

3. Your completed interview, however you recorded the answers (i.e., sometimes on the interview, if precoded; sometimes on another sheet; or on a coding form, with "probes" and qualitative comments as you choose)
4. Your two sets of notes on the two interviews in which you were involved
5. Your coding form or code book for your survey
6. Your code sheet that employed the code book on your interview (that is, your single respondent as coded)

What you now have done, if multiplied by two hundred, simulates the production of a body of survey data.

EXERCISE IN SURVEY DATA ANALYSIS

This exercise picks up where your simulated survey research ended, except that a real body of survey data will be provided by the instructor. You now have a body of survey data out of which your job is to create order. You are to select a few variables of interest to you, create a hypothesis (and rivals), then explore the data, ordering them according to your hypothesis test. That is, develop an elaborated causal model, and test it in the data. Use four, five, or six variables if you wish, but at least a bivariate and two other controls. You may want to do more, but this much is minimum.

Write up your analysis in seven or eight pages as if it were a paper being submitted for publication. The problem should be "real" and the paper should begin with a review of existing literature, an operationalized and problematic hypothesis, brief reference to research design, and so on. The bulk should be thoughtful analysis and data interpretation. You need no statistics, but can use them if you wish; percentaged tables will suffice.

A copy of the code book and data frequencies will be available in the sociology lounge for your referral. A list of potential variables for your analysis will be handed out in the lab. A sample printout should also give you an idea of how to get started.

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READING: 4

Goldenberg, Sheldon (1992). Thinking
methodologically. New York: Harper
Collins Publishers

Chapter

18

Interpretive Methods

Objects can be known purely from the outside, while mental and social processes can be known only from the inside," as well as through the shared meanings and interpretations we give to the objects. "Hence, insight may be regarded as the core of social knowledge. It is arrived at by being on the inside of the phenomena to be observed. . . . It is participation in an activity that generates interest, purpose, point of view, value, meaning, and intelligibility as well as bias.

(Louis Wirth, 1949, as quoted in Filstead, 1970, p. 4)

INTRODUCTION

Interpretive methods begin with a commitment to what we have called an "insider" perspective, which in turn involves a determination to somehow get inside the actor's mind. The guiding premise is that it is here alone that the explanation for intentional and self-conscious human behavior can be found. From this position, then, what sorts of methods exist? In addition to participant observation, researchers with such a commitment may adopt other techniques, some of them arguably even more direct. Introspection is one such tool, providing each of us with access to at least one person's intentions, desires, motivations, and definitions of the situation. Some qualitatively inclined social scientists might make the claim that introspection is a sufficient tool for discovering many things about identity formation and transformation, for example. George Herbert Mead, Sigmund Freud, and W. I. Thomas contributed quite a lot to our understanding of the development of self by looking inward and considering their own behaviors. How else can one get direct access to information about such internal processes? Of course, there are problems with such a strategy, and generalizability is the first and major one to occur to any skeptical critic. What reason is there to believe that my experience of acquiring an identity is the same as that of some other person, particularly one of a different ethnicity, social class, or gender? Beyond that, do we

really have direct access to the truth about ourselves? Do we not lie to ourselves, fool ourselves, and dupe ourselves? Is information acquired by introspection automatically valid? I think not. On the other hand, is such information automatically to be disqualified? Again, I think not.

Phillips suggests that we might make more use of personal experience than we do at present. He argues that as a person (accidentally but not importantly also a sociologist) who has played touch football on weekends for several years, he has access to personal experience in understanding the rules governing such activity that no sociologist could discover from PO, or in any other way, without a huge investment of effort. Why should he not be able to use that personal knowledge and transform it into sociological material by focusing upon it his perspective as a sociologist? S. M. Miller has written a rather insightful essay—"The Making of a Confused Middle Class Husband" (1971)—in which he reflects on his married life from the point of view of a sociologist. Jessie Bernard has written in a similar vein on her experience of women's issues through the recent history of the American Sociological Association (1973). It appears that the source of one's data must always be defended, but there is no source that is automatically invalid.

Although some authors actually enumerate and discuss a number of methods in this general interpretive category—including the strategies of the life history, film or visual sociology, the ethnography, the intensive interview, the ethnomethodologist's "breaching" experiments, the general idea of *verstehen*, and qualitative content analysis and its relatives, semiotics, conversation or discourse analysis and hermeneutics—I will not. Even from the list I've just provided, it is clear that experiments, interviews, documentary analysis, and historical methods are not *per se* qualitative or quantitative; they appear in this list and could as well appear on a list of potential positivistic methods. It is at this level of categorization that the distinction indeed breaks down. In fact, Norman Denzin in his symbolic interaction-based text on research methods defines participant observation as such an umbrella methodology that it includes all the above methods and more. He is correct, of course, but any meaningful distinction between participation observation and other methods is obviously lost in this manner of classification. I would rather simply say for now that interpretive methods share a focus on discovering, entering, and sharing the universe of discourse of the actors. They make the assumption that actors act intentionally and that extended contact can allow us to come to share the actors' definition of the situation, explaining their behavior insofar as this orientation is capable of doing so, in our judgment.

The core of the appeal of Cooley and George Herbert Mead, Schutz and Merleau-Ponty, Garfinkel, Lyman and Scott, and others is the experience of *insight* they give into social reality. . . . This emphasis on an insight experience . . . approaches the classical literary ideal in demonstrating the universality of these phenomena, giving the experience of recognizing the universal in the particular. (Collins, in Manis and Meltzer, 1978, p. 398, italics added)

In using interpretive methods we undertake a very difficult task—that of doing work that is at once both insightful and also sociological. Either task would be

difficult enough, but we have undertaken to do both, and in neither case are the criteria very clear, to say the least. Though there is little consensus about research conducted in this tradition, in part because proponents often abhor the idea of guidelines as something that smacks of the positivistic tradition and in part because many of these same proponents see their work as guided more by aesthetic than "scientific" standards, there are a few suggestions I am nonetheless prepared to make. In doing so, it should be clear that I take the position that if work is to be sociological as well as insightful, it must be "scientific" still in some sense of the word.

First, then, I am willing to assume, and prepared to argue, that sociological studies in this tradition must still be research studies. This has several implications of importance. Research studies generally locate their topic in the context of the relevant literature. They identify a sociological "problem" and critically use a review of the appropriate literature to establish the context for research on that problem. Whether this literature is introduced at the beginning of the study or at the end, it must be there so that you can demonstrate the sociological relevance of your research in the sense of how it relates to ongoing research and/or substantive tradition, and how it extends that tradition. Most often in interpretive work, the relevant literature can be identified as such only after the fact, since much of such work is exploratory. This does not, however, mean that there is no relevant literature; it means the literature review cannot be completed before the project is begun. In fact, the positivistic tradition also recognizes an interaction between identification and refinement of the problem and ongoing review of relevant literature. Note that the *review* is done throughout the project, but the resultant research paper is most often (though not necessarily) a reconstruction of the research process. In the research process, literature is identified and examined at all stages from the first idea you have to project completion. It is literature that provides you with a sense of the plausibility of your model or explanation. It is literature that guides your selection of comparison cases, and this in turn guides your literature review. Interpretive sociology involves a different sequence from that of the more standard positivistic social science. In the latter, one commonly completes the data collection before the analysis begins. In the former, data analysis and data collection occur together throughout the project. Each informs and alters the other, but each does have a place. Interpretive research is not fiction nor does it set out to create the world anew each time.

Second, interpretive researchers still have to describe how they chose to conduct the research, and why they made that choice. They have to defend the appropriateness of their method to the problem at hand, and they have to argue their case to a hypothetical skeptical reader who is willing to be persuaded only by strong evidence and logic.

Third, interpretive researchers must have a data base that is described and utilized in such a way as to make clear to the reader exactly what it is. For interpretive purposes, I have suggested that this data base usually deals with what has been called "insider" information—perspectives, definitions of the situation, or "facts" as believed and expressed by the actors themselves, in their own words wherever possible. It is this data base that is then subjected to sociological analysis; but the

Do not presume to tell them their reality. Simply (sic) make yourself vulnerable to the experience, and they will teach you to see reality as they do.

WHAT CAN BE ACCOMPLISHED?

While it is possible to consider PO as simply a means of collecting data with which to test predetermined hypotheses that have emerged from elsewhere, the method is acknowledged even by its critics to be an excellent *exploratory* technique that is not very fully exploited when used in this rather insensitive manner. It is quite common for researchers (even those who practice it) to believe that it is no more than exploratory, or to limit its use to exploration and the *generation of areas for study* ("problems") that are derived from and defined by the actors' experience. Some practitioners may go so far as to suggest that PO is useful in *generating hypotheses*, which can then be systematically tested in a more extensive and "scientific" study, by which they usually mean survey research.

On the other hand, the more committed interpretive researchers believe that participant observation is a complete and sufficient methodology in and of itself for the exploration of the actors' world, the derivation of insightful sociological hypotheses from that world experience, their systematic testing through the methods of analytic induction or purposive sampling or what Glaser and Strauss call "constant comparison," and substantial theory building based on the results of that testing alone. For this minority, PO is a reflexive technique in which one constantly uses data to generate and test emerging and revised hypotheses and build theory. One shifts from one purpose to another continually over the course of the study at hand, systematically collecting more data in an effort to test one's expectations drawn from theory that becomes progressively more plausible. Since one always searches for universal propositions in analytic induction, the sampling implied involves constant comparison of cases that are like in some ways yet different in others, not unlike the logic of quasi-experiments. If one conceptualized this as purposive sampling or theoretical sampling, it amounts to the same advice—pick the next group or instance for observation on a theoretical basis, that being that observations in that instance should most clearly force you to modify or reject your tentative hypothesis if it is not universal. For these researchers, PO is a triangulated methodology, involving everything one can do in the field, and making use of documents, observation, in-depth interviews, unobtrusive measures, and whatever else comes to hand in the natural environment of the actors.

It is a method requiring considerable time and enormous labor by the researcher. It takes great patience, empathy, and other social and intellectual skills; and even for the experienced interpretive researcher, it is very stressful and anxiety producing. It is certainly the most difficult and intense methodology I have ever tried, but in some ways it is also the most rewarding. As to how far the method can take you, on this progression from "mere" descriptive reconnaissance to complete testing-theory construction, I will encourage you to be the judge. At this point, I

simply want you to be aware that there is a considerable range of opinions, as usual, even among those who practice participant observation. I will say that it seems to me to be a waste of the potential of PO to use it only as a data-collection technique—whether this be to test hypotheses derived from elsewhere or to collect descriptive data only. PO can allow you into the actors' world if you are good at it. Using it for less than that is like using a sledgehammer to drive a nail.

THE ASSESSMENT OF INTERPRETIVE WORK

As to the criteria for assessment of studies done in this vein, again there is a division among practitioners, though opponents are quite clear. Some supporters are willing to try to address the standard issues of validity, reliability, replicability, generalizability, the demonstration of causality, and so on; others are not. Becker, for example, is concerned with these issues, and addresses them in several papers. He goes to considerable pains to help interpretive researchers structure their work so as to position themselves to be able to respond to criticism on such grounds. He advocates, for example, that one distinguish between volunteered and directed statements in the context of assessments of the validity of what one is told, and even suggests that one could describe statistically the frequency and distribution of the phenomena of interest (Becker, 1958; Becker, Geer, and Hughes, 1961.) Elsewhere, he deals with the issue of reactivity, acknowledging, as not all interpretive researchers do, that this is indeed a problem, but suggesting that it is a limited one that solves itself over time, for several reasons, discussed elsewhere in this book.

Other interpretive researchers sometimes take the position that they ought not to be forced to meet positivistic criteria when their view of social science is so fundamentally different. This group splits into those who argue that their work is essentially artistic and unassailable by scientific criteria, and those who address the issue of criteria head-on. This latter position too has already been discussed at some length—first, in the context of the feminist critique of positivism, and second, in the section dealing with issues of validity as seen from an interpretive position. The general position adopted emphasizes the critical role played by those whose world we wish to enter, by "consumers" and by colleagues, in contrast to dependence on the professional "scientific criteria" to which the positivist typically subscribes.

ACCESSING A SUITABLE SITE

The usual first problem in PO is access. One needs a group or situation to study, and it must be a group or situation to which one has access. This is not to be taken for granted, for groups frequently but not always control access to them, and some guard it jealously. As a graduate student in Chicago, I worked out a research design for my dissertation based on doing participant observation in the Ontario Secondary School Teachers' Federation (OSSTF). This organization of high school teachers in the province of Ontario was changing rather dramatically as a younger,

more professional, and more aggressive group of baby boomer teachers joined its ranks and challenged the authority of the older teachers who were the executive. This much I knew from several friends who were secondary school teachers in Toronto. I thought that a PO study of the possible transformation of a professional group into a union would be an interesting piece of work and one best done from the inside. I applied to the OSSTF executive for permission to attend meetings and review their records, and in general to hang around their offices and watch them do their job. Access was denied. I had not anticipated a problem; but the response demonstrated that the executive was well aware that they were in a battle for their jobs, and they wanted no social scientist witnesses. My contacts, my preparation, my interest, and my genuine concern for the organization were of no avail. I was forced to change topics. Many others can recount very similar denial of access, however sympathetic they may have been to the group or situation.¹

Politics is the name of the game, and interpretive researchers from the beginning grant authority (and control) to the actors whose world they wish to visit. This is rather unlike the position of the positivists, who are far more likely to impose their research demands on their "subjects," giving them at most the option to participate or not. (This was not done routinely until fairly recently and is clearly not encouraged. Indeed, the option is sometimes presented as rather "slimy" and subversive.) Think of the experiment again, in which even now it is commonly the case that student samples are strongly encouraged to participate as "volunteers," if their participation is not simply required. Think of the questionnaires you may have received in which some investigator has taken the liberty of addressing an instrument to you, hoping (if not exactly expecting) that you will make time, in the interests of scientific knowledge, to answer his or her questions with very little expectation or opportunity to ask any in turn. (Again, until recently, you may not even have been promised a summary of the study in return for your cooperation, and even now it is more common to be offered than to actually receive such a summary.) For the positivist, research relationships are asymmetrical, not democratic or egalitarian. For the greater good of science, subjects are expected basically to do as they are told. As we have seen, they are willing, most of the time, to do very boring or apparently dangerous things, having accepted that control of their behavior is in the hands of the knowing scientist. Now and then, they may be paid, but this is still far from democratic.

Coming as supplicants and novices rather than experts, interpretivists have less to trade, in some senses, and no authority (not even that of the scientist's white coat or jargon) with which to coerce or even expect automatic compliance and access. It is a subordinate position they adopt, as suggested earlier, and refusals should be anticipated. I suppose a truly committed interpretivist would even resist the temptation to tell those who might have been studied how much they have

probably lost in denying access. After all, from this perspective, the bulk of the benefits accrue to the researcher, and not to the subjects who are already experts. In fact, the question of reciprocity, or what they have to gain by allowing researchers access is one that vexes interpretive investigators a good deal, and presumably more than is typically so for the positivist. Certainly some students quite commonly hesitate to ask for permission to enter a possible research site, claiming that they don't know what they have to trade. "After all, what do I have to offer them in exchange for their taking the time and making the effort to teach me what it means to live in their world?" Without the mantle of scientific authority, they fear that others will not talk to them.² Though there are other answers, the bottom line usually is that what the interpretivist has to offer is sociability and the attention of a good listener who is sympathetic and anxious to understand the world of the actors. Most of the time this is sufficient, but it is no guarantee.

Trying Out the Water: Wading Versus Diving

Having obtained permission or otherwise secured access to a research site, one must actually enter the scene, though this stage is not easily distinguished from the first. There are at least two types of strategies that may be opted for at this point. Perhaps most commonly, investigators choose to immerse themselves in the culture of those whose world they wish to be allowed to enter, by *easing themselves into it from the edges*, trying to create as few nipples as possible, learning slowly the currents and depths before they begin to try to copy the natives' strokes. This is typically described as a very difficult period of chaotic observation, in which observers strive to keep in mind that they ought to edit nothing, miss nothing, make as few inferences as possible, make as few assumptions as possible, be as full a recording device as possible, and stay quietly in the background until things begin to make some sense. At this point, participation tends to be minimal, and the model approximates what R. Gold describes as the "complete observer" role in the PO context. Others have more colloquially described it as "hanging around." In our extensive discussion of systematic observation, we discussed the kinds of problems that are inherent in this "relationship" to the subjects, and though they may be no more severe the less guidance one has, they can remain invisible since there is no fixed background against which to look for them. These are basically the problems of "the Martian observer," and every participant observer has been extensively

² I am tempted here to speculate that there is a basic difference in orientation to others as exhibited by those students who are attracted to positivistic or interpretive versions of social science. I suspect that those who find positivism attractive are less confident of their interpersonal skills, and hence less willing to rely on them in interaction with others. They gain comfort from being able to use their position and authority as scientists to facilitate continued interaction. Those who are more confident are prepared to rely on interpersonal skills alone to facilitate interaction. Perhaps it is still more basic. Perhaps those who are positivistically inclined not only do not trust their own skills, but also believe that others are in general more suspicious and less forthcoming, more instrumental, less expressive, and more calculating. This might in turn be projection on their part, but we have now ventured far afield.

¹ See *Fieldwork Experience: Qualitative Approaches to Social Research* (W. Shafiq, R. Stebbins, and A. Turowitz, eds., [1980]) for a series of empirical research articles dealing with this and other problematic aspects of the PO experience.

warned to beware the danger of ethnocentrically or egocentrically thinking they know what is going on at this stage of research.

Some rather more adventurous and confident participant observers prefer the more direct second strategy of diving directly into the scene as a participant. And sometimes there is no choice, since a gradual entry is impossible. In essence, this "plunge" occurs when the investigator enters the scene with an established role and identity, whether this involves covert observation as an apparently authentic member or sponsored observation in a role sanctioned by an established actor. In either case and for whatever reason, this tends to be rather more risky, demanding, and stressful, since there is simply a lot you can't know about the "water" until you are actually in it, regardless of the extent of preparatory work (and this is typically minimal).

Let me deal with access by way of a personal anecdote going back to the practicum in participant observation I was fortunate enough to participate in with Howard Becker at Northwestern University. This course was a term's immersion in PO and involved weekly meetings to discuss issues that came up during our exercise. We submitted our field notes to Becker and he gave helpful and extensive feedback. Many students in the practicum used it as the initial fieldwork for subsequent dissertations. Since Becker was then interested in the sociology of occupations and professions, he advised us to pick an occupation or profession about which we knew nothing and study that. There were no other restrictions; some students studied professional photographers, butchers, sound mixers, actors, or police officers. I studied fire fighters.

My Study of Fire Fighters

I chose fire fighters since I knew nothing about them, taking them for granted as I think most people did. They were in the news now and then and they fought fires bravely, rescued cats from trees, had Dalmatian dogs and shiny trucks, and slid down poles in their haste to leave the station quickly when called upon. In elementary school I had visited a fire station and that was the sum total of my experience. When you think about it, it is striking just how little we know about what people do in the world, let alone why and how and with what consequences. Most children have an extraordinarily narrow view of the workaday world, a familiarity with only a handful of professions they see on television or are exposed to in their world, and otherwise are isolated and insulated from the thousands of jobs that people actually do "out there." Yet we call on them to make wise career decisions for their futures in this almost complete ignorance.

My ignorance qualified me to study fire fighters, and there was a more important and mundane factor too that entered the equation. I passed a fire station every day on my way home from the university. Not knowing how to begin, I wrote a letter asking permission to study the fire department. The chief said okay and invited me to come see their records and talk to the people in their head offices. I did and was given a daunting amount to read, out of which I could certainly have done a reasonable historical analysis of the development of the department. I even had

enough access to files to possibly find a few patterns relevant to standards, recruitment, or social-class background. But this was not getting me closer to an insider's point of view, though it was easy, safe, and comfortable, since I was used to desk work and analysis of secondary data.³ After a week or so of indecision, I thanked the chief for his help and left.

On the way home, as usual, I passed by the fire station. It was a warm day and the big double doors were open, the garage lit only at the back by a bare bulb over the desk, and the trucks lined up facing the street. I could see a few people clustered around the desk, and hear some talk and the occasional noise of the radio. I remember walking back and forth in front of the open doors half a dozen times, trying to find the courage to actually walk in, introduce myself, and begin a real PO exercise. Others in the practicum had already begun theirs, and here I was, still pacing the sidewalk and trying to decide what I would say if . . . I don't think I made it through those doors for a few more days, though they were open, but I finally just gave up and decided that the worse that could happen would be that they would ask me to leave.

I walked between the trucks with my heart pounding, stupidly pretending to be admiring their shine. I approached the desk area, where, as I had already observed, several guys regularly sat, talking and drinking coffee and monitoring the dispatcher's radio. I didn't actually know they were monitoring the dispatch until later on. I was prepared to tell them who I was and what I wanted, but nobody asked. I sat on an empty bridge chair on the outskirts of the group, trying not to be too nervous, and tried to pay attention to what was being said and by whom and who was doing what and what the place looked like and what they looked like and who seemed to be addressing what to whom and what was that noise on the radio. Soon an hour or two had vanished, and my mind was frazzled; so I simply got up and left. Nobody had asked me what I was doing there, or taken any notice of my presence or departure. I tried to replay the events of the afternoon as I walked home, and managed very awkwardly to transcribe some portion of what I recalled having observed. It was particularly difficult since I had no names, no way to attach remarks to one man or another, except for the guy at the desk who wore a hat. So they became "the one with the glasses," the "tall skinny guy," the "older black man," and the "younger black man," the "one with the hat," and so on. Names came later.

The next day I returned, a little later on in the day, and with just as much trepidation as the day before. I sat on a chair and watched, said nothing, did nothing else. I realized the station was bigger than the garage; there was a kitchen off to the right, an upstairs, and off to the left there were other rooms. There seemed to be about ten fire fighters, and three or four were usually out front, including the one with the hat, whose name seemed to be Mike. Mike asked me if I wanted a cup of coffee that day, and when I stammered something in reply, he got me one in a plastic foam cup, and then went back to the conversation. That was the only question I was asked.

³ One could argue that it did begin to give me the view of the administrators, but this was not my object.

Again I went home and did my best to replay the afternoon. After a few days I found it quite incredible how good I had become at observing and recording in my mind and then "replaying" the whole thing into the typewriter. (I don't expect you to take my word for this, but if you give it a try, it is truly remarkable.) I found it easy to write ten or fifteen pages for every hour or so of observation. And this was pure exploration. I still had only one name and little idea of what I was writing down. Of course, I missed a huge amount of material and completely misconstrued much else. I am sure (in hindsight); but I thought I was getting down as complete a record as I was capable of observing. After all, in PO the investigator is the instrument, and the instrument is supposed to get honed as you get more experienced.

After a few days, during which I dropped in for about an hour or two at a time, Mike asked me my name and introduced the guys around the table. He eased me into the desultory political conversation, and we exchanged goodbyes when I left. I wondered what they thought I was doing there, since nobody else seemed to "drop in" like I did, but it didn't seem to matter. I had an "in," and I was getting data without producing reactivity or anything equally awful. My notes got longer, but the men had acquired names and were beginning to acquire identities and relationships too. After Mike's overture and his subsequent introduction of me by name to a few others, I was welcomed each time I stopped by, though nobody knew what I was doing there. It's not that I wasn't ready and prepared to tell them, as I've said, and eventually Mike asked and I told him I was a graduate student who wanted to learn what it was like to be a fire fighter. I don't know if he told anybody else or not, or whether they just picked up some information about me as I picked up personal information about them. Mike became my main contact, and no one else ever asked me why I was there.

Later on, I discovered that my experience of access was far from atypical in any respect. Even Everett Hughes (1960, p. iv) admits to walking around the block, getting up his courage, and many researchers recount similar tales of anxiety. Many also tell essentially the same story about elaborate "cover stories" that turn out to be unnecessary. William F. Whyte, author of *Street Corner Society* (1943), perhaps the first classic participant observation study, describes his experience in these terms (Whyte, as quoted in *Modern Sociology*, edited by P. Worsley et al., 1970, p. 103):

I soon found that people were developing their own explanation about me: I was writing a book about Cornerville. This might seem entirely too vague an explanation, and yet it sufficed. I found that my acceptance in the district depended on the personal relationships I developed far more than upon any explanations I might give. . . . If I was all right, then my project was all right; if I was no good, then no amount of explanation could convince them that the book was a good idea.

ACQUIRING A ROLE

Let us return to our novice, now in the water and probably convinced that she is in over her head—trying desperately to keep it above water as the culture threatens to overwhelm her in its mysteries, its apparent contradictions, the understandings

that allow members to share a universe of discourse closed to her. How does one actually proceed? There may be nobody around with a lifeline, although this is one good reason for having a sponsor or key informant. In the absence of such aid, the literature suggests that talented people tread water and others flounder. But then, what is the real risk of drowning? Fortunately, the worst that can happen most of the time is that you embarrass yourself and leave the field. While there are instances where failure can be life threatening, they are fairly rare. With any kind of supervision, those who cannot tread will be advised to avoid the truly dangerous circumstances that are foreseeable. Social scientists are not undercover cops, after all.

If the Martian or complete observer role is temporary and inadequate, since it fails utterly to seek, let alone get at, the definition of the situation as understood by the insider, how does one move on to adopt a more useful role, and what does this look like? Let me return to the fire station.

I might have been content to sit around the desk and mostly listen and offer my two-cents worth when asked for it; but situations are flexible, and I was not allowed to remain quite so uncommitted for long. Mike had taken the initiative and I had responded in a friendly manner. In return for letting me visit the public part of their station all anyone had asked of me so far was an occasional question or two about education, or about Canada once they learned I was Canadian. One late afternoon, I was packing up to go when another of the men asked if I wanted to stay for dinner since they had more than enough, and as he recalled quite correctly, graduate students were always hungry and usually broke. I was pleased to agree, since this was a great opportunity to get backstage, closer to their private world in the station, and since I was both broke and hungry. I had also gathered that one of the ways a couple of them liked to spend their time was cooking; and since the city paid for the supplies, they became very good cooks indeed. Dinner was as good as it had smelled, and eating with them in the dining area opened up a whole new set of questions, and provided me with improved access to several of the men as well. I stayed well into the evening, and learned that hobbies seemed very important to them. Two cooked; a couple spent lots of time building models; one built and rebuilt radios; Mike read and was active in the movement to unionize the fire fighters' locals around the Chicago area.

I was invited for dinner fairly often after that, and I seemed to drop by late in the afternoon more often than any other time, strangely enough. They seemed to enjoy talking to me, though Mike was still the only one I spent much time talking to alone. He learned I knew a little about unions and organizations and was anxious to learn whatever he could use to promote the cause of the fire fighters' union.

One afternoon, a call finally came in while I was there. The men were in the trucks and out the doors in a few seconds, leaving me behind standing still, and one of the "cooks" casually asked me to "stir the soup 'til we're back" as they pulled away. Thus I became what I like to think of as a "sous chef." The soup was delicious.

As fall got colder, the big double doors of the garage were closed and I spoke with the men more often by themselves. These were never interviews. They were just conversations about what it was like being a fire fighter. They were quite can-

reaction

did and sometimes surprising. I thought I was learning a lot. The next time a call came when I was there, another element was added to my role. This time as they left, someone asked me to close the doors behind the trucks. "Hit the Red button for 'close' and the Green one when we're back for 'open,'" he said as he leaned over the side to hit the button himself to open the big doors as they left. My role was growing incrementally as I was taking part more fully in the life of the station. In my seminar, other students were being forced to make moral decisions concerning their roles among delinquent gangs and drug users, but I was only stirring the soup and opening and closing the garage doors. Even so, I did eventually have to withdraw from the role they were creating for me, though there was no danger of my "going native" since I could not. The captain, Mike, and I had become friends, and he asked me to accompany him on a recruiting trip to some outlying suburbs where he wanted me to help him sell the fire fighters on the advantages of unionization. Here I felt that I had to draw the line, and I told him I didn't think I could do that for him. He immediately backed off and it never came up again. Granted, it was not a big moral issue—I had not been invited to engage in criminal acts as several of my classmates had—but I saw it as an ethical issue nonetheless. Fortunately, Mike didn't feel threatened, and he wasn't testing me. Some of my classmates had much less pleasant experiences.

My main point here is that you will develop a role over time, and it will change over time. You have some power to control the direction of change and you have some power to control the nature of the role, but you are negotiating it with others in whom you are interested, and it is a role in their lives and in their environment. Being a participant observer is not a role they recognize, and they will transform you into something more comfortable for them, whatever your original role. Often you may get by as a hanger-on, as a groupy, or as a counselor. Sometimes you may be treated as a novice or convert. But the role is not firm and established. It evolves, and it requires your honest participation. Gold's discussion of PO roles is useful but too static. You will likely play all four of his roles at various points in time and move back and forth along this continuum over the course of your extended project. Most often, as described, you will begin as a complete observer, and then move into a more participatory role until you decide that complete participation is too seductive and too close, and then you will hopefully be able to back off along the continuum again. This movement may well occur more than once and is not unidirectional. The role evolves over time like any other role in a social context. The best advice I can offer is simply to be conscious of it, and don't lose control of it.

I don't think it is any easier for a participant observer to carry off a fake than it is for anyone else as you get to know them well over a reasonable span of time. Indeed, this is why reactivity is thought to decline over time. The situation, the demands of other parties, the routine work connections all force your "subjects" to act as they normally would, even if they would like to pretend to be otherwise and can even do so for a short time. The boss can't keep up the pretense of being a saint all the time you are hanging around. There are too many real-world responsibilities, too many demands for the real role performance. Neither can you continue to pretend to be something quite foreign to who you are for a long time. Most

participant observation studies that succeed have involved long periods of time, and a high level of commitment by the investigator within the social world under study. You have to live there, not pretend to be part of it. There will be responsibilities that accrue to the role, real responsibilities that are important to the people in that world and are important to you. Getting "into" a universe of discourse, into a way of life, means making yourself vulnerable to it, opening yourself up to it, taking it on its own terms, and dealing with its strengths, frailties, and horrors. It means learning a great deal about who you are and what you are like. And it is not always as pleasant as being asked to stay for dinner. You make friends; you try not to make enemies. At the end, you will have to sever ties that are meaningful to you and to them, for they are hardly ever sustainable outside the peculiar context of the double life you've led. This too will teach you valuable lessons about yourself. Whatever else can be said of PO it is surely true that no other method teaches you nearly as much about who you are even as it teaches you about other people. It is this feature, I am convinced, that is a large part of what those who love PO find so seductive. It puts you in touch with yourself. It is educational in the most real sense. I am equally convinced that it is this feature that scares the hell out of those who do not like PO. But back to the fire station.

I went to my weekly seminar and listened to the experiences of others and told them what I thought I saw at the fire department. Every day I stopped by the station for an hour or two or more at various times of day, since we had been advised that if you wanted to understand the insider's view, you certainly had to live through the entire routine or schedule they lived and appreciate its pace, its stresses, its ups, and its downs. Ideally this means you ought to sample in some manner from all twenty-four hours of the day, and it certainly means that you cannot seriously think that you have understood someone's workaday world by visiting them at lunch hour a dozen times. So I visited the station mornings, afternoons, evenings, weekdays, and weekends for various periods up to eventually about fourteen hours at a stretch. I was not able to actually stay with them for a complete shift, since a shift was thirty-six hours and I was not able to sleep in the station. Nor was I allowed to ride on the trucks with them, of course. So my view of their lives in the station was still an outsider's view, but a privileged one, informed by what they were willing to tell me and by what I was able to establish for myself.

From Ethnography to Sociology

Even assuming that one could do so (and there is every reason to doubt this assumption), it is my view that re-creating the lived experience of the actors is not the end point of sociology; it is the beginning. Yet this is the purpose and goal of ethnography, which PO stopped at this point could be called. The collection of such holistic and "thickly descriptive" data is just that—the collection of data to be analyzed and made meaningful and useful by the application to them of the sociological imagination. The transformation of such data into meaningful sociology is the tougher part, and few authors are very clear about how this is to take place. The general assumption that a suitable problem will emerge from the actors'

viewpoint and be of interest to sociologists and recognizably so is highly doubtful, as any of my methods students can tell you. In their exercises, they typically begin without explicit theory or literature review to guide their observations and they attempt to develop sociology along the way, in conformity to the stricture to "beware of premature closure." This they find very difficult, since the description is hard enough and the sociology harder. Certainly it is commendable to try to describe the "lived experience of the actor," but it is only a starting point. Denzin hints at the use of *second-order concepts* in the transformation of this into sociology. These are terms drawn from the sociological literature and not in use among the actors. Their terms are *first-order concepts*. They rarely speak of cohesion, socialization, or class consciousness. They speak of "being buddies," of teaching and learning, guiding and leading, and "togetherness." This transformation of their experience into second-order terms that are more generic and analytical is very problematic(!) and must be theory guided. There is simply no other way. It is distinctly possible, as well, that a good deal will be lost between the first- and second-order concept as we have suggested elsewhere. In fact, though, even the very first chaotic observations are theory driven, in the sense that one has to choose what to see.

On the Use of Flotation Devices

The "beware of premature closure" rule does would-be participant observers some harm, in my view, since they are unable to see what they ought to be focusing on and they commonly lose sight of the forest for the trees. It is proper and useful to warn them of the dangers of editing the material presented to them in terms of their own ethnocentric standards and meanings. But advising them to learn nothing in advance and to try to write down everything is simply impractical and does little to solve the problem. Surely we would be better off to simply advise novices to be their own best critics, to constantly second-guess, to be skeptical, to go slowly, in the knowledge that much of what they assume to be true will not be, and to strive hard to remain open to the serendipitous discovery of meaning in unlooked-for places, or of different meaning to that they had expected.

We must use the data to illuminate sociological issues—the nature of conflict and cooperation, the process of communication, of socialization, the nature of class consciousness and its internalization and exhibition. Even more, we must use the data to generate insights about these processes and conditions; for this is the task of sociology, and it includes, and in my view, transcends ethnography and the ethnographic impulse. Unfortunately, it is fairly easy to say this and very hard to say exactly how to put it into practice. In effect, I have insisted that sociologists try to utilize their sociological imaginations. This is not easy to teach. About all I can suggest is that students read widely in the discipline, and that they practice trying to conceptualize everyday events and situations in a sociological manner. Analyze (to yourself!) your family, your friends, your workplace, your newspaper, and your local political situation in sociological terms. See if you can do so in an insightful and useful manner. Use what you know of theory and methods; hone your skills,

conceptual and methodological, by practice. I know no other way to gain competence in anything.

Let me return to the fire station once again to illustrate the transformation of observational notes into sociology, after a fashion. The months I did PO in the fire station were mostly spent in exploring a world that was new and foreign to me. Still, by the time I had to write up a paper on what I had learned, I had managed to identify a few themes that I could have followed up more had I gone on to the next phase of systematic hypothesis testing by rigorous negative case analysis or constant comparison. As it was, I was guided primarily by the advice that if you have been writing down everything, and you subsequently think you have hit upon a pattern, you can look back through earlier notes and see if the presumed pattern was there even before you recognized it. But let me illustrate, since this sounds more complex than it is.

One major theme struck me as I pored over my old notes, in light of my current level of understanding. It concerned the nature of the job and its stresses. When I began, I thought fire fighters fought fires. That is, I'd have said it was their job to fight fires. I quickly discovered that this did not appear to be the case. At least, they rarely worked if that was their job! It dawned on me that it would be more accurate to conceptualize their job as *being prepared* to fight fires, and this they worked at all the time, and hard. This insight, such as it was, first struck me after some time on the scene, when the calls were rare and there was lots of apparently slack time. One guy quit during this time, and I had spoken to him before he left. He told me that it wasn't the fighting fires that scared him off. It wasn't even the boredom in between. It was the tension of having to be ready to risk his life at any moment, of never being able to start anything that he couldn't leave at a moment's notice, of never knowing for sure that he'd see his family again when his shift was over. I had written down his account and spoke to others about his departure and his reasons; but then something else had happened and I'd left this issue. Some weeks later, it occurred to me that all the men left had hobbies to occupy their time, and he had not. Further, all the hobbies were ones that could be set aside quickly with no greater damage than the risk of burning the soup. Other things began to click into place, began to make sense, given what I now knew about the nature of the job, and looking back over things they had said and done earlier, and that I had recorded but not made any effort to, or had not been able to understand.

A) In other words the participant observation experience itself is *reflexive*. One makes sense out of the world, and in light of the sense one makes from it, one reinterprets the whole previous picture, casting it in a new light—that of the new sense. We say this and do this all the time in our everyday world, and it shouldn't surprise us that we do it too as social scientists. We constitute the world; we create the order in it, and then we use that order to illuminate aspects of it that we hadn't seen before. Whether these are "really" there or not hardly matters. We are reflexive and cannot operate in any other way. All of us do this. Every time we say "Now I understand what was really going on then" and we proceed to reconfigure our past explanations of some behavior of interest to us in light of our present new understanding, we are doing this. I became reflexive as I came to understand the "true" nature of the job as they had been trying to tell me but I had not been able to hear

earlier. Was my earlier understanding invalid or less valid, or more naive, or simply different?

They had told me there was some concern in the department for their physical fitness, and this station had been built with a handball court included in the back for their exercise. At one point, it struck me that I had never seen anybody doing any exercise other than playing pool upstairs, and I commented on this. They laughed and said that whoever voted the funds for the handball court had never had to dash outside from it directly onto the back of a fire truck to race to a fire through a Chicago winter's night. It slowly dawned on me from such observations and anecdotes that they were not waiting to do their job, and keeping in shape to do it while they waited. They were doing their job every shift, and doing their job made it impossible for them to do some other things that would have been in the way of doing it properly.

Finally, the many references to boredom and stress that had come up from the beginning and had simply been left sitting now took on new meaning as I came to appreciate the mental strains that must be involved in being prepared at all times, but rarely being called upon. I began to appreciate what seemed to be their little bit guilty ambivalence about the preventive programs they ran for schools, businesses, and home owners. The better they were at preventing fires, the less often they had to actually go out and fight them. The less often they were actually seen fighting fires, the less valuable the public thought they were, and the more trouble they would have holding salaries and prestige commensurate with the police department next door who always seemed to be actively fighting crime. Ah, the troubles of a peacetime army, I thought. With this *comparative* thought, a host of new ideas and hypotheses occurred to me, involving explicit comparison with other professions and occupations that could be purposefully selected as contrasting or comparative cases that would illuminate the world of fire fighters. Had I continued, I might have interviewed shift workers of all kinds, particularly nurses, paramedics, peacetime soldiers, security guards, emergency room doctors, and police officers. I believe that I had come to see the patterns that included fire fighters but transcended their experience. In short, I began to do sociology.

YOUR LIPS ARE TURNING BLUE: IT'S TIME TO COME OUT OF THE WATER

I did PO in the station for several months. I got to know most of the men fairly well and Mike quite well, and not only in the station. I was invited to dinner with his family now and then when he was off shift, and this opened whole new perspectives. We became friends, such that for a few years after I left the study, we exchanged Christmas cards, and I followed his career with the union when he left the station where I'd met him. Involvement in others' lives is what PO is, and leaving the field means breaking off relationships that may have considerable meaning to all concerned. It can be painful if a lot has been invested, and this is quite often the case. There are those who would advise the would-be participant observer to try

to maintain a certain psychological distance from the subjects of study. Now and then this may be possible, but since the primary goal is to enter their world as they experience it, to enter their minds and understand their experience as they do, the maintenance of distance is basically counterproductive. It would seem better advice to simply tell people that you do not foresee a permanent relationship with them, but that you will face that future leave-taking when it comes and act in the interim as if it will not. After all, all our relationships are lived within these boundaries, and those who try to withhold commitment are commonly held to deprive themselves of what they might have potentially gained rather more than they have protected themselves and others from what might have been lost. In any event, the nature of the relationship is not up to the researcher exclusively, and again what we know and apply in our "normal" lives seems applicable to the PO situation. The relationships in which participant observers are involved are negotiated on a continuing basis with their co-interactants. Both you and they have a good deal to say about the nature of the relationship, and about whether and how it should be terminated.

THE STATUS OF CLAIMS TO KNOW BASED ON THE METHOD OF PARTICIPANT OBSERVATION

As might have been expected, there is a range of positions taken by those who practice and in general are sympathetic to participant observation. It is not surprising to find a range as well in terms of the kinds of defenses they make of the method. As noted, least contentious is the position that PO is a useful exploratory tool. There are few again who would disagree with the proposition that PO is an excellent device for gathering detailed descriptive data in a field setting. Beyond this, things get much more complicated. Does the reactivity or the lack of reliability of PO prevent generalizability of findings beyond the research setting? Do reactivity, demand characteristics, the same lack of reliability and the absence of a standardized method effectively limit the credibility of claims to internal validity? Clearly, the defense required depends on one's assessment of the strengths and weaknesses and the legitimate uses and liabilities of the method as used.

Eleanor Bowen raises a number of thorny issues about participant observation in her book *Return to Laughter* (1964). Central among these is the question of how one knows that one is finally observing "their" reality successfully. This is a question first of the internal validity of the investigator's experience, and subsequently of the reliability and generalizability of the account so provided. She points out that over time the view of the investigator evolves or changes, and asks what confidence we can have that the view frozen at any single point in time is correct, since it might have continued to change had the investigator remained longer in the field. She raises the disturbing possibility that some possibly ascribed characteristic of the investigator may limit access to a whole arena that is important to the subjects but off limits or taboo to females, for example, or people under 60 years of age. She likens the process to that of peeling an onion, and raises the question of how one

can know when there is still another layer to be removed before one reaches the core.

Howard S. Becker takes a rather modest and conservative line on these issues. In dealing with the question of how one knows when one has done enough fieldwork, he suggests that *redundancy* provides a reasonable measure of confidence. That is, one stops when time spent in the field only confirms further observations already made and provides no new insights. This is a relatively simple rule of thumb, but an insightful and useful one. Becker suggests in some relatively early papers that PO could be transformed into quantitative analysis, after all, and that there are essentially quantitative answers to some of these questions of internal validity. One can code one's field notes and perform quantitative analysis in support of some assertions about the frequency with which some answers were given or events occurred, for instance. That is, "frequently," "sometimes," and "rarely" are terms that could be quantified if one wanted to do so in response to a critic's need for supporting numbers. Similarly, one could develop a sort of "credibility hierarchy" in terms of which one might locate and assess provisionally the claims made by respondents. Becker distinguishes between claims that are offered by the respondents voluntarily versus those that are solicited. He also distinguishes between those made in public and in private. This is really a suggestion for the operationalization of what Deutscher calls one element of his "double screen"—the element of discounting the source. Not all claims are equally credible, and one can place more faith in those offered unsolicited and made both in private and public than those that seem to be made for the audience when the respondent has been "put on the spot." Becker's treatment of reactivity as an issue concentrates as I have suggested on the difficulty of maintaining a false performance in the field setting for any appreciable length of time when others' demands are paramount. So Becker is willing to accept the basic need to address the issues of credibility, validity, reactivity, and generalizability; and he offers his usual eminently commonsense suggestions for ways of strengthening PO's ability to produce a credible basis for a claim to knowledge.

There are more radical positions in the interpretive literature, and these frequently begin by resisting the claim that PO ought to have to account for itself in terms of the criteria set up by the positivists. Apparently taking to heart the old adage that the best defense is a good offense, some defenders/apologists/disciples simply rail against positivism's infatuation with reliability and quantification, taking refuge in the final defense that "after all, I was there and you were not. And I wouldn't lie." Such a defense of the validity of direct observation carries little enough weight under other circumstances and fares no better under these. Many such treatments begin and end by simply stating that the purpose of PO is to enter the universe of discourse—the head space—of the actors. Where the purpose of the method is such understanding or *verstehen*, the only criticism that is judged meaningful and worthy of response is one that charges that PO fails to produce *verstehen*. Most commonly, the defense of this claim is still a version of that provided by Glaser and Strauss (1965, p. 7): "The field worker knows that he knows, not only because he's there in the field and because of his careful verification of hypotheses, but because 'in his bones' he feels the worth of his final analysis."

Beyond this not very helpful defense Phillips, Bloor, and others have a number of concrete suggestions that together suggest that PO be evaluated by other criteria, more compatible with its underlying interpretive premises. Phillips (1971) suggests that a good account can be tested by using it as a set of instructions to a naive confederate, by virtue of which they should be able to "pass" or at least function adequately in an environment of which they had no prior knowledge. This is very much in line with the ethnomethodological principle of putting yourself on the line and making "getting by" a practical matter of survival for the investigator. It is also a simple adaptation of what has been archetypically true for many anthropological investigators dropped into hostile societies with no preparation. A somewhat less romantic and risky method for judging credibility has been suggested by Bloor (1983) and is referred to as "member or host validation." The idea is simply that one ought to be able to test one's formulations against the expert knowledge of the members, and it is the extent of their willingness to concur with your characterization that measures its accuracy and credibility.

Jack Katz's essay in Emerson's excellent collection *Contemporary Field Research* (1983) exemplifies a still more radical approach. It seems to me that what is advocated by Katz is a logical extension of the idea of analytic induction and the systematic search for negative cases that forces one to revise one's claims to have understood. The logic of analytic induction transcends itself since one always oversteps one's data in striving to make universal propositions. Though of course one's first responsibility is to carry this procedure reasonably far oneself by systematically seeking out the most appropriate negative cases that will test one's propositions, ultimately any investigator must rely on others' experience for help in finding these limiting cases, or establishing that the proposition does in fact hold for all instances. Similarly, claims of both internal and external validity rest on repeated attempts to find the limiting cases, made by the investigator and by the audience that reads the material in question. In this version of interpretive work, the obligation of the critic is of course to be constructive and open-minded; but the role is far more active than it is in the positivistic version. The critic may agree or disagree with the research claims of validity, reliability, and so on, not only the basis of their demonstration in the study or data but also because they do or do not accord with the experience of the critic. The critic's life experience is actively called upon to confer validity beyond the sample and reliability beyond the (single) judge. This insistence on the active and legitimate role of the critic's experience is the logical extension of analytic induction. Indeed, it is analytic induction taken as a methodological maxim. Seek out the negative case. State your proposition as a universal and let negative cases revise it until there are no more of them.

These progressively more radical solutions to some standard methodological issues and positivistic critiques of PO suggest that for some adherents of interpretive research, it is quite clear that work can be assessed as to its validity without excessive concern with its reliability. In other words, in distinction to the position of the positivistically inclined, reliability is simply not the *sine qua non* of competent and valid research, and questions of validity are not moot if a strong case for reliability cannot be made. One can and we regularly do assess knowledge claims as to their validity or "truth value" primarily in terms of their plausibility, their

ability to handle rivals, their convergent validity, and their simplicity. And PO claims can be assessed quite adequately by such commonsense criteria.

ISSUES IN THE CONDUCT OF PO

These may be conceived of as problems or not, severe or not, peculiar to PO or not. They are concerns. How serious they are and how, and indeed if, they are to be dealt with in any particular piece of work is up to you.

(1) *Reliability* PO does not typically provide us with data that are demonstrably reliable either across investigators or over time. With few exceptions, PO is practiced by individual social scientists rather than teams. Some critics have wondered at the validity of Castaneda's reports on Don Juan, and some have even doubted the credibility of William Foot Whyte's version of *Street Corner Society*, claiming that the version he produced was a unique function of his and Doc's personalities, and could not have been produced by other investigators. Note how the absence of evidence on reliability is used to challenge validity claims in this instance.

(2) *Reactivity* PO does involve reactivity. How much, for how long, and with what effects we must assess for ourselves. Participant observers claim that all methods except for unobtrusive measures are reactive, and that PO may well be less seriously distorted by this feature than experiments or surveys. Some interpretive workers are content to note that in all methods, the investigator as instrument surely produces reactivity, and this is best thought of as simply part of the research topic.

(3) *The limitations of the role taken* The participant observer does acquire a role of some sort within the world of the actors. He or she is not omniscient, and the role implies a vantage point from which some things are seen and others are not. It is possible for the participant observer as mental patient to find out what it is like to be a mental patient in the ward or hospital under study. It is very difficult for the participant observer *qua* inmate to ask to interview the chief medical officer. Similarly, the janitor can gather information about what goes on on the floor of the assembly line but not in the corporate offices or boardroom, and the inmate does not often have easy access to the warden even in the movies.

(4) *"Going native"* The role of participant observer requires that one walk a tightrope in a high wind, and there is great risk that one may lose one's balance at any time, falling wholly into the role of the natives or that of the Martian observer.

(5) *Participant observation is essentially a fairly passive methodology* That is, it is not usually the case that the participant observer will wish to intervene directly in the lives of those being studied. He or she will therefore have to wait

patiently for events to occur. The participant observer records and interprets—you are not administering an instrument; you are that instrument.

(6) *Ethical dilemmas* The participant observer may well come to know things that others consider private or privileged information. Indeed, some of these things are often imparted to the social scientist in such a setting as a test of loyalties. In probably every setting there are rule violations that should result in the dismissal of employees or the sanctioning of interactants at least. Knowledge of these items is power, and with power comes the responsibility to exercise it with discretion, care, and humanity.

(7) *Selective perception* We have already referred to this problem as it is a function of vantage point, which is in turn a function of the role taken by the participant observer. This may be a serious problem, and it may be of little apparent consequence. In any event, it is not a problem peculiar to PO, though it is far more often dealt with in this context than in that of experiments or surveys, for example. Essentially, it is a "problem" affecting both validity and reliability of all research.

(8) *Access* As we have discussed, this can be the limiting problem, since without access the study simply cannot be done unless it is done covertly. Covert PO raises serious ethical problems, and is probably not worth it, unless one can be convinced that the gains far outweigh the risks.

(9) *Leaving the field* Withdrawal is not always easy and is always painful. Having built a relationship with a group, and spent a great deal of time and effort becoming one of them, it is not uncommon for the participant observer to feel as though he or she was losing close friends and meaningful relationships when the project draws to a close. Some of us handle these leave-takings more successfully than others, but PO is an intense interpersonal relationship and leaving friends is always difficult.

There are also some features usually seen as positive characteristics of PO. Again, these are not always present, they are always variable, and they may or may not be regarded as strengths.

(1) *PO occurs in the natural setting.* This suggests that what is sometimes called its "ecological validity" is high, or that if setting and context strongly affect behavior, then presumably one would want to observe behavior in as natural a setting as possible, rather than the artificial setting of laboratory, living room, or office.

(2) *Using PO, one can observe emotional and physical as well as verbal responses.* This makes it a *more subtle and sensitive* method than many. It has internal triangulation built in, with all this implies for maximizing validity.

(3) PO is a *longitudinal* method by which a very large amount of data is amassed over an extended period of time. This means that we can pay attention to change over time in ways that are impossible for other methods that are cross-sectional in nature. In turn, there are those who would argue that the temporal dimension allows one to make stronger causal arguments than can be made from cross-sectional data bases, since the question of causal ordering is more easily handled when an observer sees events occur in sequence.

(4) The level of rapport characteristic of PO allows for considerably *more and deeper probing* than is common in survey research. In addition, since PO is a longitudinal method, one can follow up leads that develop over a period of time, seeking expansion or corroboration where these are indicated. Others have argued that rapport inhibits validity since virtually by definition the flip side of rapport is reactivity.

(5) PO can contribute to *theory building* more directly than many other techniques, since it is not only a data-collection tool but one that, if used in the context of constant comparison and analytic induction, is designed to produce theoretical insight.

(6) PO is one of the qualitative methods that treats human beings as actors who act intentionally and sensibly within their own parameters. It is essentially humane and respectful of the people who are its subjects, and rarely manipulates, deceives, disregards, or minimizes them and their concerns.⁴

(7) PO is capable of letting the investigator learn more about himself or herself than any other method. Indeed, if it is an art form, you will quickly learn your status as artist. It produces a humility and a respect for others, and grows from a genuine affection for others. Those who do it well are in a very real sense better human beings for the experience. Participating in the worlds of others is a basic way of enlarging your personality and enriching your life.

⁴ I have stressed that there is a wide range of positions even among those who are positivistically inclined. Those who are positivistically inclined may use it as a data-collection technique only; they may use it covertly and without taking seriously the injunction to join in the universe of discourse of the actors.