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Confessions of a Mad Ethnographer

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John Van Maanen once told me that when ethnographers get too old to do fieldwork, they start writing about it. John is always good with the quip. On the day he quipped this, I swore I would resist pontificating about fieldwork (at least in print) for as long as possible to sustain my Peter Pan fantasy. In fact, I've made a habit of looking for exceptions to Van Maanen's rule. There is Gary Allen Fine (1996, 1998, 2001, 2004, 2007), just a couple of years older than me, who produces an ethnography every two or three years (and if you count his papers, more often than that). Howie Becker, the eminent statesman of fieldwork, recently wrote a book on playing jazz with Robert Faulkner, himself an ethnographer and professor emeritus (2009). Then, there is the ever pragmatic Aaron Cicourel, who in his late 70s told me he was too old to study young people, so he began fieldwork on Alzheimer's patients and their spouses (2013). But the truth is, exceptions to John's observation are depressingly few. Ethnography may, in fact, be a young person's game. If so, for God's sake don't waste your youth.

Although I have had many invitations to do so, until now I have refused to write about doing ethnography with one exception (Barley 1990a). The exception occurred because the editors of a special issue of *Organization Science* did not want the theory paper that Pam Tolbert and I originally wrote for the conference that fed the special issue. Instead, they leaned on me to write a methods paper about longitudinal field methods. I succumbed by riffing on the methods section of my dissertation. In the end, poetic justice prevailed. The theory paper, which Pam and I eventually published several years later, drew a vastly larger audience (Barley and Tolbert 1997). (To the surprise of both of us, I might add.) All of this is to say that writing the present chapter nearly amounts to Peter Pan admitting that his wings have been clipped. Time will tell. But, as Martin Ruef noted in his recent address to the American Sociological Society, time is getting short; like it or not, I am uncomfortably near the point where the Feds will let me start drawing social security.[1]

Despite the fact that I am now capitulating on my resolution at Elsbach's request (who can resist Kim?), what I am not going to do is tell you how to design an ethnography, gain entry into the field, manage your informants, do an interview, analyze fieldnotes, compare cases, or write a methods section. I am not going to explore the mysteries of coding or the pros and cons of working alone versus in teams. There are plenty of books and papers that address these topics, including some in this volume. I have nothing of value to add to the discourse. Even more certainly, I will not flirt with reflexivity in fieldwork. But I am going to tell what John would call a confessional tale of sorts, although I like an aging ethnographer's autoethnographic rant better. The rant will focus on where ethnography (if we want to call it that) in organization studies seems to have arrived over the last three decades and why I think more of us should take a different path. Along the way, I'll note places where I may bear as much or more blame than most for the current state of affairs. Much of what I will say draws on my accumulated experience with reviewers, comments that others have made about their experiences publishing fieldwork, and my own reading of qualitative papers published in the field of organization and management studies. At no point will I back up my critiques with citations or illustrations of offenses. There is no honor in pointing fingers, and there is even less honor in being pointed to. If my approach makes it easier to dismiss my grumblings, so be it.

From Fighting Outsiders to Internecine Skirmishing

Thirty-six years ago when I decided to become an ethnographer, the guardians of epistemological dogma were boogeymen known as "the positivists."[2] Exactly who or what this tribe was remained vague. I am certain the epithet did not refer simply to members of the Vienna Circle and their acolytes, because I don't think most qualitative researchers who invoked the term had ever read Rudolf Carnap and Otto Neurath or even recognized that Karl Popper (1934) was not exactly a positivist.[3] What was not lost on us neophytes in the qualitative camp was that to be called a *positivist* was the academic equivalent of playing "Yo Mama" (for explication see, Abrahams 1962; Lefever 1981). "God help us be anything but a positivist," many of us prayed. In everyday talk, we used the term to slur those who believed in deductive analysis, the testing of hypotheses, and the law of large numbers, unless one could pull off a controlled experiment. According to such individuals—who terrorized us most from the shadows in the form of reviewers—verification, replicability, validity, and, above all, generalizability were commandments that ethnographers willingly broke. So judged, we deserved to be summarily cast from the garden of science.

I vividly remember an eminent organizational psychologist asking me at my first doctoral consortium at the Academy of Management, "What's the difference between you and a journalist?" Flustered, more than a little defensive, and unable to see why good investigative journalism was a problem, I responded lamely, "What's the difference between you and a rock?" It was not my finest moment. Personally, though, as long as positivists behaved civilly, I held little animosity toward them. After all, they were empiricists and at MIT, if you learned nothing else, you learned to worship at the altar of empiricism. Then, as now, I think of good ethnographers as empiricists, too. The difference is that ethnographers like to get up close to the people and settings we study and document phenomena *in situ*, not unlike the naturalists who pioneered the fields of physics, geology, astronomy, and biology centuries earlier. Unfortunately, unlike the naturalists' phenomena, which change slowly, if at all, the dynamics of social life rebel against standing still. When push comes to shove, I came to believe that what the positivists were really asking was for us to provide proof that justified our claims (see Becker 1958 for a similar argument). This did not seem unreasonable; it amounted to nothing more than making a good empirical case for a claim's facticity.[4]

There are many ways for an ethnographer to make a case for facticity. For example, if you are studying the themes that underwrite an organizational or occupational culture, you can show how the themes suffuse multiple domains of activity or how they co-occur in our informants' talk and action. In my first paper, which was on funeral directors, I took this approach (Barley 1983). I used domain analysis to break down the funeral directors' talk about their work using the techniques of cognitive anthropology (Conklin 1955; Frake 1981; Spradley 1979) and then showed that the different domains of talk and action had an identical structure of meaning by mapping them into semiotic charts that revealed connotations, denotations, and the operation of the same metaphorical and metonymical tropes. Soon afterward, I adopted an even better strategy for appeasing the positivists' need for a warrant by sorting and counting behaviors by time period, category of actor, setting, or some other unit of social life that fit the setting and topic I was studying. In short, I used numbers descriptively (and I occasionally used descriptive but almost never inferential statistics) to bolster my interpretations. At the time, this struck me as no big deal. Anthropologists had been using numbers for years to help them make their interpretive cases (Hage and Harary 1983), as had the ecological psychologists clustered around Roger Barker, who were incredibly obsessive fieldworkers (Barker 1963). But apparently the deal was bigger than I thought. I didn't learn this until one afternoon while Royston Greenwood was driving me from the Edmonton airport to the University of Alberta, where I was to give a talk. He accused me teasingly of messing things up for everybody else. I was flabbergasted as to what he meant. Simple, he said, reviewers' now expected qualitative researchers to include counts to support their storylines.

By the late 2000s, the situation had changed: The kicking shoe was on the other foot. There are now many more qualitative researchers in organization studies than there were when I started. This is particularly true in Europe. Moreover, many of these fieldworkers subscribe to epistemologies and ontologies that are different than mine. In full disclosure, I am what Van Maanen called a realist ethnographer (1988).[5] Suddenly, positivists demanding warrants for claims became the least of my troubles. Out of nowhere, it seemed, colleagues and I began to encounter reviewers who told us that it was inappropriate to mix numbers with qualitative data because doing so is inconsistent with the *true* purpose of ethnography. As one anonymous reviewer recently told me:

> To begin, your data analysis strategy seemed at odds with the purpose of an ethnography. Key to an ethnographic analysis is understanding the world according to the people you are studying (Spradley, 1979, The Ethnographic Interview). . . . This concern grew when I saw you comparing percentages of what you found . . . the comparisons seem inappropriate, reflecting a more deductive stance that fails to take into account taken-for-granted meanings [see Pratt 2009].

There is much going on in this statement with which I would take issue, but here I simply want to set the record straight. Pratt (2009) did not say what the reviewer implies. In fact, Pratt wrote early in his essay:

> Qualitative research can be either inductive or deductive or, in very rare circumstances, a combination of both. Finally, it is possible to analyze qualitative data quantitatively, just as we analyze quantitative data qualitatively when constructing stories around the numbers we present. (p. 856)

My point is not that the reviewer was wrong about Pratt, rather I would like to use his or her comment to raise a larger issue. I view this and similar stances I have encountered over the last decade as an indication that qualitative researchers have adopted their own mantles of dogma and that some are out for revenge! On three separate occasions I have been told that I could learn a thing or two about doing ethnography by reading Barley. How should I make sense of such advice? Is the system of double blind reviews working or failing? Do I interpret the advice as, "Damn, they know who I am and they are telling me I'm not as good as I used to be?" (Van Maanen's words echo in my head.) Should I assume that reviewers see something in my work that I never saw and that it's time to read myself hermeneutically? (The echo gets louder.) More problematically, how should I reply? "Thank you for referring me to Barley, I found his papers very helpful" or as I've been tempted to say, "I have consulted Barley and he assures me that he totally agrees with me"? In all cases, I've let the advice slide and said nothing.

But now, the time has come to say a few words. There are lots of ways to do ethnography. Sometimes the resulting analysis is etic, sometimes it's emic. Neither is inherently better than the other. It all depends on what you are trying to do. In fact, the two can be mixed together without the world ending. Moreover, anyone who equates deduction with the use of numbers and induction with their absence should take a course in logic. All ethnographers employ induction, deduction, and sometimes abduction when they work with their fieldnotes. (The same, believe it or not, is true of quantitative researchers.) What ultimately matters are not these issues, but whether the ethnographer makes a coherent, believable, interesting, useful, and hopefully enlightening case. Has the ethnographer convinced you that the social structures he or she may have witnessed actually existed in the world studied? If the objective is to draw you into the perspective of the members of a group, did he or she do so persuasively and effectively? In short, does the narrative hold together empirically? Do the story and the data fit? And if not, how could the ethnographer do his or her job of convincing and communicating better (assuming he or she has sufficient data to do so)?

What is not acceptable is for a reviewer to say that I subscribe to this ontology and you don't, so you are wrongheaded. When we make such demands, explicitly or implicitly, we undercut our claim of being social scientists, which I take to mean that we are dedicated to telling accurate stories that somehow enlighten us about a social phenomenon. I worry that behind such critiques lie battle lines of brewing internecine warfare based on ontology, which unlike epistemology, can rapidly shade into religion. Whatever faults they may otherwise have, at least the positivists understood how to critique a paper, which is why positivists typically spend considerable time worrying about methods and reviewing papers for methodological adequacy.

Methods Sections as a Genre

Our problem is, what ethnographers actually do when they collect and analyze data is not easily explained. Of course, this is true for quantitative research as well. For example, survey researchers who get lower than optimal response rates (and most all do) are obligated to explain why their sample is or is not biased (usually they claim that it's not). To do this they can only rely on the variables that they have at their disposal, most frequently some demographic characteristic of the sample that they can compare to the population from which the sample was drawn. Whether these are the appropriate variables for assessing bias with respect to the dependent variable under consideration is rarely discussed, even though accounts are given for why the variables on hand might be relevant. More troubling, survey researchers rarely tell us why some questionnaires were discarded, and they almost never admit when they have ignored outliers to reveal a correlation that they know the bulk of the data supports. Nevertheless, quantitative researchers have tools and generally accepted rules about how to use these tools as well as knowledge of their tools' limitations.

We are not so fortunate. The only tools we typically have are ourselves, our informants, some paper, and maybe a tape recorder. How we get from our fieldnotes and transcripts to the stories we tell is difficult to explain. Why did we choose to code some things and not others? Why did we treat certain behaviors or comments as similar and others as different, especially when behaviors or talk tend to be multithematic? Our usual answer is: We code and convey what the data support, and to do this we rely on our integrity and our commitment to scientific skepticism. (I realize that there are some qualitative researchers who believe that it is impossible to ever rise above our own subjectivity, so we should wallow in it. I do not subscribe to this doctrine. If I did, I would most certainly quit being an ethnographer and become a journalist, an essayist, or a writer of fiction.)

There was a day when ethnographers could pretty much get by on their integrity, their commitment to veracity, and their deep familiarity with the social scenes they investigated. They didn't have to write extensive methods sections. Some of the foundational papers in our field were like this. Here, for instance, is the entire methods section from Howard Becker's (1952) famous paper on the careers of school teachers in Chicago published in the *American Journal of Sociology*:

> The analysis is based on interviews with sixty teachers in the Chicago system. The interviewing was unstructured to a large extent and varied somewhat with each interviewee, according to the difficulty encountered in overcoming teachers' distrust and fear of speaking to outsiders. Despite this resistance, based on anxiety regarding the consequences of being interviewed, material of sufficient validity for the analysis undertaken here was secured through insisting that all general statements of attitude be backed up with concrete descriptions of actual experience. This procedure, it is felt, forced the interviewees to disclose more than they otherwise might have by requiring them to give enough factual material to make their general statements plausible and coherent. (p. 471)

The passage appears as the last paragraph in the paper's introductory section and it is not set off from the rest of the text by the heading "Methods."

Since the 1950s, reviewers have urged ethnographers and other qualitative researchers to pen methods sections that emulate the methods sections of quantitative papers. It is worth remembering that the premise of a methods section is that it will allow readers not only to assess the adequacy of the empiricism but also, in principle, to replicate the study if they so desired. We know that these claims are problematic even in the physical and the life sciences (Collins 1974; Lynch 1985). They are even more dubious, if not laughable, with an ethnographic or qualitative study. Yet, over the years, probably through a process of mimesis, ethnographers and qualitative researchers in organization studies have converged on formulae for writing methods sections that placate reviewers. I certainly share some of the blame for this development because even in my earliest papers I wrote methods sections in which I went to considerable trouble to make what I did seem reasonable, if not replicable (see Barley 1983: 399–402; Barley 1986: 84–86).

What troubles me is not that we write such methods sections. It is valuable to force ourselves to try to be as explicit about what we've done as possible, realizing that we always say less than we can ever say, and not for a lack of space. What does trouble me is that methods sections in qualitative papers have taken on the characteristics of a genre: a type of writing defined by specific stylistic and substantive conventions which only the foolish break. Ritualistic adherence to the conventions has become so expected that adherence to the conventions compete with the paper's content for importance.

In 2003, I indulged my conviction that these conventions were useless. I decided to write a short methods section reminiscent of Becker's, albeit several paragraphs longer. In return, I received a three-page lecture from the editor on the difference between qualitative and quantitative research. I learned my lesson quickly: You either give them what they want or your paper doesn't get published. So, in response to the editor, my co-authors and I explained and justified the now much-longer methods section in our letter on the revisions we had made:

> The methods section of our original paper was so brief that it did not allow the reviewers to understand what we had done. The current draft expands considerably on our methodology. Specifically, we make clear that the data we use were part of a much larger ethnographic project that provides the context for our analysis. We offer a more extended discussion of our sample, our coding and our analytic approach. We provide additional counts and cross tabulations to assure the readers that our interpretations were warranted by our data. We also explain how to interpret our numerical data within the constraints of an ethnographic agenda so that readers will no longer hold our counts to the standards of survey data or be tempted to infer that we wish to speak to issues beyond the social world of our informants. We believe that these changes will put to rest the editor's concern that interviews are not appropriate material for an ethnographic approach. We now make clear that the interviews were situated within a larger ethnographic study that involved participant observation. We also emphasize that our focus is

on our informants' interpretations of their experience and behavior, which is the purpose of ethnography and for which ethnographic interviewing has long been accepted as an appropriate method.[6]

I have not systematically analyzed the methods sections of qualitative papers published in organization studies, so I cannot define the elements of the genre rigorously or exhaustively enough to provide counts or to say how the genre evolved. (Although this does strike me as the kind of study that ethnographers who have become too old for the field could do. I do not recommend that the more vital waste their time on the endeavor.) Nevertheless, I can offer a preliminary list of conventions that I would expect such an analysis to confirm. (I think this may count as a hypothesis.)

First, an adequate methods section in a qualitative paper must invoke scholars who have previously blessed the path you've taken as kosher. Oracular texts include Berger and Luckman (1967, who by the way did not write about methods), James Spradley (1979), Lofland and Lofland (1984), Yin (1989), Eisenhardt (1989), and of course everyone's favorites, Glaser and Strauss (1969) and Strauss and Corbin (1990). Many authors also think it's a good idea to use the term *grounded*, ideally joined at the hip with the term *theory*. (I certainly stand guilty here.) Second, qualitative researchers frequently provide a count of the pages of fieldnotes and transcripts that they collected (guilty again). Aside from suggesting that the ethnographer has done a lot of work, what do such numbers tell us? How do we know whether those 500 pages of notes are pure gold or verbal dung? Third, it has become *de rigueur* to talk about how one started with first-level codes then moved on to second-level codes and even to third-level codes, aggregating and subsuming earlier codes as one goes along. Sometimes researchers speak of open codes and axial codes following the language of Strauss and Corbin (1990). A more recent innovation is to provide tables containing illustrative examples of passages from field-notes and excerpts from transcripts that we associated with each code to demonstrate ostensibly and ostensively the reasonableness of the coding. In recent years, I have also adopted this convention to placate reviewers' demands (guilty). Although I can't prove it, my hunch is that researchers select their best-fitting quote to populate such tables so that readers feel more assured that the code matches the content of what was said or done. (You'd be a fool to do otherwise!)

Now that qualitative researchers have a fledgling genre for writing up our methods, what have we gained? Have we somehow made progress since ethnographers like Becker could get away with a brief paragraph on methods? Practically speaking, I suppose we have devised ways of allaying reviewers' concerns over what we've done, or at least a way of hushing them up. But have we come any closer to conveying an epistemic warrant for our claims or for ensuring that our studies are replicable? Could anyone else who bothered to study the same setting find pretty much the same thing? I doubt it, but not on epistemological grounds. I doubt it on substantive grounds. All social settings are rich and complex: They can support a number of viable analytical foci depending upon the researcher's interests.

Consider, for instance, my work on medical imaging and radiology departments (Barley 1986, 1990b). While I had much to say about the machines, how they were used, and the roles that radiologists and technologists played when using different technologies, I wrote nothing about patients. It's not that the data weren't there or that they couldn't be had (in fact, there is a ponderous chapter in my dissertation on patients); it was just that I wasn't interested in patients. A more humanistic researcher with a different agenda than mine could have easily watched the same exams I watched, the same people I watched, the same technology I watched and have written a paper on how patients experience radiological examinations, particularly if they bothered to interview the patients, which I did not. Such a paper might have focused on the experience of being treated like a biological object, of the indignity of a barium enema, the callousness of radiologists, or the fear of learning that one has a deadly disease or a malformed fetus. In short, the raw data would have been roughly the same, but the researcher's focus would have been different and, hence, the analysis would have been totally different. In the end, I submit we are still where Becker was in 1952. Despite our much more elaborate methods sections, readers still have to assess whether the story holds together, whether it fits the data, whether the data are adequate and convincing, as well as trust that the researcher has acted with integrity and a commitment to scientific skepticism. This is no different than with any other method in any other science. Our indignation with revelations of scientific fraud underscores the importance of our willingness to trust that researchers play the game with integrity.

The Tyranny of Alternate Theories

That two ethnographers could enter the same setting; observe the same actions, events, and actors; and come out with different tales points to a third change I have witnessed over my 35 years as an ethnographer. Early in my career, reviewers focused on the adequacy of the evidence for the tale I was telling. They might, for example, ask for additional proof: Just how many times did you see X or how many people said Y and under what conditions? Sometimes they might ask for comparisons: Were the people who said Y different in important ways from the people who either did not mention Y or who said not Y? Sometimes reviewers asked for quotes or excerpts from my fieldnotes to back up or illustrate a claim I made. If I argued that social dynamics evolved through phases, they wanted to know by what criteria I demarked phases and whether these criteria could be made explicit and potentially observable. Were interpretations mine or were they my informants'? They almost always asked me to shorten my findings section and occasionally they asked me to rule out alternative explanations. What I was never asked to do was adopt an alternative theory, perspective, or organizing construct. In fact, I usually received no feedback on the framing of my paper except for requests to write more clearly and to be more precise about the concepts I happened to be using.

In the intervening years, reviewers' expectations and demands have changed. They have done an about face. I rarely receive any comments these days on my findings or my analysis. Instead, the vast majority of comments focus on the theoretical or substantive frame of the story I want to tell. The logic of such comments boils down to this: "You say your paper is about X, but I think it is really about Y." Diane Bailey and I, for example, had a very difficult time publishing a paper that we eventually called "Teaching-Learning Ecologies" (Bailey and Barley 2011) because some reviewers thought we needed to write about tacit learning. Others thought we should be writing a paper about communities of practice. Still others thought we should be writing about knowledge transfer. None of these suggestions was especially helpful. We were simply trying to show how engineers in two different specialties had very different ways of learning what they didn't know, but needed to know, to do complete some task. We weren't even interested in whether the engineers actually learned what they set out to learn or what someone tried to teach them. The point is that the reviews we received focused on the "proper" theory and concepts rather than the data or their analysis. In a more recent paper, Beth Bechky, Bonnie Nelsen, and I were told that we needed to approach our data from the perspective of discourse analysis rather than as an ethnography of speaking, even though the two approaches have very different histories, objectives, and ontologies (Bechky, Barley, and Nelsen, in press).

Numerous ethnographers I have spoken with complain of being subjected to the same kind of critique. When ethnographers gather and talk about publishing, their complaints can be heated. Interestingly enough, no one complains about positivists anymore because our critics no longer delve into the adequacy of our empirics or the coherence of our storylines; besides, these are easy to handle. Rather, they ask us to reframe our data from a different perspective or theory. One colleague recently confided in me that she was considering giving up field research and returning to quantitative research (surveys and experiments) because she was tired of being told her papers should be about something else. She claims that when she does quantitative research, she never gets such requests. The implicit threat in such reviews is that "if you don't tell the kind of story I would have told, I won't recommend your paper be accepted."

There are two problems with this new emphasis on subscribing to the right theory or perspective. First, it cuts against the grain of why one does ethnography in the first place. Ethnographers go into the field to learn how others see their worlds and how their worlds are structured. Few ethnographers enter the field to elaborate, much less test, a theory or perspective. Ethnographers are, of course, interested in variation because variation allows comparative analysis. But the variation that feeds ethnography is defined by conditions, circumstances, and outcomes that are situationally relevant and situationally defined. In the purest sense, ethnographers desire to come back from the field with tales and analyses of how things are among a group of people who are different than us.

To be sure, if we are going to tell such tales in journals, we need an organizing framework, but the framework must mesh with the data. Sometimes a theoretical frame comes from analyzing data (Glazer and Strauss 1965). Sometimes it does not (Blau 1955). But in all cases, the data and the frame must be aligned. Finding a framework or theory that organizes the data without doing it violence can take a long time. In my experience, it often takes years and it usually happens serendipitously (see Barley, 2004: 76–77). One thing I am sure of: With ethnography, the fit between concept and data does not occur because you like a theory and want to apply it to your data. If this is the kind of research you prefer, you should do quantitative work where you can design your methods to your theory.

Second, and more importantly, insisting that a paper adopt a framework different than the one the author prefers only makes sense if the framework better organizes the data. The problem is that unless you are familiar with the data, there is no way you can decide on the framework's relative utility. In the absence of such familiarity, urging an author to adopt a different framework comes dangerously close to admitting, however unwittingly, that organization studies is one of the humanities. In the humanities, interpretations of novels or philosophical works lie in the eye of the reader. Worse yet, if insisting that a paper adopt a different frame amounts to telling the author to write the paper you would have written and if authors were to take the advice, it is easy to see how theories might become fads rather than explanatory or orienting vehicles.

Interviews Versus Observation

The final change I have observed over my career as an ethnographer is an increasing reliance on interviews as opposed to observational data. I can't prove it, but my guess is that the number of observational studies has not significantly declined. Participant observation was never the technique du jour. Rather, I suspect that as qualitative research has become more acceptable, interview-based studies have mushroomed, creating a bias for verbal data. Before I turn to the issues of the preference for verbal data, let me clearly state that I have nothing against interview data and, when used correctly, data on talk is extremely important and often necessary. For instance, if one is studying how informants make sense of their worlds or themselves, interview data are useful. Meaning is to be found in talk. Interviews are also critical components of observational studies, not only because they bring out why people believe what they do, but because they can highlight differences between what people say and what they do. Finally, interviews across a sizable sample of people provide insight into the rhetorics and ideologies to which people subscribe. The truth is, no participant-observer avoids talking with his or her informants, although the talking may not be bracketed in the flow of activity as an interview.

Nevertheless, interviews are dangerous when improperly used. It is important to recognize that interviews may or may not provide evidence of what people do and, for that matter, what they actually think. This is particularly troublesome for one-shot interviews that are done without accompanying observations of behavior. In an interview, people are prone to make themselves look good, to attempt to impress the interviewer, or to provide the kinds of answers they think the interviewer wants. As Spradley (1979) emphasized, if you ask people why they do things, you are sure to get an answer, even if the informants never thought about why they do what they do. To do otherwise would be for informants to reveal themselves as incompetents. If Garfinkel (1967) taught us anything, it is that as social beings, we are always prepared to render our actions and thoughts accountable; otherwise, we look like idiots. Interviews are also poor indicators of what actually goes on in a setting or a line of work. We know that what people say and what people do are often radically different (Bernard et al. 1985). The problem is acute when multiple groups of people are involved in a social scene, but researchers only interview one group of actors (managers, for instance). Failure to collect data from representatives of all groups in the setting can lead researchers to make faulty inferences and draw faulty conclusions.

There are good reasons that the relative number of qualitative papers that rely solely on interviews has probably increased over time. Studies built around interviews are much easier to manage, particularly when researchers have limited time for research. In an era where the number of papers one publishes can matter as much or more than the quality of those papers, interviews definitely trump observational studies. All else being equal, one can do more interview studies per unit time. Interviews also require less commitment from informants than does participant observation. Informants are, therefore, more likely to agree to interviews than they are to participant observation. Finally, interviews give informants more control over what the researcher learns and doesn't learn. It is nearly impossible for people to modify their behavior for the benefit of a researcher when the researcher works in a setting as a participant observer over a long period of time. Of course, informants may initially attempt to perform for the observer, but acting eventually falls away because people cannot afford to neglect their work or other routines forever.

Reliance on interviews may partially explain why certain topics have become more common in organization studies, for instance, identity, sense-making, and careers. Such topics are particularly amenable to analyzing interview data. Conversely, topics that require observation are less frequently studied, for example, work processes, rituals, worker/management relations, situated negotiations, decision making, role relations, and role structures.

The relative infrequency of participant observation in contemporary organization studies bodes ill for our ability to understand workplaces and occupations if work and organizations are changing as greatly as many claim they are. It is worth recalling that participant observation lay at the core of much of the research that defined organization studies as a field during the 1950s and 1960s when organizational theorists built their understanding of bureaucracies (Becker et al. 1961; Blau 1955; Blauner 1964; Burns and Stalker 1961; Dalton 1950; Gouldner 1954; Kanter 1977; Marcson 1960; Roy 1959; Rothlesberger and Dickson 1939; Trist and Bamforth 1951). If the nature of work and organizing is indeed changing, we need more people on the ground studying the day-to-day activities of those who work within these organizations. Only then can we determine if—and if so, how—our theories of work and organization need to change to become more veridical. Simply relying on what we are told, especially by managers, is insufficient. When I was a student, Van Maanen introduced us to ethnography by painting an image of Franz Boas stepping off of a boat on Baffin Island, suitcase in hand, ready for a long stay among the Inuit. For the sake of ethnography and our knowledge of the world, more of us need to undertake the long stay.

Notes

1 For a summary of Reuf's comments on the future of organizational sociology and my eminent demise, see http://orgtheory.wordpress.com/2014/09/03/does-organizational-sociology-have-a-future-the-answer-part-2/.

2 My decision was made at the welcoming party for new Ph.D. students at the Sloan School of Management. I had never heard of Van Maanen before that. But I quickly saw that here was a hippie surfer who got a

doctorate by becoming a cop. How cool was that?! I remember thinking, if you can get a Ph.D. by infiltrating other people's lives and then coming back to tell stories about it, sign me up!

3 For a succinct summary of the history and tenets of logical positivism as formulated by Carnap, Neurath, and others, see Suppe (1977).

4 The careful reader will note that I used the word *facticity* not *fact*, which is an extremely useful and pragmatic ontological hedge.

5 A graduate student of my friend and colleague, Deborah Meyerson, recently told me that Deb had told her that I was the only positivist ethnographer she knew. I don't think a positivist would count me as one of their own, but I must admit I like the irony and tension in the term.

6 You can read the resulting methods section in Evans, Kunda, and Barley (2004).

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