WHAT GOOD IS THE ETHNOGRAPHIC INTERVIEW?

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INTRODUCTION

In 1979, James Spradley (1979) published a small book entitled *The Ethnographic Interview.* In the past 40 years, it has been cited nearly 15,000 times by researchers who, for one reason or another, wanted to distinguish the interviews they have done as being ethnographic. What have we learned in that time about the sense in which interviews can be more or less ethnographic and about the relationship between participant observation—generally recognized as the ethnographic method par excellence (Van Maanen 2006, 14)—and interviewing? Are we in danger of the label "ethnographic" losing its descriptive power and becoming merely an honorific in qualitative research in organizations (Gans 1999, 541)? Or is the bigger risk that, haunted by the ghost of Bronislaw Malinowski (Forsey 2010a, 65), we unfairly malign some very good interview-based studies of culture in organizations by superstitiously insisting they can't be called ethnographies? Much is riding on this in the sense that what we gain in time and flexibility by doing even extensive interviewing, rather than devoting a continuous year or more full-time in the field, is enormous and should not be under-estimated. For academics who are neither graduate students nor on sabbatical, the question of whether interviews and short observations can count as sufficient field work to serve as the basis of ethnography may be the question of whether ethnography can be done it all. Yet I will argue in this chapter that cleanly distinguishing participant observation and ethnographic interviewing is not as straightforward as it might seem,

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particularly when each is done well. It is when they are done poorly that the differences between the two methods are most evident but also least interesting.

PARTICIPANT OBSERVATION AND INTERVIEWING: A COMPARISON

“The most complete form of the sociological datum, after all, is the form in which participant observation gathers it,” Becker and Geer (1957, 28) wrote in their seminal comparison of participant observation and interviewing. “An observation of some social event, the events which precede and follow it, and explanations of its meaning by participants and spectators, before, during, and after its occurrence. Such a datum gives us more information about the event under study than data gathered by any other sociological method.” Their definition of participant observation makes clear why they find it so obvious that participant observation is more complete than interviewing:

> By participant observation we mean that method in which the observer participates in the daily life of the people under study, either openly in the role of researcher or covertly in some disguised role, observing things that happen, listening to what is said, and questioning people, over some length of time (Becker and Geer 1957, 28).

Participant observation includes questioning people. It includes too, as Becker and Geer (1958, 39) add in a rejoinder published a year later, “private conversations with members of the group under observation [that are] in many ways the functional equivalent of an interview and can be used to get the same sort of information.”

This makes sense. As Van Maanen (1983, 43) puts it, “fieldwork, despite the best intentions of the researcher almost always boils down to a series of endless conversations intersected by a few major events and a host of less formidable ones.” Participant observation is mainly conversation. In both anthropology and sociology, most participant observation data is gathered through informal interviews and supplemented by observation (Lofland and Lofland 1995, 18). Fieldworkers “collect droppings of talk” (Moerman 1988, 8) and then fertilize their ethnographies with quotes. Forsey (2010a, 74), describing the results of his study showing that ethnographers report more of what they hear in the field than what they observe, argues that “engaged listening” is as important as, and maybe a better label than, “participant observation.”

Participant observation, then, includes interviewing. It is bigger than interviewing. Interviewing is impoverished by comparison. Interviewing could have no advantage over participant observation because whatever advantage it might have—aside from a savings of time and energy—would automatically accrue to participant observation as well. From this point of view, researchers should limit themselves to interviewing only as a second choice imposed by force of circumstance (Hockey 2002, 209). Framed this way, Becker and Geer’s (1957, 28) argument that “participant observation can thus provide us with a yardstick against which to measure the completeness of data gathered in other ways, a model which can serve to let us know what orders of information escape us when we use other methods,” is unassailable (Atkinson and Coffey 2003, 418).

ADVANTAGES OF INTERVIEWING IN THE CONTEXT OF PARTICIPANT OBSERVATION

The value of participant observation, then, at least in part, is that it allows us to do interviewing better. Fieldworkers gain three advantages from conducting their interviews in the context of participant observation: they are able to ask better questions, get better answers, and better interpret the answers they get.

First, doing participant observation allows interviewers to ask better questions because being there, “living with and living like those who are studied” (Van Maanen 1988, 2), they see things to ask about. Ethnography is about attending to mundane detail. “Usually of most interest is what is unremarkable to participants” (Silverman 2014, 447). The participant observer sees things, and can ask about things, that would never occur to the interviewee to volunteer. Describing his study of the police, Van Maanen says, “informants are sometimes totally unaware of certain aspects underlying many of their own activities. Like fish who are presumable unaware of the water in which they swim, there are things associated with police work that all policemen take more or less for granted and therefore have a hard time articulating” (Van Maanen 1983, 47–8). These are things that won’t come up in response to “grand tour” questions recommended by Spradley (1979, 87) for ethnographic interviews, like, “Could you describe a typical night at Brady’s Bar?” The interviewer has to know to ask about them.

Second, participant observation helps the interviewer get better answers because fieldwork provides the time and opportunity to build rapport with those being studied. Spradley (1979, 51) says about ethnographically interviewing an informant, “at a minimum, it will take six to seven one-hour interviews, so it is important to estimate whether a potential informant has adequate time to participate.” This is long, longer than found in most interview studies. Longer even than what Jackall (1988) did in his excellent interview-based ethnography, *Moral Mazes.* Jackall interviewed 143 people with each interview lasting between two and three hours, some much longer (Jackall 1988, 205). He re-interviewed more than a quarter and many of those a third and fourth time. It takes time for an interviewee to feel comfortable enough to open up and lower their defenses. It takes time for the interviewee to explore with the researcher the meanings they place on events in their world. Participant observation affords time with informants because you are living with them. Informal interviews can be done here and there as activities permit. And asking an informant to take time for a private, more formal interview, may not be such a big request when they have already accepted your presence as a participant observer.

Much has been written about the third advantage of doing interviews in the context of participant observation: a better ability to interpret what interviewees say. This interpretative advantage comes in two forms: a sensitivity to what the interviewee is trying to say and an ability to compare what interviewees say with what they do. Becker and Geer (1957, 29) note the peculiar difficulty that is created by studying groups close to home: the researcher speaks the language. Anthropologists going off to study a people whose language they have to learn before they can possibly interview anybody are aware of the likelihood of misunderstanding.

> But in speaking American English with an interviewee who is, after all, much like us, we may mistakenly assume that we have understood him and the error be small enough that it will not disrupt communication to the point where a correction will be in order.
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> The interview provides little opportunity of rectifying errors of this kind where they go unrecognized. In contrast, participant observation provides a situation in which the meanings of words can be learned with great precision through study of their use in context, exploration through continuous interviewing of their implications and nuances, and the use of them oneself under the scrutiny of capable speakers of the language (Becker and Geer 1957, 29).

Becker and Geer give the example from their ethnography of medical students, *Boys in White* (1961), of the word “crock,” which was a derogatory term for a patient who complains of many symptoms but has no discoverable organic pathology. They do admit that a skillful interviewer might have been able to uncover the use of the word “crock,” its subtle meaning, and its significance in a culture of medical school students who see patients primarily as objects from which they can learn. Indeed, Spradley (1979, 21) says that *The Ethnographic Interview* is, in a sense, “a set of instructions for learning another language,” and gives a very similar example from his interview ethnography of skid row men. He had to learn what “making a flop” meant for these men and, in so doing, came to understand that they don’t consider themselves to be homeless. When asked by a naïve interviewer what is their address, these men—who know the language of researchers and social workers—will respond that they don’t have a home. But to each other, Spradley (1979, 19) learned, they speak instead of having made a good flop last night or about how they used to jungle up down by the waterfront. In his book, Spradley (1979) aims to teach interviewers how to learn such language from interviewees, how to be taught by them, and thus to learn about their culture of the speakers. Becker and Geer’s point is that participant observation makes this work easier.

The participant part of participant observation plays a role too in tuning the researcher to be more sensitive to what interviewees are trying to say, trying not to say, and what they might be feeling. Goffman puts it this way:

> I feel that the way this is done is to not, of course, just listen to what they talk about, but to pick up on their minor grunts and groans as they respond to their situation. When you do that, it seems to me, the standard technique is to try to subject yourself, hopefully, to their life circumstances, which means that although, in fact, you can leave at any time, you act as you can’t and you try to accept all of the desirable and undesirable things that are a feature of their life. That “tunes your body up” and with your “tuned-up” body and with the ecological right to be close to them (which you’ve obtained by one sneaky means or another), you are in a position to note their gestural, visual, bodily response to what’s going on around them and you’re empathetic enough—because you’ve been taking the same crap they’ve been taking—to sense what it is they’re responding to (Goffman 1989, 125-6).

Organizational ethnographers should be cautious about taking this too far. We typically observe much more than we participate in the organizations we study. We most often go home each night. We seldom truly take the same crap taken by the people we study because the organizational stakes are so different for us than for them. But, by remaining close enough to the people taking that crap that we can smell it and get some of it on our shoes, we may understand better what they tell us in response to our questions.

"In essence," Van Maanen (1983, 37-8) writes, "ethnographers believe that separating the facts from the fictions, the extraordinary from the common, and the general from the specific is best accomplished by lengthy, continuous, firsthand involvement in the organizational settings under study." Speaking about his own ethnography of police, he says that "only by observing the phenomena firsthand and questioning the police about the actions they had just taken (or not taken) was I able to corroborate and elaborate upon what my informants were telling me" (Van Maanen 1983, 41). Goffman (1989, 131) is more blunt: "I don't give hardly any weight to what people say, but I try to triangulate what they are saying with events." What we get from interviews are accounts, "researcher-provoked data" as opposed to "naturally occurring data" (Silverman 2014, 357). This point is central for Becker and Geer:

> Participant observation makes it possible to check description against fact and, noting discrepancies, become aware of systematic distortions made by the person under study; such distortions are less likely to be discovered by interviewing alone (Becker and Geer 1957, 31).

Dean and Foote Whyte (1958) argue in their famous article, "How Do You Know If the Informant Is Telling the Truth?" that separating fact and fiction and detecting distortion is possible too in an interview study without participant observation especially when, as in the case of ethnographic interviewing, the study involves lengthy interviews and many of them, not simply one. This gives the interviewer the opportunity to compare an informant's account with other things he or she has said and with the accounts given by other informants. Over the course of repeated interviews, and as part of the process of building a relationship and rapport with informants, the researcher will also make inferences about their relative reliability, openness, and truthfulness. Nevertheless, participant observers, seeing and hearing the people they study in many different situations, will have an even better basis for making such inferences and, as Becker and Geer (1958, 40) conclude, "the inference can never be more accurate than the observation and may be less so."

ETHNOGRAPHY ALWAYS INVOLVES INTERVIEWING

Presumably no one is arguing that a study consisting of only interviews done with no participant observation whatsoever is ever preferable if the goal is to create ethnography. This chapter is about whether it is possible; I take for granted it is sometimes necessary and often convenient, but never preferable. Even if we were interested in doing an ethnography of the culture of people who volunteer to be interviewed by ethnographers, we might profitably observe them waiting to be interviewed and also sit in the interview chair ourselves to see how it feels. Certainly, this is not something that Spradley is claiming for the ethnographic interview:

> I want to discuss … the interaction that goes on *during* interviews. In doing this, however, we should not lose sight of the wider context of field work. Most ethnographers will conduct participant observation at the same time, thus encountering key informants when they are working, visiting friends, enjoying leisure time, and carrying out ordinary activities (Spradley 1979, 79).

As Agar (2008, 156) points out, framing the question—participant observation or interviewing—as if it were a choice between a) listening to people in an isolated room, or b) observing them without ever listening to what they say about what they do, is absurd. What I am arguing is that ethnography always involves interviewing and that this interviewing benefits from participant observation. Once I have established that, the remaining question is how much of that special benefit, if any, is needed to create good ethnography.

In his “Comment on ‘Participant Observation and Interviewing: A Comparison,’” published in *Human Organization* next to Becker and Geer’s piece, Trow (1957) makes the argument that we can’t claim advantages for participant observation or interviewing as method in general.

> The alternative view, and I would have thought this the view most widely accepted by social scientists, is that different kinds of information about man and society are gathered most fully and economically in different ways, and that the problem under investigation properly dictates the methods of investigation (Trow 1957, 33).

If we accept that this view is as widely accepted as Trow claims, then it means that to understand what methods might be appropriate for ethnography, we need to take a step back and consider what is the “problem under investigation” in the case of ethnography. This is contentious because there are many different definitions of ethnography and they fall into two types. There are definitions that conflate ethnography and participant observation and those that treat them separately (Hockey and Forsey 2012, 69). Definitions of the first type are obviously unhelpful to our project because they assume the answer. Definitions of the second type are what we want. Van Maanen’s (1988, 1) is clear: “an ethnography is written representation of a culture (or selected aspects of a culture).” It is the product, the outcome. “Ethnography is the result of fieldwork, but it is the written report that must represent the culture, not the fieldwork itself. Ethnography as a written product, then, has a degree of independence (how the culture is portrayed) from the fieldwork on which it is based (how culture is known)” (Van Maanen 1988, 4). This is in line with Geertz, who puts it this way:

> From one point of view, that of the textbook, doing ethnography is establishing rapport, selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary, and so on. But it is not these things, techniques and received procedures, that define the enterprise. What defines it is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, “thick description” (Geertz 1973, 6).

Famously, Ryle (2009 [1968]) compared the thin description of two boys rapidly constricting their eyelids with the thick description of one boy with an involuntary twitch and another boy who is winking. Ethnography as thick description is a "construction of other people's constructions of what they and their compatriots are up to" (Geertz 1973, 9). Or, as Van Maanen explains, organizational ethnography aims "to uncover and explicate the ways in which people in particular work settings come to understand, account for, take action, and otherwise manage their day-to-day situation" (Van Maanen 1983, 39).

Interviewing is central to ethnography because to learn how people understand and account for their day-to-day situation, to learn what they think they are up to, we should ask them. To ask them and simply take their response at face value is naïve. But to not ask them at all is ethnographic malpractice. If, as Van Maanen (1983, 52) argues, "the essential ethnographic question" is "what it is to *be* rather than to *see* a member of the organization," and if we are appropriately modest about the extent to which we, as researchers, even after a year or more of fieldwork, can feel confident that we experienced firsthand what it is like to be an actual member of the organization, then we have no choice but to ask people and make what sense we can of the accounts they give. Also, ethnographers care about things that they cannot observe. This includes accounts of what people are thinking and how they are feeling, but also historical events, infrequent events, and events that happened elsewhere that are context for the current situation. This is why, as Bernard says, "unstructured interviewing is the most widely used method of data collection in cultural anthropology" (Bernard 1994, 208).

DECENTERING PARTICIPANT OBSERVATION

Observing and participating are, in contrast to interviewing, decentered in organizational ethnography. As Agar (2008, 9) says, “observation is subordinate to what one learns in interviews. Observations are ways to test out what you have learned, ways to complicate and contradict the encyclopedia, and develop additional interviews and conversations based on these problems.” Agar is exaggerating here, but in many organizational ethnographies, observation is most important for the ways, described above, that it helps the researcher do better interviews. The trope that observation is important because observations represent the “facts,” the “naturally occurring data” as opposed to the mere “accounts,” the “researcher-provoked data,” provided by interviews is suspect on two counts. First, it is naïve of two of the “Ten Lies of Ethnography” identified by Fine (1994). As participant observers we don’t observe everything, not even everything that happens while we are present. We also don’t capture in our fieldnotes everything we observe, and we capture nothing in our fieldnotes exactly as it happened. Second, the reflexive turn in ethnography has made unqualified claims by researchers of determining the fact of the matter to compare with accounts given by informants seem fairly quaint (Atkinson and Coffey 2003, 426).

The importance of the participation part of participant ethnography has always been problematic, suffering from what we might call the Yogi Berra problem, after his famous quote that “nobody goes there anymore, it’s too crowded” (Berra 1997, 16). Participant observers—aside from those writing autoethnography—are always both participating not enough to really experience what the people under study are experiencing and participating too much to avoid influencing the scene under study. In my experience, the more important of the two problems is usually that we do too little participating. The organizational ethnographer doesn't have the advantage of doing what Malinowski called "open-air ethnography" in the Trobriand Islands (Stocking 1983, 111). The people we are studying don't live outdoors in situations of low privacy. We are lucky if we are able to find a role to be able to work among them; we seldom live among them in their homes. Our access, as Kunda (1992, 236) notes is typically inversely related to hierarchical level since one indicator of power in organizations is often the ability to preserve privacy. Kunda's (1992) book, *Engineering Culture,* is one of the best organizational ethnographies written. By his own account, with top managers he had to content himself for the most part with formal interviews and with his main contacts—engineers and middle managers—he observed more than he participated, and he observed less than he would have liked (Kunda 1992, 233-7). This echoes my own experience doing the fieldwork for *Unpopular Culture* (Weeks 2004) where "participation" most of the time meant shadowing someone and asking endless questions about what they were doing: in essence, interviewing them. Unobtrusive participant observation of a back office bank clerk working alone on his or her computer is not very illuminating. Easier were aspects of work that were more "open-air" such as meetings, customer visits, conferences, and internal training programs. In other words, aspects of work where people talk to each other.

Apropos of the decentering of participant observation, the inveterate fieldworker, Geertz, says:

> There is a certain value, if you are going to run on about the exploitation of the masses, in having seen a Javanese sharecropper turning earth in a tropical downpour or a Moroccan tailor embroidering kaftans by the light of a twenty-watt bulb. But the notion that this gives you the thing entire (and elevates you to some moral vantage ground from which you can look down up on the ethically less privileged) is an idea which only someone too long in the bush could possibly entertain (Geertz 1973, 22).

When your audience is a group of people many of whom may well have been long in the bush, it may be that, even more than its role in enhancing interview quality, the most important thing about participant observation is the credibility, the face validity, that a year or more of participant observation affords your ethnography. Goffman calls it the justification and warrant:

> I think you should spend at least a year in the field. Otherwise you don't get the random sample, you don't get a range of unanticipated events, you don't get deep familiarity. It's deep familiarity that is the rationale—that plus getting material on a tissue of events—that gives the justification and warrant for such apparently "loose" things as fieldwork (Goffman, 1989, 130).

A year is clearly arbitrary and yet Sanday (1983, 20) notes that most ethnographers would agree that at least a year of participant observation is required to do serious ethnographic work. That is to say, I suspect, that most ethnographers who themselves did at least a year of participant observation would agree that at least a year of participant observation is required to do serious ethnographic work. It is a rite of passage as much as a research method.

ETHNOGRAPHIC INTERVIEWS AND THE INTERVIEW ETHNOGRAPHIES

Organization studies has many examples of excellent ethnographies which are based primarily on interview data and which did not involve a year of participant observation. I have mentioned already Jackall's (1988) *Moral Mazes*. The 143 intensive interviews he did were complemented by "attendance at some management meetings, informal conversations and discussions over meals, coffee, and drinks and participation at a range of social events," as well as, "two seminars for up-and-coming managers in the company. Hochschild's (1983) *The Managed Heart* is another example of excellence. Fine (1994) calls *The Managed Heart* "estimable" and "one of the most influential ethnographies of the past decade," but also "not richly ethnographic" because her methods are not primarily participant observation. In fact, those methods include an open-ended questionnaire handed out to 261 of her Berkeley students as well as interviews with flight attendants, union representative, airline officials, advertising agents, bill collectors, the public relations people assigned by the airline to handle her, and a sex therapist with ten years of experience treating flight attendants as clients. The participant observation she did consisted of attending training sessions for flight attendants and having lunch and, once, dinner with trainers, observing the recruitment interviews of flight attendants, a guided tour of an airplane and a two-hour visit to an airplane galley (Hochschild 1983, 12–17). To choose one more, Nippert-Eng's (1995) *Home and Work* is correctly singled out by Van Maanen (2006, 15) as a "wonderful" example of multi-site ethnography. Her sources were:

> Seventy-two, two- to six-hour hour interviews with employees at "the Laboratory," or "the Lab," a research laboratory in the Northeast United States. (A copy of the interview schedule I developed is included as an appendix to this text.) I also incorporate observations made on-site while conducting the interviews. Six pretest interviews and dozens of less formal discussions with family, acquaintances, and other members of the Laboratory supplement the formal interviews (Nippert-Eng 1995, Kindle Locations 492–5).

In sociology, the tradition of interview ethnography goes all the way back to the Chicago School under the leadership of former newspaperman Robert Park. As Van Maanen (1988: 19) explains, a good part of this work was interview based with the interviews being intensive and repeated and accomplished in natural settings that provided the opportunity for observation.

So, it is possible to do interview ethnography, at least in sociology and organization studies. In anthropology, the stakes are higher because ethnography is so important to the discipline that it is often identified with it in the sense that, for an anthropologist, not having done ethnography is a source of status deprivation and not having done at least a year of participant observation means not having done real ethnography (Marcus 2009). Hockey describes graduate students in anthropology "reluctant to describe their rich qualitative interviews as ethnography, fearful that an external examiner might judge their methods inadequate when measured against this term" (Hockey 2002, 209).

> As a discipline, we may have become less apologetic about the study of locally produced "exotica," but participant-observation continues to occupy the methodological high ground (Hockey 2002, 209).

Agar describes similar apprehensions among applied anthropologists:

> The applied anthropologists have obsessed for years about research opportunities that offered them *less* time that a year. Their intuitions—I'm guessing—were that the opportunity shouldn't be tossed away; you could learn *something* important in a shorter period of time. But they were embarrassed about it, because by the old one-year standard it only added to the stereotype of applied work as somehow deficient (Agar 2008, 38).

Enthusiasts of participant observation in sociology and organization studies more often see the opposite problem: that "empirical ethnography is now a synonym for virtually all qualitative research except surveys and polls" (Gans 1999, 541).

> Why ethnography has gained widespread acceptance among social researchers to become the label of choice for much of the qualitative/descriptive work currently being reported remains something of a mystery. Whether ethnography is the appropriate label for some of this work is even more questionable (Wolcott 1995, 79).

Protecting and defending the boundaries of the territory that "real ethnography" is allowed to occupy holds more than academic interest to academics. It is a matter also of identity and status.

It is possible, of course, to do interview studies that are not ethnography. The name for this is "qualitative research." Interview studies make up the vast majority of qualitative research (Denzin and Lincoln 2018, 577), and it is not hard to sympathize with researchers who, having invested dozens or scores of hours in intensive and serial semi-structured interviews, conducted with the "ethnographic imaginary" (Forsey 2010b, 569), "that definitionally-elusive ethnographic sensibility" (Yanow 2009, 194), want to distinguish their work from the mass of thinner descriptions based on other, presumably easier, interview techniques.

> Among the biggest problems faced by researchers who rely heavily on semi-structured interviews are boredom and fatigue. Even a small project requires 40 to 60 interviews to generate sufficient data to be worthwhile. Most anthropologists collect their own interview data and asking the same question over and over again can get pretty old (Bernard 1994, 221).

Ethnographic interviewing is efficient compared to participant observation, but it is time and energy consuming compared to other types of interviews. This is part of its definition:

> Thus, both the time factor—duration and frequency of contact—and the quality of the emerging relationship help distinguish ethnographic interviewing from other types of interview projects by empowering interviewees to shape, according to their world-views, the questions being asked and possibly even the focus of the research study (Heyl 2001, 369).

It is even part of the name of interview techniques synonymous with ethnographic interviewing: "the long interview," (McCracken 1988), "the active interview" (Holstein and Gubrium 1995). Ethnographic interviewers are as concerned with the size of their *N* (number of hours of interviews and pages of transcriptions) as are participant observers (number of days of fieldwork and pages of fieldnotes).

WHAT IS A GOOD ETHNOGRAPHIC INTERVIEW?

If we agree with Van Maanen (1988, 1) that ethnography is a product, an account of a culture, or selected aspects of a culture, and if we agree with Clifford (1986, 7) that ethnographic truths are inherently partial—committed and incomplete—then we should evaluate how ethnographic an interview study is on the basis, not of its *N,* but of how well it succeeds as an account. How well does it help the reader "to understand themes of the lived everyday world from the subjects' own perspectives" (Brinkmann and c 2015, 31)? How well does it translate "the meaning of actions and events to the people we seek to understand" Spradley (1979, 5) to the reader? How convincingly does it manage to interpret "presentational data"—those data which concern the appearances that informants strive to maintain (or enhance) in the eyes of the interviewer or fieldworker, as well as colleagues and even themselves (Van Maanen 1983, 42). How well does it account for all the things that the interview situation may represent to the interviewees, shaping the narratives they construct with the interviewer: the interview as identity work, as political action, as local accomplishment, as a cultural script, and so on (Alvesson 2003). How well does it cope with the challenges of reflexivity and grasp the opportunity that interviews don't simply (and sometimes don't at all) collect information about nonobservable or unobserved actions, or past events, or private experiences: "interviews generate accounts and narratives that are forms of social action in their own right" (Atkinson and Coffey 2003, 424)? Does it produce "new concepts, concepts that take you closer to the world that is the object of research than previous understandings could have," (Agar 2008, 40) and does it draw upon sufficient data from numerous different sources, all of which support its conclusions in a "massive over-determination of pattern" (*ibid.*, 37)?

I suspect that it is because these aspects of quality are loose and hard to assess that we so often fall back to the size of the *N.* The time we invested and the paper trail we created in collecting data for the study is, as Goffman (1989, 130) said, our warrant to justify to ourselves and our readers that we have gathered enough data to credibly claim that we have learned something about the culture of the people we are studying, that we have escaped a little bit the gravitational pull of postmodern doubt that we can learn anything from interviews except how people behave in interviews and that we can learn anything in participant observation except how biased, subjective and human we are, that all ethnography is autoethnography. Yet, the truth of the sentiment that ethnographies are inherently partial (Clifford 1986, 7), that cultural analysis is inherently incomplete (Geertz 1973, 29), should lead us away from grandiose pronouncements of what can and cannot be counted as ethnography—the academic equivalent of peeing on fence posts to mark the territory of ethnography—and away from pulling out our *Ns* to compare sizes and assign status. Gans (1999, 544) is surely right to lament the fact, if it is a fact and not just nostalgia, that participant observation is in decline because it fits less well into today's academic economy. Brinkmann and Kvale (2015, 337) are equally right to point out that not everything should be geared toward minimizing time. If we want to understand other people, we have to spend time with them. On the other hand, as every ethnographer knows, after a certain point, more data as often overwhelms as much as edifies. Reserving the label "ethnography" as some sort of reward to motivate scholars to spend more time in the field seems to me distasteful and unhelpful and—in the case of organization studies, if not anthropology—fairly hypocritical given that the methods underpinning many of our best ethnographies fall conspicuously short of a continuous year or more of participant observation in the field.

As Agar (2008, 64) notes, the opposition between participant observation and interviewing has been with us since the birth of modern anthropology and the time of its founding fathers.

> Franz Boas stressed the interview as he tried to get descriptions of disappearing American Indian cultures from older informants; Bronislaw Malinowski pitched his tent among the natives, learning their language and sharing their lives (Agar 2008, 64).

The close relationship between the two approaches has been apparent for that long too, however. For Malinowski, what was to be studied was a "long conversation" taking place among the people we are studying, a conversation in which we inevitably join (Bloch 1977, 278). It was Boas, "stepping off the boat in an Eskimo village, with his suitcase in hand, preparing for a long stay in residence," that established the paradigm of fieldwork in American anthropology (Wallace 1972, 469).

Interviewing, by which I mean some mix of informal questioning, conversation and formal unstructured interviews, is a central part of participant observation and it is where most ethnographic data comes from. Participant observation is the preferred method of ethnography in large part because of the advantages that interviewing done in the context of participant observation has over interviewing done without that benefit. Participant observation is not the same as ethnography, however, and it is not essential for it. We have seen in this chapter good examples of ethnographic interviewing and its product, interview ethnography. Ethnographers who cite Spradley (1979) would do well to read him too and imitate exemplary interview ethnographies. Ethnographic interviewing is difficult and time consuming to do well. It requires a substantial commitment of time and energy (and, *pace* Bernard (1994), apparently a high boredom threshold), albeit a commitment that conveniently doesn't require continuous time away from work, family and other obligations. If some readers and reviewers are suspicious of the very idea of interview ethnography, it may be in reaction to papers that claim to be based on "ethnographic interviews" but read as if what their authors meant by that method was "interviews requiring little preparation." In this case, we should keep in mind Sturgeon's Law. Science fiction writer Theodore Sturgeon (1957) noted, in response to claims that 90% of science fiction is of poor quality, that “90% of science fiction is crap, but 90% of everything is crap.” There is plenty of bad ethnography of all kinds, the difference with interview ethnography is mainly that it can be done badly more quickly. The similarity with participant observation ethnography is that it can also be done well.

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