obligation to *A*. But this is a statement about social organization, not about epistemology, about power in hierarchical systems, not about logic. When, however, scholarly communities operate independently, instead of being arranged in a hierarchy of power and obligation, as is presently the case with respect to differing breeds of social science, their members need not use the language of other groups; they use their own language. The relations between the groups are lateral, not vertical, to use a spatial metaphor. One community is not in a position to require that the other use its language.

That has to some extent happened in the social sciences, as the growth of social science (note that this argument has a demographic base) made it possible for subgroups to constitute worlds of their own, with their own journals, organizations, presidents, prizes, and all the other paraphernalia of a scientific discipline.

Does that mean that I'm reducing science to matters of demographic and political weight? No, it means recognizing that this is one more version of a standard problem in relations between culturally differing groups. To make that explicit, the analogies to problems of translation between languages and cultures (neatly analyzed, for instance, in Talal Asad's essay, "The Concept of Cultural Translation in British Social Anthropology" [1986]) are close. Superordinate groups in situations of cultural contact (for example, colonial situations) usually think everything should be translated so that it makes sense in *their* language rather than being translated so that the full cultural difference in the concepts in question is retained. They are very often powerful enough, at least for a while, to require that that be done.

This problem of translation between culturally differing groups is what Kuhn called attention to in noting that when there is a substantial paradigm difference, as in the case of a paradigm shift, the languages in which scientific work is conducted cannot be translated into one another. If the groups are in fact independent, then there is a translation problem and the same dynamic—the question, you might say, of whose categories will be respected—comes into play.

So what seem like quite reasonable requests for a little clarification are the playing out of a familiar ritual, which occurs whenever quantitative workers in education, psychology, and sociology decide that they will have to pay attention to work of other kinds and then try to coopt that work by making it answer to their criteria, criteria like reliability and validity, rather than to the criteria I proposed, commonly used by qualitative workers. I would say that I wasn't *not dealing* with validity, but *was*, rather, *dealing* with something else that seems as fundamental to me as validity does to others.

This will all sound at odds with my fundamental belief, expressed in the paper, that the two styles of work actually share the same, or a very similar, epistemology. I do believe that's true. But I also think that some workers get fixated on specific procedures (not the same thing as epistemology), act as I have described with respect to those procedures, and have this same feeling that other styles of work must be justified by reference to how well they accomplish what those procedures are supposed to accomplish.

Finally, some people asked how one could tell good from bad or better from worse in qualitative work. I've suggested one answer in the criteria already discussed. Work that is based on careful, close-up observation of a wide variety of matters that bear on the question under investigation is better than work which relies on inference and more remote kinds of observations. That's a criterion. One reason *Street Corner Society* (Whyte 1981) is widely recognized as a masterwork of social science research is that it satisfies this criterion; William Foote Whyte knew what he was talking about, he had observed the social organization he analyzed in minute detail over a long time, and had looked not only at the interactions of a few "corner boys" but also at the operation of much larger organizations in politics and crime, which impinged on the corner boys' lives.

But something else needs to be said. Many people who are quick to recognize the quality of Whyte's work or of Erving Goffman's studies of social organization are just as quick to say that this kind of thing can only be done by specially gifted people, that only *they* can get these remarkable results and, thus, that the methods they have used are not suitable for the development of a science. This recognizes what must be recognized—quality that everyone knows is there—while marginalizing the enterprise that made that quality possible. Goffman was indeed a gifted social scientist, but his gifts expressed themselves within a tradition of thinking and fieldwork that extended from Durkheim through Radcliffe-Brown to Lloyd Warner, as well as from Simmel to Park to Hughes and Blumer. The tradition made his work possible.

That is, however, true of good work in every branch of social science, qualitative or quantitative. Stanley Lieberson, for instance, is a gifted quantitative researcher, but what makes his work outstanding is not that he uses some particular method or that he follows approved procedures correctly, but that he has imagination and can smell a good problem and find a good way to study it. Which is to say that telling good from bad is not as simple as it appears. It's easy enough to tell work that's done badly, and to tell how it was done badly, and where it went off the track. But that in no way means that it is possible, in any version of social science, to write down the recipe for doing work of the highest quality, work that goes beyond mere craft. That's another story. Physicists, who so many social scientists think to imitate, know that. How come we don't?

So these are matters that are deeper than they seem to be, in a variety of ways, and mostly, I think, in organizational ways. I haven't, for reasons I hope to have made clear, answered these questions as the people who asked them hoped. I've explained things in my terms, and I guess they will have to do the translating.

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