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Participant Observation

Participant observation fieldwork is the foundation of cultural anthropology. It involves getting close to people and making them feel comfortable with your presence so that you can observe and record information about their lives. If this sounds a bit blunt, I mean it to come out that way. Only by confronting the truth about participant observation—that it involves deception and impression management—can we hope to conduct ourselves ethically in fieldwork. Much more about this later.

Participant observation is both a humanistic method and a scientific one. It produces the kind of experiential knowledge that lets you talk convincingly from the gut, about what it feels like to plant a garden in the high Andes or dance all night in a street rave in Seattle.

It also produces effective, positivistic knowledge—the kind that moves the levers of the world. Nancy Scheper-Hughes developed a theory, based on participant observation, that accounts for the tragedy of infant mortality in northeast Brazil and the direct involvement of mothers in their infants' deaths (1992). And participant observation is used in product development and other direct applications research. When credit-card readers were installed on gasoline pumps in the early 1990s, consumers avoided using the technology. John Lowe and a team of participant observers figured out why (Solomon 1993). Brigitte Jordan and her team of ethnographers at Xerox Corporation determined the information flow and the hierarchy of interactions in the operations room of a major airline at a metropolitan airport (Jordan 1992b).

Romancing the Methods

It used to be that the skills for doing fieldwork were mysterious, unteachable, something you just learned out there in the field. In the 1930s, John Whiting and some of his fellow anthropology students at Yale University asked their professor, Leslie Spier, for a seminar on methods. "This was a subject to discuss casually at breakfast," Whiting recalled Spier telling him, not something worthy of a seminar (Whiting 1982:156). Tell this story to seasoned anthropologists at a convention, and it's a good bet they'll come back with a story of their own just like it.

It's fine for anthropologists to romanticize fieldwork—vulcanologists and oceanographers do it, too, by the way—particularly about fieldwork in places that take several days to get to, where the local language has no literary tradition, and where the chances are nontrivial of coming down with a serious illness. Research really is harder to do in some places than in others. But the fact is, anthropologists are as likely these days to study army platoons (Killworth 1997), or consumer behavior (Sherry 1995), or gay culture in major cities (Herdt 1992; Murray 1992), or life on the mean streets of big cities (Bourgeois 1995; Fleisher 1998) as they are to study isolated tribal peoples. And while participant observation in small, isolated communities has some special characteristics, the techniques and skills that are required seem to me to be pretty much the same everywhere.

What Is Participant Observation?

Participant observation usually involves fieldwork, but not all fieldwork is participant observation. Goldberg et al. (1994) interviewed 206 prostitutes and collected saliva specimens (to test for HIV and for drug use) during 53 nights

of fieldwork in Glasgow's red-light district. This was serious fieldwork, but hardly participant observation.

So much for what participant observation isn't. Here's what it is: Participant observation is one of those **strategic methods** I talked about in Chapter 1—like experiments, surveys, or archival research. It puts you where the action is and lets you collect data . . . any kind of data you want, narratives or numbers. It has been used for generations by positivists and interpretivists alike.

A lot of the data collected by participant observers are qualitative: field notes taken about things you see and hear in natural settings; photographs of the content of people's houses; audiotapes of people telling folk tales; videotapes of people making canoes, getting married, having an argument; transcriptions of taped, open-ended interviews.

But lots of data collected by participant observers are quantitative and are based on methods like direct observation, questionnaires, and pile sorts. Whether you consider yourself an interpretivist or a positivist, participant observation gets you in the door so you can collect life histories, attend rituals, and talk to people about sensitive topics.

Participant observation involves going out and staying out, learning a new language (or a new dialect of a language you already know), and experiencing the lives of the people you are studying as much as you can. Participant observation is about stalking culture in the wild—establishing rapport and learning to act so that people go about their business as usual when you show up. If you are a successful participant observer, you will know when to laugh at what people think is funny; and when people laugh at what you say, it will be because you *meant* it to be a joke.

Participant observation involves immersing yourself in a culture and learning to remove yourself every day from that immersion so you can intellectualize what you've seen and heard, put it into perspective, and write about it convincingly. When it's done right, participant observation turns field workers into instruments of data collection and data analysis.

The implication is that *better* field workers are *better* data collectors and *better* data analyzers. And the implication of *that* is that participant observation is not an attitude or an epistemological commitment or a way of life. It's a craft. As with all crafts, becoming a skilled artisan at participant observation takes practice.

Some Background and History

Bronislaw Malinowski (1884–1942) didn't invent participant observation, but he is widely credited with developing it as a serious method of social

research. A British social anthropologist (born in Poland), Malinowski went out to study the people of the Trobriand Islands in the Indian Ocean just before World War I. At the time, the Trobriand Islands were a German possession, so when the war broke out, Malinowski was interned and could not return to England for three years.

He made the best of the situation, though. Here is Malinowski describing his methods:

Soon after I had established myself in Omarkana, Trobriand Islands, I began to take part, in a way, in the village life, to look forward to the important or festive events, to take personal interest in the gossip and the developments of the village occurrences; to wake up every morning to a new day, presenting itself to me more or less as it does to the natives. . . . As I went on my morning walk through the village, I could see intimate details of family life, of toilet, cooking, taking of meals; I could see the arrangements for the day's work, people starting on their errands, or groups of men and women busy at some manufacturing tasks.

Quarrels, jokes, family scenes, events usually trivial, sometimes dramatic but always significant, form the atmosphere of my daily life, as well as of theirs. It must be remembered that the natives saw me constantly every day, they ceased to be interested or alarmed, or made self-conscious by my presence, and I ceased to be a disturbing element in the tribal life which I was to study, altering it by my very approach, as always happens with a newcomer to every savage community. In fact, as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as a part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco. (1961 [1922]:7–8)

Ignore the patronizing rhetoric about the "savage community" and "donations of tobacco." (I've learned to live with this part of our history in anthropology. Knowing that all of us, in every age, look quaint to those who come later, has made it easier.) Focus instead on the amazing, progressive (for that time) method that Malinowski advocated: Spend lots and lots of time in studying a culture, learn the language, hang out, do all the everyday things that everyone else does, become inconspicuous by sheer tenaciousness, and stay aware of what's really going on. Apart from the colonialist rhetoric, Malinowski's discussion of participant observation is as resonant today as it was three-quarters of a century ago.

By the time Malinowski went to the Trobriands, *Notes and Queries on Anthropology*—the fieldwork manual produced by the Royal Anthropological Institute (RAI) of Great Britain and Ireland—was in its fourth edition. The first edition came out in 1874 and the last edition (the sixth) was reprinted five times until 1971.

That final edition of *Notes and Queries* is still must reading for anyone interested in learning about anthropological field methods. Once again, ignore the fragments of paternalistic colonialism—"a sporting rifle and a shotgun are . . . of great assistance in many districts where the natives may welcome extra meat in the shape of game killed by their visitor" (Royal Anthropological Institute 1951:29)—and *Notes and Queries* is full of useful, late-model advice about how to conduct a census, how to handle photographic negatives in the field, and what questions to ask about sexual orientation, infanticide, food production, warfare, art . . . The book is just a treasure.

We make the most consistent use of participant observation in anthropology, but the method has very, very deep roots in sociology. Beatrice Webb was doing participant observation—complete with note taking and informant interviewing—in the 1880s and she wrote trenchantly about the method in her 1926 memoir (Webb 1926). Just about then, the long tradition in sociology of urban ethnography—the "Chicago School"—began at the University of Chicago under the direction of Robert Park and Ernest Burgess (see Park et al. 1925). One of Park's students was his son-in-law, Robert Redfield, the anthropologist who pioneered community studies in Mexico.

Just back from lengthy fieldwork with Aborigine peoples in Australia, another young anthropologist, William Lloyd Warner, was also influenced by Park. Warner launched one of the most famous American community-study projects of all time, the Yankee City series (Warner and Hunt 1941; Warner 1963). (Yankee City was the pseudonym for Newburyport, Massachusetts.) In 1929, sociologists Robert and Helen Lynd published the first of many ethnographies about Middletown a pseudonym for Muncie, Indiana).

Some of the classic ethnographies that came out of the early Chicago School include Harvey Zorbaugh's *The Gold Coast and the Slum* (1929) and Clifford Shaw's *The Jack Roller* (1930). In *The Jack Roller*, a 22-year-old named Stanley talks about what it was like to grow up as a delinquent in early twentieth-century Chicago. It still makes great reading.

Becker et al.'s *Boys in White* (1961)—about the student culture of medical school in the 1950s—should be required reading, even today, for anyone trying to understand the culture of medicine in the United States. The ethnographic tradition in sociology continues in the pages of the journal *Contemporary Ethnography*, which began in 1972 under the title *Urban Life and Culture*. (See L. Lofland 1983 and Bulmer 1984 for more on the history of the Chicago School of urban ethnography.)

Participant observation today is everywhere—in political science, management, education, nursing, criminology, social psychology—and one of the terrific results of all this is a growing body of literature about participant obser-

vation itself. There are highly focused studies, full of practical advice, and there are poignant discussions of the overall *experience* of fieldwork. (For large doses of both, see Wolcott 1995, Agar 1996, and C. D. Smith and Kornblum 1996). There's still plenty of mystery and romance in participant observation, but you don't have to go out unprepared.

Fieldwork Roles

Fieldwork can involve three very different roles: (1) **complete participant**; (2) **participant observer**; and (3) **complete observer**. The first role involves deception—becoming a member of a group without letting on that you're there to do research. The third role involves following people around and recording their behavior with little if any interaction. This is part of direct observation, which we'll take up in the next chapter.

By far, most ethnographic research is based on the second role, that of the participant observer. Participant observers can be insiders who observe and record some aspects of life around them; or they can be outsiders who participate in some aspects of life around them and record what they can.

In 1965, I went to sea with a group of Greek sponge fishermen in the Mediterranean. I lived in close quarters with them, ate the same awful food they did, and generally participated in their life—as an outsider. I didn't dive for sponges, but spent most of my waking hours studying the behavior and the conversation of the men who did. The divers were curious about what I was writing in my notebooks, but they went about their business and just let me take notes, time their dives, and shoot movies (Bernard 1987). I was a **participating observer**.

Similarly, when I went to sea in 1972 and 1973 with oceanographic research vessels, I was part of the scientific crew, there to watch how oceanographic scientists, technicians, and mariners interacted and how this interaction affected the process of gathering oceanographic data. There, too, I was a participating observer (Bernard and Killworth 1973).

Circumstances can sometimes overtake the role of mere participating observer. In 1979, El Salvador was in civil war. Thousands fled to Honduras, where they were sheltered in refugee camps near the border. Philippe Bourgois went to one of those camps to initiate what he hoped would be his doctoral research in anthropology. Some refugees there offered to show him their home villages and Bourgois crossed with them, illegally, into El Salvador for what he thought would be a 48-hour visit. Instead, Bourgois was trapped, along with about a thousand peasants, for two weeks, as the Salvadoran military bombed,

shelled, and strafed a 40-square-kilometer area in search of rebels (Bourgois 1990).

Mark Fleisher studied the culture of guards at a federal penitentiary in California (1989), but as an **observing participant**, an insider. Researchers at the U.S. Federal Bureau of Prisons asked Fleisher to do an ethnographic study of job pressures on guards—called correctional officers, or COs in the jargon of the profession—in a maximum-security federal penitentiary. It costs a lot to train a CO, and there was an unacceptably high rate of them leaving the job after a year or two. Could Fleisher look into the problem?

Fleisher said he'd be glad to do the research and asked when he could start "walking the mainline"—that is, accompanying the COs on their rounds through the prison. He was told that he'd be given an office at the prison and that the guards would come to his office to be interviewed.

Fleisher said he was sorry, but he was an anthropologist, he was doing participant observation, and he'd have to have the run of the prison. Sorry, they said back, only sworn correctional officers can walk the prison halls. So Fleisher went to training camp for six weeks and became a sworn federal correctional officer. *Then* he began his year-long study of the United States Penitentiary at Lompoc, California. In other words, he became an observing participant in the culture he was studying. Fleisher never hid what he was doing. When he went to USP-Lompoc, he told everyone that he was an anthropologist doing a study of prison life.

Barbara Marriott studied how the wives of U.S. Navy officers contributed to their husbands' careers (1991). Marriott was herself the wife of a retired captain. She was able to bring the empathy of 30 years' full participation to her study. She, too, took the role of observing participant and, like Fleisher, she told her informants exactly what she was doing.

Holly Williams spent 14 years as a nurse, ministering to the needs of children who had cancer. When Williams did her doctoral dissertation (1995), on how the parents of those young patients coped with the trauma, she started as a credible insider, as someone whom the parents could trust with their worst fears and their hopes against all hope. Williams was an insider who became an observing participant by telling the people whom she was studying exactly what she was up to and enlisting their help with the research.

Going Native

Some field workers start out as participating observers and find that they are drawn completely into their informants' lives. In 1975, Kenneth Good went to study the Yanomami in the Venezuelan Amazon. He planned on living with the Yanomami for 15 months, but he stayed on for nearly 13 years. "To my

great surprise," says Good, "I had found among them a way of life that, while dangerous and harsh, was also filled with camaraderie, compassion, and a thousand daily lessons in communal harmony" (1991:ix). Good learned the language and became a nomadic hunter and gatherer. He was adopted into a lineage and given a wife. (Good and his wife, Yarima, later tried living in the United States, but after a few years, Yarima returned to the Yanomami.)

Marlene Dobkin de Rios did fieldwork in Peru and married the son of a Peruvian folk healer whose practice she studied (Dobkin de Rios 1981). And Jean Gearing is another anthropologist who married her closest informant on the island of St. Vincent (1995).

Does going native mean loss of objectivity? Perhaps, but not necessarily. In the industrialized countries of the West—the United States, Canada, Australia, Germany, England, France, etc.—we *expect* immigrants to go native. We expect them to become fluent in the local language, to make sure that their children become fully acculturated, to participate in the economy and politics of the nation, and so on.

Some fully assimilated immigrants to those countries become anthropologists and no one questions whether their immigrant background produces a lack of objectivity. Since total objectivity is, by definition, a myth, I'd worry more about producing credible data and strong analysis and less about whether going native is good or bad.

How Much Time Does It Take?

Most basic anthropological research takes a year or more of fieldwork, because it takes that long to get a feel for the full round of people's lives. It can take that long just to settle in, learn a new language, gain rapport, and be in a position to ask good questions and to get good answers.

A lot of participant observation studies, however, are done in a matter of weeks or a few months. Yu (1995) spent four months as a participant observer in a family-run Chinese restaurant, looking at differences in the conceptions that Chinese and non-Chinese employees had about things like good service, adequate compensation, and the role of management.

At the extreme low end, it is possible to do useful participant observation in just a few days. Assuming that you've wasted as much time in laundromats as I did when I was a student, you could conduct a reasonable participant observation study of one such place in a week. You'd begin by bringing in a load of wash and paying careful attention to what's going on around you.

After two or three nights of observation, you'd be ready to tell other patrons that you were conducting research and that you'd appreciate their letting you

interview them. The reason you could do this is because you already speak the native language and have already picked up the nuances of etiquette from previous experience. Participant observation would help you intellectualize what you already know.

In general, though, participant observation is not for the impatient. Gerald Berreman studied life in Sirkanda, a Pahari-speaking village in north India. Berreman's interpreter-assistant, Sharma, was a Hindu Brahmin who neither ate meat nor drank alcohol. As a result, villagers did neither around Berreman or his assistant. Three months into the research, Sharma fell ill and Berreman hired Mohammed, a young Muslim schoolteacher, to fill in. When the villagers found out that Mohammed ate meat and drank alcohol, things broke wide open and Berreman found out that there were frequent intercaste meat-and-liquor parties. When villagers found out that the occasional drink of locally made liquor was served at Berreman's house, "access to information of many kinds increased proportionately" (Berreman 1962:10). Even then, it still took Berreman six months in Sirkanda before people felt comfortable performing animal sacrifices when he was around (*ibid.*:20).

And don't think that "long-term" is only for foreign fieldwork. It took Daniel Wolf three years just to get into the Rebels, a brotherhood of outlaw bikers, and another couple of years riding with them before he had the data for his doctoral dissertation (Wolf 1991).

The amount of time you spend in the field can make a big difference in what you learn. Raoul Naroll found that anthropologists who stayed in the field for at least a year were more likely to report on sensitive issues like witchcraft, sexuality, political feuds, etc. (Naroll 1962). Ethnographers who have done very long-term participant observation—that is, a series of studies over decades—find that they eventually get data about social change that is simply not possible to get in any other way (Foster et al. 1979).

My wife and I spent May 2000 on Kalymnos, the Greek island where I did my doctoral fieldwork in 1964–65. We've been visiting that island steadily for 35 years, but something qualitatively different happened this time. I couldn't quite put my finger on it, but by the end of the month I realized that people were talking to me about grandchildren. The ones who had grandchildren were chiding me—very good-naturedly, but chiding nonetheless—for not having any grandchildren yet. The ones who didn't have grandchildren were in commiseration mode. They wanted someone with whom to share their annoyance that "Kids these days are in no hurry to make families" and that "All kids want today . . . especially girls . . . is to have careers."

This launched lengthy conversations about how "everything had changed" since we were all our children's ages, about how life in Greece was getting to be more and more like "Europe" (which is what many Greeks call Germany,

France, and the rest of the fully industrialized nations of the European Union) and even like the United States. I suppose there were other ways I could have gotten people into give-and-take conversations about culture change, gender roles, globalization, modernization, and other big topics, but the grandchildren deficit was a terrific opener. And it would have been a nonstarter had I been 30 instead of 60 years old. It wasn't just age, by the way; it was the result of the rapport that comes with having common history with people.

Here's history. In 1964, Carole and I brought our then two-month old daughter with us. Some of the same people who joked with me in 2000 about not having grandchildren had said to me in 1964: "Don't worry, next time you'll have a son." I recall having been really, really annoyed at the time, but writing it down as data. A couple of years later, I sent friends on Kalymnos the announcement of our second child—another girl. I got back kidding remarks like "Congratulations! Keep on trying. . . . Still plenty of time to have a boy!" That was data, too. And when I told people that Carole and I had decided to stop at two, some of them offered mock condolences: "Oh, now you're really in for it! You'll have to get dowries for *two* girls without any sons to help." Now *that's* data.

Bottom line: You can do highly focused participant observation research in your own language, to answer specific questions about your own culture, in a short time. How do middle-class, second-generation Mexican American women make decisions on which of several brands to select when they go grocery shopping? If you are a middle-class Mexican American woman, you can probably find the answer to that question, using participant observation, in a few weeks, because you have a wealth of personal experience to draw on.

But if you're starting out fresh, and not a member of the culture you're studying, count on taking three months or more, under the best conditions, to be accepted as a participant observer—that is, as someone who has *learned enough to learn*. And count on taking a lifetime to learn some things.

Rapid Assessment

Applied ethnographic research is often done in just a few weeks. Applied researchers just don't have the luxury of doing long-term participant observation fieldwork and may use **rapid assessment** procedures, instead. Rapid assessment of agricultural or medical practices may include participant observation.

Rapid assessment means going in and getting on with the job of collecting data without spending months developing rapport. This means going into a field situation armed with a list of questions that you want to get answered and perhaps a checklist of data that you need to collect. Chambers (1991) advocates

what he calls **participatory rural appraisal**. In **participatory mapping**, for example, he asks people to draw maps of villages and to locate key places on the maps.

In **participatory transects**, he borrows from wildlife biology and systematically walks through an area, with key informants, observing and asking for explanations of everything he sees along the transect. He engages people in group discussions of key events in a village's history and asks them to identify clusters of households according to wealth. In other words, as an applied anthropologist Chambers is called on to do rapid assessment of rural village needs, and he takes the people fully into his confidence as research partners. This method is just as effective in organizations as in small villages.

Applied medical anthropologists also use rapid assessment methods. The focused ethnographic study method, or FES, was developed by Sandy Gove (a physician) and Gretel Peltó (an anthropologist) for the World Health Organization to study acute respiratory illness (ARI) in children. The FES manual gives detailed instructions to field workers for running a rapid ethnographic study of ARI in a community (WHO 1993; Gove and Peltó 1994).

Many ARI episodes turn out to be what physicians call pneumonia, but that is not necessarily what mothers call the illness. Researchers ask mothers to talk about recent ARI events in their households. Mothers also free list the symptoms, causes, and cures for ARI and do pile sorts of illnesses to reveal the folk taxonomy of illness and where ARI fits into that taxonomy. There is also a matching exercise, in which mothers pair locally defined symptoms (fever, sore throat, headache . . .) with locally defined causes (bad water, evil eye, germs . . .), cures (give rice water, rub the belly, take child to the doctor . . .), and illnesses.

The FES method also uses vignettes, or scenarios, much like those developed by Peter Rossi (see Chapter 10). Mothers are presented with cases in which variables are changed systematically ("Your child wakes up with [mild] [strong] fever. He complains that he has a [headache] [stomach ache]," and so on) and are asked to talk about how they would handle the case.

All this evidence—the free narratives, the pile sorts, the vignettes, etc.—is used in understanding the emic part of ARI, the local explanatory model for the illness.

Researchers also identify etic factors that make it easy or hard for mothers to get medical care for children who have pneumonia. These are things like distance to a clinic, availability of transportation, the number of young children at home, availability to mothers of people with whom they can leave their children for a while, and so on. (For an example of the FES in use, see Hudson 1994.)

The key to high-quality, quick ethnography, according to Handwerker

(2001), is to go into a study with a clear question and to limit your study to five focus variables. If the research is exploratory, you just have to make a reasonable guess as to what variables might be important and hope for the best. Most rapid assessment studies, however, are applied research, which usually means that you can take advantage of earlier, long-term studies to narrow your focus.

For example, Edwins Laban Moogi Gwako (1997) spent more than a year testing the effects of eight independent variables on Maragoli women's agricultural productivity in western Kenya. At the end of his doctoral research, he found that just two variables—women's land tenure security and the total value of their household wealth—accounted for 46% of the variance in productivity of plots worked by women. None of the other variables—household size, a woman's age, whether a woman's husband lived at home, and so on—had any effect on the dependent variable.

If you were doing a rapid assessment of women's agricultural productivity elsewhere in east Africa, you would take advantage of Laban Moogi Gwako's work and limit the variables you tested to perhaps four or five—the two that he found were important and perhaps two or three others. Of course, you can study this same problem for a lifetime, and the more time you spend, the more you'll understand the subtleties and complexities of the problem. But the point here is that if you have a clear question and a few, clearly defined variables, you can produce quality work in a lot less time than you might imagine.

Validity—Again

There are at least five reasons for insisting on participant observation in the conduct of scientific research about cultural groups.

1. Participant observation opens thing up and makes it possible to collect all kinds of data. Participant observation field workers have witnessed births, interviewed violent men in maximum-security prisons, stood in fields noting the behavior of farmers, trekked with hunters through the Amazon forest in search of game, and pored over records of marriages, births, and deaths in village churches and mosques around the world.

It is impossible to imagine a complete stranger walking into a birthing room and being welcomed to watch and record the event or being allowed to examine any community's vital records at whim. It is impossible, in fact, to imagine a stranger doing *any* of the things I just mentioned, or the thousands of other

intrusive acts of data collection that field workers engage in all the time. What makes it all possible is participant observation.

- Participant observation reduces the problem of **reactivity**—of people changing their behavior when they know they are being studied. As you become less and less of a curiosity, people take less and less interest in your comings and goings. They go about their business and let you do such bizarre things as conduct interviews, administer questionnaires, and even walk around with a stopwatch, clipboard, and camera. Philippe Bourgois spent four years living in El Barrio (the local name for Spanish Harlem) in New York City (1995). It took him a while, but eventually he was able to keep his tape recorder running for interviews about dealing crack cocaine and even when groups of men bragged about their involvement in gang rapes.

In other words: Presence builds trust. Trust lowers reactivity. Lower reactivity means higher validity of data.

Nothing is guaranteed in fieldwork, though. When LeCompte told children at a school that she was writing a book about them, they started acting out in “ways they felt would make good copy” by mimicking characters on popular TV programs (LeCompte et al. 1993).

- Participant observation helps you ask sensible questions in the native language. Have you ever gotten a questionnaire in the mail and said to yourself, “What a dumb set of questions”? If a social scientist who is a member of your own culture can make up what you consider to be “dumb” questions, imagine the risk *you* take in making up a questionnaire in a culture very different from your own! Remember, it’s just as important to ask sensible questions in a face-to-face interview as it is on a survey instrument.
- Participant observation gives you an intuitive understanding of what’s going on in a culture and allows you to speak with confidence about the meaning of data. Participant observation lets you make strong statements about cultural facts that you’ve collected. It extends both the internal and the external validity of what you learn from interviewing and watching people.

In short, participant observation helps you understand the *meaning* of your observations. Here’s a classic example.

In 1957, N. K. Sarkar and S. J. Tambiah published a study, based on questionnaire data, about economic and social disintegration in a Sri Lankan village. They concluded that about two-thirds of the villagers were landless. British anthropologist Edmund Leach did not accept that finding (Leach 1967). He had done participant observation fieldwork in the area and knew that the villagers practiced patrilocal residence after marriage. By local custom, a young man might receive *use* of some of his father’s land even though legal ownership might not pass to the son until the father’s death.

In assessing land ownership, Sarkar and Tambiah asked whether a “household” had any land, and if so, how much. They defined an independent household as a unit that cooked rice in its own pot. Unfortunately, all married women in the village had their own rice pots. So, Sarkar and Tambiah wound up estimating the number of independent households as very high and the number of those households that owned land as very low. Based on these data, they concluded that there was gross inequality in land ownership and that this characterized a “disintegrating village” (the title of their book).

Don’t conclude from Leach’s critique that questionnaires are “bad,” while participant observation is “good.” I can’t say often enough that participant observation makes it possible to collect quantitative survey data or qualitative interview data from some sample of a population. Qualitative and quantitative data inform each other. Whatever data collection methods you choose, participant observation maximizes your chances for making valid statements.

- Many research problems simply cannot be addressed adequately by anything except participant observation. If you want to understand how a local court works, you can’t very well disguise yourself and sit in the court room unnoticed. The judge would soon spot you as a stranger, and, after a few days, you would have to explain yourself. It is better to explain yourself at the beginning and get permission to act as a participant observer. In this case, your participation consists of acting like any other local person who might sit in on the court’s proceedings.

After a few days, or weeks, you would have a pretty good idea of how the court worked: what kinds of crimes are adjudicated, what kinds of penalties are meted out, and so forth. You might develop some specific hypotheses from your qualitative notes—hypotheses regarding covariations between severity of punishment and independent variables other than severity of crime. Then you could test those hypotheses on a sample of courts.

Think this is unrealistic? Try going down to your local traffic court and see whether defendants’ dress or manner of speech predict variations in fines for the same infraction. The point is, getting a general understanding of how any social institution or organization works—the local justice system, a hospital, a ship, or an entire community—is best achieved through participant observation.

Entering the Field

Perhaps the most difficult part of actually doing participant observation fieldwork is making an entry. There are five rules to follow.

- There is no reason to select a site that is difficult to enter* when equally good sites are available that are easy to enter (see Chapter 3). In many cases, you *will* have a choice—among equally good villages in a region, or among school districts,

hospitals, or cell blocks. When you have a choice, take the field site that promises to provide easiest access to data.

2. *Go into the field with plenty of written documentation* about yourself and your project. You'll need formal letters of introduction—at a minimum, from your university, or from your client if you are doing applied work on a contract. Letters from universities should spell out your affiliation, who is funding you, and how long you will be at the field site. Be sure that those letters are in the language spoken where you will be working and that they are signed by the highest academic authorities possible.

Letters of introduction should not go into detail about your research. Keep a separate document handy in which you describe your proposed work, and present it to gatekeepers who ask for it, along with your letters of introduction.

Of course, there are always exceptions. If you study an outlaw biker gang, like Daniel Wolf did, forget about letters of introduction (Wolf 1991).

3. *Don't try to wing it*, unless you absolutely have to. There is nothing to be said for "getting in on your own." Use personal contacts to help you make your entry into a field site.

When I went to Kalymnos, Greece, in 1964, I carried with me a list of people to look up. I collected the list from people I knew in the Greek American community of Tarpon Springs, Florida, who had relatives on Kalymnos. When I went to Washington, D.C., to study how decision makers in the bureaucracy used (or didn't use) scientific information, I had letters of introduction from colleagues at Scripps Institution of Oceanography (where I was working at the time).

If you are studying any hierarchically organized community (hospitals, police departments, universities, or school systems, for instance), it is usually best to start at the top and work down. Find out the names of the people who are the gatekeepers and see them first. Assure them that you will maintain strict confidentiality and that no one in your study will be personally identifiable.

In some cases, though, starting at the top can backfire. If there are warring factions in a community or organization, and if you gain entry to the group at the top of *one* of those factions, you will be asked to side with that faction.

Another danger is that top administrators of institutions may try to enlist you as a kind of spy. They may offer to facilitate your work if you will report back to them on what you find out about specific individuals. This is absolutely off limits in research. If that's the price of doing a study, you're better off choosing another institution. In the two years I spent doing research on communication structures in federal prisons, no one ever asked me to report on the activities of specific inmates. But other researchers have reported experiencing this kind of pressure, so it's worth keeping in mind.

4. *Think through in advance what you will say* when ordinary people (not just gatekeepers) ask you: What are you doing here? Who sent you? Who's funding you?

What good is your research and who will it benefit? Why do you want to learn about people here? How long will you be here? How do I know you aren't a spy for _____? (where the blank is filled in by whoever people are afraid of).

The rules for presentation of self are simple: Be honest, be brief, and be absolutely consistent. In participant observation, if you try to play any role besides yourself, you'll just get worn out (Jones 1973).

But understand that not everyone will be thrilled about your role as a researcher. Terry Williams studied cocaine use in after-hours clubs in New York. It was "gay night" in one bar he went to. Williams started a conversation with a man whose sleeves were fully rolled, exposing tattoos on both arms. The man offered to buy William a drink. Was this Williams's first time at the bar? Williams said he'd been there before, that he was researcher, and that he just wanted to talk. The man turned to his friends and exploded: "Hey, get a load of this one. He wants to do research on us. You scum bag! What do we look like, pal? Fucking guinea pigs?" (Williams 1996:30).

After that experience, Williams became, as he said, "more selective" in whom he told about his real purpose in those after-hours clubs.

5. *Spend time getting to know the physical and social layout of your field site*. It doesn't matter if you're working in a rural village, an urban enclave, or a hospital. Walk it and write notes about how it *feels* to you. Is it crowded? Do the buildings or furniture seem old or poorly kept? Are there any distinctive odors?

You'd be surprised how much information comes from asking people about little things like these. I can still smell the distinctive blend of diesel fuel and taco sauce that's characteristic of so many bus depots in rural Mexico. Asking people about those smells opened up long conversations about what it's like for poor people who don't own cars to travel in Mexico and all the family and business reasons they have for traveling. If something in your environment makes a strong sensory impression, write it down.

A really good early activity in any participant observation project is to make maps and charts—kinship charts of families, chain-of-command charts in organizations, maps of offices or villages or whatever physical space you're studying, charts of who sits where at meetings, and so on.

For making maps, try to take a GPS (global positioning system) device to the field with you. They are small, easy to use, and relatively inexpensive. In recent years, U.S. federal regulations have been relaxed, so that GPS devices that are accurate to within 10 meters are now available for under \$200 (see Appendix F). What a GPS does is track your path via satellite, so that if you

in adolescents is weakened, but her record of life on Samoa in the 1920s is among the great ethnographies ever written.)

When it comes to doing effective participant observation, learning a new jargon in your own language is just as important as learning a foreign language. Peggy Sullivan and Kirk Elifson studied the Free Holiness Church, a rural group of Pentecostals whose rituals include the handling of poisonous snakes (rattlesnakes, cottonmouths, copperheads, and water moccasins). They had to learn an entirely new vocabulary:

Terms and expressions like "anointment," "tongues," "shouting," and "carried away in the Lord" began having meaning for us. We learned informally and often contextually through conversation and by listening to sermons and testimonials. The development of our understanding of the new language was gradual and probably was at its greatest depth when we were most submerged in the church and its culture. . . . We simplified our language style and eliminated our use of profanity. We realized, for example, that one badly placed "damn" could destroy trust that we had built up over months of hard work. (Sullivan and Elifson 1996:36)

HOW TO LEARN A NEW LANGUAGE

In my experience, the way to learn a new language is to learn a few words and to say them brilliantly. Yes, study the grammar and vocabulary, but the key to learning a new language is saying things right, even just a handful of things. This means capturing not just the pronunciation of words, but also the intonation, the use of your hands, and other nonverbal cues that show you are really, really serious about the language and are trying to look and sound as much like a native as possible.

When you say the equivalent of "Hey, hiya doin'" in any language—Zulu or French or Arabic—with just the right intonation, people will think you know more than you do. They'll come right back at you with a flurry of words, and you'll be lost. Fine. Tell them to slow down—again, in that great accent you're cultivating.

Consider the alternative: You announce to people, with the first, badly accented words out of your mouth, that you know next to nothing about the language and that they should therefore speak to you with that in mind. When you talk to someone who is not a native speaker of your language, you make an automatic assessment of how large their vocabulary is and how fluent they are. You adjust both the speed of your speech and your vocabulary to ensure comprehension. That's what Zulu and Arabic speakers will do with you, too. The trick is to act in a way that gets people into pushing your limits of fluency and into teaching you cultural insider words and phrases.

The real key to learning a language is to acquire vocabulary. People will usually figure out what you want to say if you butcher the grammar a bit, but they need nouns and verbs to even begin the effort. This requires studying lists of words every day and using as many new words every day as you can engineer into a conversation. Try to stick at least one conspicuously idiomatic word or phrase into your conversation every day. That will not only nail down some insider vocabulary, it will stimulate everyone around you to give you more of the same.

A good fraction of any culture is in the idioms and especially in the metaphors (more about metaphors in the section on schemata in Chapter 17). To understand how powerful this can be, imagine you are hired to tutor a student from Nepal who wants to learn English. You point to some clouds and say "clouds," and she responds by saying "clouds." You say, "Very good," and she says, "No brainer." You can certainly pick up the learning pace after that kind of response.

As you articulate more and more insider phrases like a native, people will increase the rate at which they teach you by raising the level of their discourse with you. They may even compete to teach you the subtleties of their language and culture. When I was learning Greek in 1960 on a Greek merchant ship, the sailors took delight in seeing to it that my vocabulary of obscenities was up to their standards and that my usage of that vocabulary was suitably robust.

To prepare for my doctoral fieldwork in 1964–65, I studied Greek at the University of Illinois. By the end of 1965, after a year on the island of Kalymnos, my accent, mannerisms, and vocabulary were more Kalymnian than Athenian. When I went to teach at the University of Athens in 1969, my colleagues there were delighted that I wanted to teach in Greek, but they were conflicted about my accent. How to reconcile the fact that an educated foreigner spoke reasonably fluent Greek with what they took to be a rural, working-class accent? It didn't compute, but they were very forgiving. After all, I was a foreigner, and the fact that I was making an attempt to speak the local language counted for a lot.

So, if you are going off to do fieldwork in a foreign language, try to find an intensive summer course in the country where that language is spoken. Not only will you learn the language (and the local dialect of that language), you'll also make personal contacts, find out what the problems are in selecting a research site, and discover how to tie your study to the interests of local scholars. You can study French in France, but you can also study it in Montreal, Martinique, or Madagascar. You can study Spanish in Spain, but you can also study it in Mexico, Bolivia, or Paraguay.

You'd be amazed at the range of language courses available at universities these days: Ulithi, Aymara, Quechua, Nahuatl, Swahili, Turkish, Amharic,

Basque, Eskimo, Navajo, Zulu, Hausa, Amoy . . . If the language you need is not offered in a formal course, try to find an individual speaker of the language (the husband or wife of a foreign student) who would be willing to tutor you in a self-paced course.

There are, of course, many languages for which there are no published materials, except perhaps for a dictionary or part of the Judeo-Christian Bible. For those languages, you need to learn how to reduce them to writing quickly so that you can get on with learning them and with fieldwork. To learn how to reduce *any* language to writing, see the tutorial by Oswald Werner (2000a, 2000b, 2001).

WHEN NOT TO MIMIC

The key to understanding the culture of loggers, lawyers, bureaucrats, schoolteachers, or ethnic groups is to become intimately familiar with their vocabulary. Words are where the cultural action is. My rule about mimicking pronunciation changes, though, if you are studying an ethnic or occupational subculture in your own society and the people in that subculture speak a different dialect of your native language. In this situation, mimicking the local pronunciation will just make you look silly. Even worse, people may think you're ridiculing them.

Building Explicit Awareness

Another important skill in participant observation is what Spradley (1980:55) called **explicit awareness** of the little details in life. Try this experiment: The next time you see someone look at their watch, go right up to them and ask them the time. Chances are they'll look again because when they looked the first time they were not *explicitly aware* of what they saw. Tell them that you are a student conducting a study and ask them to chat with you for a few minutes about how they tell time.

Many people who wear analog watches look at the *relative positions* of the hands and not at the numbers on the dial. They subtract the current time (the position of the hands now) from the time they have to be somewhere (the image of what the position of the hands will look like at some time in the future) and calculate whether the difference is anything to worry about. They never have to become explicitly aware of the fact that it is 3:10 P.M. People who wear digital watches may be handling the process somewhat differently.

Kronenfeld et al. (1972) report an experiment in which informants leaving several different restaurants were asked what the waiters and waitresses (as they were called in those gender-differentiated days) were wearing and what

kind of music was playing. Informants agreed much more about what the waiters were wearing than about what the waitresses were wearing. The hitch: None of the restaurants had waiters, only waitresses.

Informants also provided more detail about the kind of music in restaurants that did not have music than they provided for restaurants that did have music. Kronenfeld speculated that, in the absence of real memories about things they'd seen or heard, informants turned to cultural norms for what must have been there (i.e., "what goes with what") (D'Andrade 1973).

You can test this yourself. Pick out a large lecture hall where a male professor is not wearing a tie. Ask a group of students on their way out of a lecture hall what color tie their professor was wearing. Or observe a busy store clerk for an hour and count the number of sales she rings up. Then ask her to estimate the number of sales she handled during that hour.

You can build your skills at becoming explicitly aware of ordinary things. Get a group of colleagues together and write separate, detailed descriptions of the most mundane, ordinary things you can think of: making a bed, doing laundry, building a sandwich, shaving (face, legs, underarms), picking out produce at the supermarket, etc. Then discuss one another's descriptions and see how many details others saw that you didn't and vice versa. If you work carefully at this exercise, you'll develop a lot of respect for how complex, and how important, are the details of ordinary life.

Building Memory

Even when we are explicitly aware of things we see, there is no guarantee that we'll remember them long enough to write them down. Building your ability to remember things you see and hear is crucial to successful participant observation research.

Try this exercise: Walk past a store window at a normal pace. When you get beyond it and can't see it any longer, write down all the things that were in the window. Go back and check. Do it again with another window. You'll notice an improvement in your ability to remember little things almost immediately. You'll start to create mnemonic devices for remembering more of what you see. Keep up this exercise until you are satisfied that you can't get any better at it.

Here's another one. Go to a church service, other than one you're used to. Take along two colleagues. When you leave, write up what you each think you saw, in as much detail as you can muster, and compare what you've written. Go back to the church and keep doing this exercise until all of you are satisfied that (1) you are all seeing and writing down the same things and (2) you have reached the limits of your ability to recall complex behavioral scenes.

Try this same exercise by going to a church service with which you *are* familiar and take along several colleagues who are *not*. Again, compare your notes with theirs, and keep going back and taking notes until you and they are seeing and noting the same things. You can do this with any repeated scene that's familiar to you: a bowling alley, a fast-food restaurant, etc. Remember, training your ability to see things reliably does not guarantee that you'll see them accurately. But unless you become at least a reliable instrument of data gathering, you don't stand much of a chance of making valid observations.

Bogdan (1972:41) offers some practical suggestions for remembering details in participant observation. If, for some reason, you can't take notes during an interview or at some event, and you are trying to remember what was said, *don't talk to anyone* before you get your thoughts down on paper. Talking to people reinforces some things you heard and saw at the expense of other things.

Also, when you sit down to write, try to remember things in historical sequence, as they occurred throughout the day. As you write up your notes you will invariably remember some particularly important detail that just pops into memory out of sequence. When that happens, jot it down on a separate piece of paper (or tuck it away in a separate little note file on your word processor) and come back to it later, when your notes reach that point in the sequence of the day.

Another useful device is to draw a map—even a rough sketch will do—of the physical space where you spent time observing and talking to people that day. As you move around the map, you will dredge up details of events and conversations. In essence, let yourself walk through your experience in your mind. You can practice all these memory-building skills now and be much better prepared if you decide to do long-term fieldwork later.

Maintaining Naiveté

Try also to develop your skill at being a novice—at being someone who genuinely wants to learn a new culture. This may mean working hard at suspending judgment about some things. David Fetterman made a trip across the Sinai Desert with a group of Bedouins. One of the Bedouins, says Fetterman,

shared his jacket with me to protect me from the heat. I thanked him, of course, because I appreciated the gesture and did not want to insult him. But I smelled like a camel for the rest of the day in the dry desert heat. I thought I didn't need the jacket. . . . I later learned that without his jacket I would have suffered from sunstroke. . . . An inexperienced traveler does not always notice when the temper-

ature climbs above 130 degrees Fahrenheit. By slowing down the evaporation rate, the jacket helped me retain water. (1989:33)

Maintaining your naiveté will come naturally in a culture that's unfamiliar to you, but it's a bit harder to do in your own culture. Most of what you do "naturally" is so automatic that you don't know how to intellectualize it.

If you are like many middle-class Americans, your eating habits can be characterized by the word "grazing"—that is, eating small amounts of food at many, irregular times during the course of a typical day, rather than sitting down for meals at fixed times. Would you have used that kind of word to describe your own eating behavior? Other members of your own culture are often better informants than you are about that culture, and if you really let people teach you, they will.

If you look carefully, though, you'll be surprised at how heterogeneous your culture is and how many parts of it you really know nothing about. Find some part of your own culture that you don't control—an occupational culture, like long-haul trucking, or a hobby culture, like amateur radio—and try to learn it. That's what you did as a child, of course. Only this time, try to intellectualize the experience. Take notes on what you learn about *how to learn*, on what it's like being a novice, and how you think you can best take advantage of the learner's role. Your imagination will suggest a lot of other nooks and crannies of our culture that you can explore as a thoroughly untutored novice.

WHEN NOT TO BE NAIVE

The role of naive novice is not *always* the best one to play. Humility is inappropriate when you are dealing with a culture whose members stand a lot to lose by your incompetence. Michael Agar did field research on the life of heroin addicts in New York City (1973, 1980a). His informants made it plain that Agar's ignorance of their lives wasn't cute or interesting to them.

Even with the best of intentions, Agar could have given his informants away to the police just by being stupid. Under such circumstances, you shouldn't expect your informants to take you under their wing and teach you how to appreciate their customs. Agar had to learn a lot, and very quickly, to gain credibility with his informants.

There are situations where your expertise is just what's required to build rapport with people. Anthropologists have typed documents for illiterate people in the field and have used other skills (from coaching basketball to dispensing antibiotics) to help people and to gain their confidence and respect. If you are studying highly educated people, you may have to prove that you know

a fair amount about research methods before they will deal with you. Agar once studied an alternative lifestyle commune and was asked by a biochemist who was living there, "Who are you going to use as a control group?" (1980b:58). In my study of ocean scientists (Bernard 1974), several informants asked me what computer programs I was going to use to do a factor analysis of my data.

Building Writing Skills

The ability to write comfortably, clearly, and often is one of the most important skills you can develop as a participant observer. Ethnographers who are not comfortable as writers produce few field notes and little published work. If you have any doubts about your ability to pound out thousands of words, day in and day out, then try to build that skill now, before you go into the field for an extended period.

The way to build that skill is to team up with one or more colleagues who are also trying to build their expository writing ability. Set concrete and regular writing tasks for yourselves, and criticize one another's work on matters of clarity and style. There is nothing trivial about this kind of exercise. If you think you need it, do it.

Good writing skills will carry you through participant observation fieldwork, writing a dissertation, and, finally, writing for publication. Don't be afraid to write clearly and compellingly. The worst that can happen is that someone will criticize you for "popularizing" your material. I think ethnographers should be criticized if they take the exciting material of real people's lives and turn it into deadly dull reading.

Hanging Out, Gaining Rapport

It may sound silly, but just **hanging out** is a skill, and until you learn it you can't do your best work as a participant observer. Remember what I said at the beginning of this chapter: Participant observation is a *strategic* method that lets you learn what you want to learn and apply all the data collection methods that you may want to apply.

When you enter a new field situation, the temptation is to ask a lot of questions in order to learn as much as possible as quickly as possible. There are many things that people can't or won't tell you in answer to questions. If you ask people too quickly about the sources of their wealth, you are likely to get incomplete data. If you ask too quickly about sexual liaisons, you may get thoroughly unreliable responses.

Hanging out builds trust, or **rapport**, and trust results in ordinary conversa-

tion and ordinary behavior in your presence. Once you know, from hanging out, exactly what you want to know more about, and once people trust you not to betray their confidence, you'll be surprised at the direct questions you can ask.

In his study of Cornerville, William Foote Whyte wondered whether "just hanging on the street corner was an active enough process to be dignified by the term 'research'. Perhaps I should ask these men questions," he thought. He soon realized that "one has to learn when to question and when not to question as well as what questions to ask" (1989:78).

Philip Kilbride studied child abuse in Kenya. He did a survey and focused ethnographic interviews, but "by far the most significant event in my research happened as a byproduct of participatory 'hanging out', being always in search of case material." While visiting informants one day, Kilbride and his wife saw a crowd gathering at a local secondary school. It turned out that a young mother had thrown her baby into a pit latrine at the school. The Kilbrides offered financial assistance to the young mother and her family in exchange for "involving ourselves in their . . . misfortune." The event that the Kilbrides had witnessed became the focus for a lot of their research activities in the succeeding months (Kilbride 1992:190).

THE ETHICAL DILEMMA OF RAPPORT

Face it: "Gaining rapport" is a euphemism for "impression management." E. E. Evans-Pritchard, the great British anthropologist, made clear in 1937 how manipulative the craft of ethnography really is. He was doing fieldwork with the Azande of Sudan and wanted to study their rich tradition of witchcraft. Even with his long-term fieldwork and command of the Azande language, Evans-Pritchard couldn't get people to open up about witchcraft, so he decided to "win the good will of one or two practitioners and to persuade them to divulge their secrets in strict confidence" (1958 [1937]:151). Strict confidence? He was planning on writing a book about all this.

Progress was slow, and while he felt that he could have "eventually wormed out all their secrets," he hit on another idea: His personal servant, Kamanga, was initiated into the local group of practitioners and "became a practising witch-doctor" under the tutelage of a man named Badobo (ibid.). With Badobo's full knowledge, Kamanga reported every step of his training to his employer. In turn, Evans-Pritchard used the information "to draw out of their shells rival practitioners by playing on their jealousy and vanity."

Now, Badobo knew that anything he told Kamanga would be tested with rival witch-doctors. Badobo couldn't lie to Kamanga, but he could certainly withhold the most secret material. Evans-Pritchard analyzed the situation

carefully and pressed on. Once an ethnographer is "armed with preliminary knowledge," he said, "nothing can prevent him from driving deeper and deeper the wedge if he is interested and persistent" (ibid.:152).

Still, Kamanga's training was so slow that Evans-Pritchard nearly abandoned his inquiry into witchcraft. Providence intervened. A celebrated witch-doctor named Bögwözu showed up from another district and Evans-Pritchard offered him a very high wage if he'd take over Kamanga's training. Evans-Pritchard explained to Bögwözu that he was "tired of Badobo's wiliness and extortion," and that he expected his generosity to result in Kamanga learning all the tricks of the witch-doctor's trade (ibid.).

But the really cunning part of Evans-Pritchard's scheme was that he continued to pay Badobo to tutor Kamanga. He *knew* that Badobo would be jealous of Bögwözu and would strive harder to teach Kamanga more about witch-doctoring. Here is Evans-Pritchard going on about his deceit and the benefits of this tactic for ethnographers:

The rivalry between these two practitioners grew into bitter and ill-concealed hostility. Bögwözu gave me information about medicines and magical rites to prove that his rival was ignorant of the one or incapable in the performance of the other. Badobo became alert and showed himself no less eager to demonstrate his knowledge of magic to both Kamanga and to myself. They vied with each other to gain ascendancy among the local practitioners. Kamanga and I reaped a full harvest in this quarrel, not only from the protagonists themselves but also from other witch-doctors in the neighborhood, and even from interested laymen. (ibid.:153)

Objectivity

Finally, **objectivity** is a skill, like language fluency, and you can build it if you work at it. Some people build more of it, others less. More is better.

If an objective measurement is one made by a robot—that is, a machine that is not prone to the kind of measurement error that comes from having opinions and memories—then no human being can ever be completely objective. We can't rid ourselves of our experiences, and I don't know anyone who thinks it would be a good idea even to try.

We can, however, become aware of our experiences, our opinions, our values. We can hold our field observations up to a cold light and ask whether we've seen what we *wanted* to see, or what is *really out there*. The goal is not for us, as humans, to become objective machines; it is for us to achieve objective—that is, accurate—knowledge by transcending our biases.

Laurie Krieger, an American doing fieldwork in Cairo, studied physical

punishment against women. She learned that wife beatings were less violent than she had imagined but that the act still sickened her. Her reaction brought out a lot of information from women who were recent recipients of their husbands' wrath. "I found out," she says, "that the biased outlook of an American woman and a trained anthropologist was not always disadvantageous, as long as I was aware of and able to control the expression of my biases" (1986:120).

Colin Turnbull held objective knowledge as something to be pulled from the thicket of subjective experience. Fieldwork, said Turnbull, involves a self-conscious review of one's own ideas and values—one's self, for want of any more descriptive term. During fieldwork, you "reach inside," he observed, and give up the "old, narrow, limited self, discovering the new self that is right and proper in the new context." We use the field experience, he said, "to know ourselves more deeply by conscious subjectivity." In this way, he concluded, "the ultimate goal of objectivity is much more likely to be reached and our understanding of other cultures that much more profound" (Turnbull 1986:27).

Many phenomenologists see objective knowledge as the goal of participant observation. Danny Jorgensen, for example, advocates complete immersion and *becoming the phenomenon* you study. "Becoming the phenomenon" Jorgensen says, "is a participant observational strategy for penetrating to and gaining experience of a form of human life. It is an objective approach insofar as it results in the accurate, detailed description of the insiders' experience of life" (1989:63). In fact, many ethnographers have become cab drivers or exotic dancers, jazz musicians, or members of satanic cults, in order to do participant observation fieldwork.

If you use this strategy of full immersion, Jorgensen says, you must be able to switch back and forth between the insiders' view and that of an analyst. To do that—to maintain your objective, analytic abilities—Jorgensen suggests finding a colleague with whom you can talk things over regularly. That is, give yourself an outlet for discussing the theoretical, methodological, and emotional issues that inevitably come up in full participation field research. It's good advice.

Objectivity and Neutrality

Objectivity does not mean (and has never meant) **value neutrality**. No one asks Cultural Survival, Inc., to be neutral in documenting the violent obscenities against indigenous peoples of the world. No one asks Amnesty International to be neutral in its effort to document state-sanctioned torture. We

recognize that the power of the documentation is in its objectivity, in its chilling irrefutability, not in its neutrality.

Claire Sterk, an ethnographer from the Netherlands, has studied prostitutes and intravenous drug users in mostly African American communities in New York City and Newark, New Jersey (1989, 1999). Sterk was a trusted friend and counselor to many of the women with whom she worked. In one two-month period in the late 1980s, she attended the funeral of seven women she knew who had died of AIDS. She felt that "every researcher is affected by the work he or she does. One cannot remain neutral and uninvolved; even as an outsider, the researcher is part of the community" (1989:99, 1999).

At the end of his second year of research on street life in El Barrio, Phillippe Bourgois's friends and informants began telling him about their experiences as gang rapists. Bourgois's informants were in their mid- to late 20s then, and the stories they told were of things they'd done as very young adolescents, more than a decade earlier. Still, Bourgois says, he felt betrayed by people whom he had come to like and respect. Their "childhood stories of violently forced sex," he says, "spun me into a personal depression and a research crisis" (1995:205).

In *any* long-term field study, be prepared for some serious tests of your ability to remain a dispassionate observer. Hortense Powdermaker was once confronted with the problem of knowing that a lynch mob was preparing to go after a particular black man. She was powerless to stop the mob and fearful for her own safety (1966).

I have never grown accustomed to seeing people ridicule the handicapped, though I see it every time I'm in rural Mexico and Greece, and I recall with horror the death of a young man on one of the sponge diving boats I sailed with in Greece. I knew the rules of safe diving that could have prevented that death; so did all the divers and the captains of the vessels. They ignored those rules at terrible cost. I wanted desperately to *do* something, but there was nothing anyone could do.

Objectivity and Indigenous Research

Objectivity gets its biggest test in **indigenous research**—that is, when you study your own culture. Barbara Meyerhoff worked in Mexico when she was a graduate student. Later, in the early 1970s, when she became interested in ethnicity and aging, she decided to study elderly Chicanos. The people she approached kept putting her off, asking her, "Why work with us? Why don't you study your own kind?" Meyerhoff was Jewish. She had never thought about studying her own kind, but she launched a study of poor, elderly Jews

who were on public assistance. She agonized about what she was doing and, as she tells it, never resolved whether it was anthropology or a personal quest.

Many of the people she studied were survivors of the Holocaust. "How, then, could anyone look at them dispassionately? How could I feel anything but awe and appreciation for their mere presence? . . . Since neutrality was impossible and idealization undesirable, I decided on striving for balance" (Meyerhoff 1989:90).

There is no final answer on whether it's good or bad to study your own culture. Plenty of people have done it and plenty of people have written about what it's like to do it. On the plus side, you'll know the language and you'll be less likely to suffer from culture shock. On the minus side, it's harder to recognize cultural patterns that you live every day and you're likely to take a lot of things for granted that an outsider would pick up right away.

If you are going to study your own culture, start by reading the experiences of others who have done it, so you'll know what you're facing in the field (Messerschmidt 1981; Stephenson and Greer 1981; Fahim 1982; Altorki and El-Sohl 1988). (See the section on native ethnographies in Chapter 17 for more about indigenous research.)

Gender, Parenting, and Other Personal Characteristics

By the 1930s, Margaret Mead had already made clear the importance of gender as a variable in data collection (see Mead 1986). Gender has at least two consequences: (1) It limits your access to certain information; and (2) It influences how you perceive others.

In all cultures, you can't ask people certain questions because you're a [woman] [man]. You can't go into certain areas and situations because you're a [woman] [man]. You can't watch this or report on that because you're a [woman] [man]. Even the culture of social scientists is affected: Your credibility is diminished or enhanced with your colleagues when you talk about a certain subject because you're a [woman] [man] (Scheper-Hughes 1983; Golde 1986; Whitehead and Conaway 1986; Altorki and El-Sohl 1988; Warren 1988).

Sara Quandt, Beverly Morris, and Kathleen DeWalt spent months investigating the nutritional strategies of the elderly in two rural Kentucky counties (Quandt et al. 1997). According to DeWalt, the three women researchers spent months interviewing key informants and never turned up a word about the use of alcohol. "One day," says DeWalt:

the research team traveled to Central County with Jorge Uquillas, an Ecuadorian sociologist who had expressed an interest in visiting the Kentucky field sites. One

of the informants they visited was Mr. B, a natural storyteller who had spoken at length about life of the poor during the past sixty years. Although he had been a great source of information about use of wild foods and recipes for cooking game he had never spoken of drinking or moonshine production.

Within a few minutes of entering his home on this day, he looked at Jorge Uquillas, and said "Are you a drinking man?" (Beverly whipped out the tape recorder and switched it on.) Over the next hour or so, Mr. B talked about community values concerning alcohol use, the problems of drunks and how they were dealt with in the community, and provided a number of stories about moonshine in Central County. The presence of another man gave Mr. B the opportunity to talk about issues he found interesting, but felt would have been inappropriate to discuss with women. (DeWalt et al. 1998:280)

On the other hand, feminist scholars have made it clear that gender is a negotiated idea. What you can and can't do if you are a man or a woman is more fixed in some cultures than in others, and in all cultures there is lots of individual variation in gender roles. While men or women may be "expected" to be this way or that way in any given place, the variation in male and female attitudes and behaviors within a culture can be tremendous.

All participant observers confront their personal limitations and the limitations imposed on them by the culture they study. When she worked at the Thule relocation camp for Japanese Americans during World War II, Rosalie Wax did not join any of the women's groups or organizations. Looking back after more than 40 years, Wax concluded that this was just poor judgment.

I was a university student and a researcher. I was not yet ready to accept myself as a total person, and this limited my perspective and my understanding. Those of us who instruct future field workers should encourage them to understand and value their full range of being, because only then can they cope intelligently with the range of experience they will encounter in the field. (1986:148)

Besides gender, we have learned that being a parent helps you talk to people about certain areas of life and get more information than if you were not a parent. My wife and I arrived on the island of Kalymnos, Greece, in 1964 with a two-month-old baby. As Joan Cassell says, children are a "guarantee of good intentions" (1987:260), and wherever we went, the baby was the conversation opener. But be warned: Taking children into the field can place them at risk. (More on health risks below. And for more about the effects of fieldwork on children who accompany researchers, see Butler and Turner 1987.)

Being divorced has its costs. Nancie González found that being a divorced mother of two young sons in the Dominican Republic was just too much.

"Had I to do it again," she says, "I would invent widowhood with appropriate rings and photographs" (1986:92).

Even height may make a difference: Alan Jacobs once told me he thought he did better fieldwork with the Maasai because he's 6'5" than he would have if he'd been, say, an average-sized 5'10".

Personal characteristics make a difference in fieldwork. Being old or young lets you into certain things and shuts you out of others. Being wealthy lets you talk to certain people about certain subjects and makes others avoid you. Being gregarious makes some people open up to you and makes others shy away. There is no way to eliminate the "personal equation" in participant observation fieldwork, or in any other scientific data-gathering exercise for that matter, without sending robots out to do the work. Of course, the robots would have their own problems. In all sciences, the personal equation (the influence of the observer on the data) is a matter of serious concern and study (Romney 1989).

Sex and Fieldwork

It is unreasonable to assume that single, adult field workers are all celibate, yet the literature on field methods was nearly silent on this topic for many years. When Evans-Pritchard was a student, just about to head off for Central Africa, he asked his major professor for advice. "Seligman told me to take ten grains of quinine every night and keep off women" (Evans-Pritchard 1973:1). As far as I know, that's the last we heard from Evans-Pritchard on the subject.

Colin Turnbull (1986) tells us about his affair with a young Mbuti woman, and Dona Davis (1986) discusses her relationship with an engineer who visited the Newfoundland village where she was doing research on menopause. In Turnbull's case, he had graduated from being an asexual child in Mbuti culture to being a youth and was expected to have sexual relations. In Davis's case, she was expected not to have sexual relations, but she also learned that she was not bound by the expectation. In fact, Davis says that "being paired off" made women more comfortable with her because she was "simply breaking a rule everyone else broke" (1986:254).

With changing sexual mores in our late industrial society, anthropologists have become more open about the topic of sex and fieldwork. Several anthologies have been published in which researchers discuss their own sexual experiences during participant observation fieldwork (Kulick and Willson 1995; Lewin and Leap 1996; Markowitz and Ashkenazi 1999). Proscriptions against sex in fieldwork are silly, because they don't work. But understand that this is one area that people everywhere take very seriously.

The rule on sexual behavior in the field is this: Do nothing that you can't live with, both professionally and personally. On the ground, this means that you have to be even more conscious of any fallout, for you and for your partner, than you would in your own community. Eventually, you will be going home. How will that affect your partner's status?

Surviving Fieldwork

The title of this section is the title of an important book by Nancy Howell (1990). All researchers—whether they are anthropologists, epidemiologists, or social psychologists—who expect to do fieldwork in developing nations should read Howell's book. Howell surveyed 204 anthropologists about illnesses and accidents in the field, and the results are sobering. The maxim that "anthropologists are otherwise sensible people who don't believe in the germ theory of disease" is apparently correct (Rappaport 1990).

One hundred percent of anthropologists who do fieldwork in south Asia reported being exposed to malaria and 41% reported contracting the disease. Eighty-seven percent of anthropologists who work in Africa reported exposure and 31% reported having had malaria. Seventy percent of anthropologists who work in south Asia reported having had some liver disease.

Among all anthropologists, 13% reported having had hepatitis A. I was hospitalized for six weeks for hepatitis A in 1968 and spent most of another year recovering. Glynn Isaac died of hepatitis B at age 47 in 1985 after a long career of archaeological fieldwork in Africa. Typhoid fever is also common among anthropologists, as are amoebic dysentery, giardia, ascariasis, hookworm, and other infectious diseases.

Accidents have injured or killed many field workers. Fei Xiaotong, a student of Malinowski's, was caught in a tiger trap in China in 1935. The injury left him an invalid for six months. His wife died in her attempt to go for help. Michelle Zimbalist Rosaldo was killed in a fall in the Philippines in 1981. Thomas Zwickler, a graduate student at the University of Pennsylvania, was killed by a bus on a rural road in India in 1985. He was riding a bicycle when he was struck. Kim Hill was accidentally hit by an arrow while out with an Ache hunting party in Paraguay in 1982 (Howell 1990:passim).

Five members of a Russian-American team of researchers on social change in the Arctic died in 1995 when their *umiak* (a traditional, walrus-hided Eskimo boat) was overturned by a whale (see Broadbent 1995). The researchers included three Americans (two anthropologists—Steven McNabb and Richard Condon—and a psychiatrist, William Richards) and two Russians (one anthropologist—Alexander Pika—and the chief Eskimo ethnographic

consultant to the project, Boris Mumikhpykak). Nine Eskimo villagers also perished in that accident. I've had my own unpleasant brushes with fate and I know many others who have had very, very close calls.

What can you do about the risks? Get every inoculation you need before you leave, not just the ones that are required by the country you are entering. Check your county health office for the latest information from the Centers for Disease Control about illnesses prevalent in the area you're going to. If you go into an area that is known to be malarial, take a full supply of anti-malarial drugs with you so you don't run out while you're out in the field.

When people pass around a gourd full of *chicha* or *pulque* or palm wine, decline politely and explain yourself if you have to. You'll probably insult a few people, and your protests won't always get you off the hook, but even if you only lower the number of times you are exposed to disease, you lower your risk of contracting disease.

After being very sick in the field, I learned to carry a supply of bottled beer with me when I'm going to visit a house where I'm sure to be given a gourd full of local brew. The gift of bottled beer is generally appreciated and heads off the embarrassment of having to turn down a drink I'd rather not have. It also makes plain that I'm not a teetotaler. Of course, if you *are* a teetotaler, you've got a ready-made get-out.

If you do fieldwork in a remote area, consult with physicians at your university hospital for information on the latest blood-substitute technology. If you are in an accident in a remote area and need blood, a nonperishable blood substitute can buy you time until you can get to a clean blood supply. Some field workers carry a supply of sealed hypodermic needles with them in case they need an injection. Don't go anywhere without medical insurance and don't go to developing countries without evacuation insurance. It costs about \$60,000 to evacuate a person by jet from central Africa to Paris or Frankfurt, but insurance to cover the cost is very affordable.

Fieldwork in remote areas isn't for everyone, but if you're going to do it, you might as well do it as safely as possible. Candice Bradley is a Type-I diabetic who does long-term fieldwork in western Kenya. She takes her insulin, glucagon, blood-testing equipment, and needles with her. She arranges her schedule around the predictable, daily fluctuations in her blood-sugar level. She trains people how to cook for her, and she lays in large stocks of diet drinks so that she can function in the relentless heat without raising her blood sugar (Bradley 1997:4-7). With all this, Bradley has still had close calls—near blackouts from hypoglycemia—but her close calls are no more frequent than those experienced by other field researchers who work in similarly remote areas. The rewards of foreign fieldwork can be very great, but so are the risks.

The Stages of Participant Observation

In what follows, I will draw on three sources of data: (1) a review of the literature on field research; (2) conversations with colleagues during the last 30 years, specifically about their experiences in the field; and (3) five years of work, with the late Michael Kenny, directing National Science Foundation field schools in cultural anthropology and linguistics.

During our work with the field schools (1967–1971), Kenny and I developed an outline of **researcher response** in participant observation fieldwork. Those field schools were 10 weeks long and were held each summer in central Mexico, except for one that was held in the interior of the Pacific Northwest. In Mexico, students were assigned to Nāhñu-speaking communities in the vicinity of Ixmiquilpan, Mexico. In the Northwest field school, students were assigned to small logging and mining communities in the Idaho panhandle. In Mexico, a few students did urban ethnography in the regional capital of Pachuca, while in the Northwest field school, a few students did urban ethnography in Spokane, Washington.

What Kenny and I found so striking was that the stages we identified in the 10-week field experiences of our students were the same across all these places. Even more interesting—to us, anyway—was the experiences our students had during those 10-week stints as participant observers apparently had exact analogs in our own experiences with year-long fieldwork.

1. Initial Contact

During the initial contact period, many long-term field workers report experiencing a kind of euphoria as they begin to move about in a new culture. It shouldn't come as any surprise that people who are attracted to the idea of living in a new culture are delighted when they begin to do so.

But not always. Here is Napoleon Chagnon's recollection of his first encounter with the Yanomami: "I looked up and gasped when I saw a dozen burly, naked, sweaty, hideous men staring at us down the shafts of their drawn arrows! . . . Had there been a diplomatic way out, I would have ended my fieldwork then and there" (Chagnon 1983:10–11).

The desire to bolt and run is more common than we have admitted in the past. Charles Wagley, who would become one of our discipline's most accomplished ethnographers, made his first field trip in 1937. A local political chief in Totonicapán, Guatemala, invited Wagley to tea in a parlor overlooking the town square. The chief's wife and two daughters joined them. While they were having their tea, two of the chief's aides came in and hustled everyone off to another room. The chief explained the hurried move to Wagley:

He had forgotten that an execution by firing squad of two Indians, "nothing but vagrants who had robbed in the market," was to take place at five P.M. just below the parlor. He knew that I would understand the feelings of ladies and the grave problem of trying to keep order among brutes. I returned to my ugly pensión in shock and spent a night without sleep. I would have liked to have returned as fast as possible to New York. (Wagley 1983:6)

Finally, listen to Rosalie Wax describe her encounter with the Arizona Japanese internment camp that she studied during World War II. When she arrived in Phoenix, it was 110°. Later that day, after a bus ride and a 20-mile ride in a GI truck, across a dusty landscape that "looked like the skin of some cosmic reptile" with a Japanese American who wouldn't talk to her, Wax arrived at the Gila camp. By then it was 120°. She was driven to staff quarters, which was an army barracks divided into tiny cells, and abandoned to find her cell by a process of elimination.

It contained four dingy and dilapidated articles of furniture: an iron double bedstead, a dirty mattress (which took up half the room), a chest of drawers, and a tiny writing table—and it was hotter than the hinges of Hades. . . . I sat down on the hot mattress, took a deep breath, and cried. . . . Like some lost two-year-old, I only knew that I was miserable. After a while, I found the room at the end of the barrack that contained two toilets and a couple of wash basins. I washed my face and told myself I would feel better the next day. I was wrong. (Wax 1971:67)

2. Culture Shock

Even among those field workers who have a pleasant experience during their initial contact period (and many do), almost all report experiencing some form of depression and shock soon thereafter—usually within a few weeks. (The term "culture shock," by the way, was introduced in 1960 by an anthropologist, Kalervo Oberg.) One kind of shock comes as the novelty of the field site wears off and there is this nasty feeling that research has to get done. Some researchers (especially those on their first field trip) may also experience feelings of anxiety about their ability to collect good data.

A good response at this stage is to do highly task-oriented work: making maps, taking censuses, doing household inventories, collecting genealogies, and so on. Another useful response is to make clinical, methodological field notes about your feelings and responses in doing participant observation fieldwork.

Another kind of shock is to the culture itself. **Culture shock** is an uncomfortable stress response and must be taken very seriously. In serious cases of culture shock, nothing seems right. You may find yourself very upset at a lack

of clean toilet facilities, or people's eating habits, or their child-rearing practices. The prospect of having to put up with the local food for a year or more may become frightening. You find yourself focusing on little annoyances—something as simple as the fact that light switches go side to side rather than up and down may upset you.

This last example is not fanciful, by the way. It happened to a colleague of mine, and I once became infuriated with the fact that men didn't shake hands the way "they're supposed to." You may find yourself blaming everyone in the culture, or the culture itself, for the fact that your informants don't keep appointments for interviews.

Culture shock commonly involves a feeling that people really don't want you around (which may, in fact, be the case). You feel lonely and wish you could find someone with whom to speak your native language. Even with a spouse in the field, the strain of using another language day after day, and concentrating hard so that you can collect data in that language, can be emotionally wearing.

A common personal problem in field research is not being able to get any privacy. Many people across the world find the Anglo-Saxon notion of privacy grotesque. When we first went out to the island of Kalymnos in Greece in 1964, my wife and I rented quarters with a family. The idea was that we'd be better able to learn about family dynamics. Women of the household were annoyed and hurt when my wife asked for a little time to be alone. When I came home at the end of each day's work, I could never just go to my family's room, shut the door, and talk to my wife about my day, or hers, or our new baby's. If I didn't share everything during waking hours with the family we lived with, they felt rejected.

After about two months of this, we had to move out and find a house of our own. My access to data about intimate family dynamics was curtailed. But it was worth it, because I felt that I'd have had to abort the whole trip if I had to continue living in what my wife and I felt was a glass bowl all the time. As it turns out, there is no word for the concept of privacy in Greek. The closest gloss translates as "being alone" and connotes loneliness.

I suspect that this problem is common to all English-speaking researchers who work in developing countries. Here's what M. N. Srinivas, himself from India, wrote about his work in the rural village of Ramapura, near Mysore:

I was never left alone. I had to fight hard even to get two or three hours absolutely to myself in a week or two. My favorite recreation was walking to the nearby village of Kere where I had some old friends, or to Hogur which had a weekly market. But my friends in Ramapura wanted to accompany me on my walks. They were puzzled by my liking for solitary walks. Why should one walk when

one could catch a bus, or ride on bicycles with friends. I had to plan and plot to give them the slip to go out by myself. On my return, however, I was certain to be asked why I had not taken them with me. They would have put off their work and joined me. (They meant it.) I suffered from social claustrophobia as long as I was in the village and sometimes the feeling became so intense that I just had to get out. (1979:23)

Culture shock subsides as researchers settle in to the business of gathering data on a daily basis, but it doesn't go away because the sources of annoyance don't go away.

Unless you are one of the very rare people who truly go native in another culture, you will cope with culture shock, but not eliminate it. You will remain conscious of things annoying you, but you won't feel like they are crippling your ability to work. Like Srinivas, when things get too intense, you'll have the good sense to leave the field site for a bit rather than try to stick it out. (For more about culture shock, see Furnham and Bochner 1986 and Mumford 1998.)

3. *Discovering the Obvious*

In the next phase of participant observation, researchers settle into collecting data on a more or less systematic basis (see Kirk and Miller 1986). This is sometimes accompanied by an interesting personal response—a sense of discovery, where you feel as if informants are finally letting you in on the "good stuff" about their culture. Much of this "good stuff" will later turn out to be commonplace. You may "discover," for example, that women have more power in the community than meets the eye or that there are two systems for dispute settlement—one embodied in formal law and one that works through informal mechanisms.

A concomitant to this feeling of discovery is sometimes a feeling of being in control of dangerous information, and a sense of urgency about protecting informants' identities. You may find yourself going back over your field notes, looking for places that you might have lapsed and identified an informant, and making appropriate changes. You may worry about those copies of field notes you have already sent home and even become a little worried about how well you can trust your major professor to maintain the privacy of those notes.

This is the stage of fieldwork when you hear anthropologists start talking about "their" village, and how people are, at last, "letting them in" to the secrets of the culture. The feeling has its counterpart among all long-term participant observers. It often spurs researchers to collect more and more data; to accept every invitation, by every informant, to every event; to fill the days

with observation, and to fill the nights with writing up field notes. Days off become unthinkable, and the sense of discovery becomes more and more intense.

This is the time to take a serious break.

4. *The Break*

The mid-fieldwork break, which usually comes after three or four months, is a crucial part of the overall participant observation experience for long-term researchers. It's an opportunity to get some distance, both physical and emotional, from the field site. It gives you a chance to put things into perspective, to think about what you've got so far and what you need to get in the time remaining. Use this time to collect data from regional or national statistical services, to visit with colleagues at the local university and discuss your findings, or to visit other communities in other parts of the country. And be sure to leave some time to just take a vacation, without thinking about research at all.

Your informants also need a break from you. "Anthropologists are uncomfortable intruders no matter how close their rapport," wrote Charles Wagley. "A short respite is mutually beneficial. One returns with objectivity and human warmth restored. The anthropologist returns as an old friend," one who has gone away and returned, and has thereby demonstrated his or her genuine interest in a community (Wagley 1983:13). Everyone needs a break.

5. *Focusing*

After the break, you will have a better idea of exactly what kinds of data you are lacking, and your sense of problem will also come more sharply into focus. The reason to have a formally prepared design statement *before* you go to the field, of course, is to tell you what you should be looking for. Nevertheless, even the most focused research design will have to be modified in the field. In some cases, you may find yourself making radical changes in your design, based on what you find when you get to the field and spend several months actually collecting data.

There is nothing wrong or unusual about this, but new researchers sometimes experience anxiety over making any major changes. The important thing at this stage is to focus the research and use your time effectively rather than agonizing over how to save components of your original design.

6. *Exhaustion, the Second Break, and Frantic Activity*

After seven or eight months, some participant observers start to think that they have exhausted their informants, both literally and figuratively. That is,

they may become embarrassed about continuing to ask informants for more information. Or they may make the supreme mistake of believing that their informants have no more to tell them. The reason this is such a mistake, of course, is that the store of cultural knowledge in any culturally competent person is enormous—far more than anyone could hope to extract in a year or two.

At this point, another break is usually a good idea. You'll get another opportunity to take stock, order your priorities for the time remaining, and see both how much you've done and how little. The realization that, in fact, informants have a great deal more to teach them, and that they have precious little time left in the field, sends many investigators into a frenetic burst of activity during this stage.

7. *Leaving the Field*

The last stage of participant observation is leaving the field. When should you leave? Steven Taylor, a sociologist at the Center for Human Policy, says that when he starts to get bored writing field notes, he knows it's time to close down and go home. Taylor recognizes that writing field notes is time consuming and tedious, but it's exciting, too, when you're chasing down information that plugs directly into your research effort (Taylor 1991:243). When it stops being exciting, it's time to leave the field.

Don't neglect this part of the process. Let people know that you are leaving and tell them how much you appreciate their help. The ritual of leaving a place in a culturally appropriate way will make it possible for you to go back and even to send others.

Participant observation is an intensely intimate and personal experience. People who began as your informants may become your friends as well. In the best of cases, you come to trust that they will not deceive you about their culture, and they come to trust you not to betray them—that is, not to use your intimate knowledge of their lives to hurt them. (You can imagine the worst of cases.) There is often a legitimate expectation on both sides that the relationship may be permanent, not just a one-year fling.

For many long-term participant-observation researchers, there is no final leaving of "the field." I've been working with some people, on and off, for 35 years. Like many anthropologists who work in Latin America, I'm godparent to a child of my closest research collaborator. From time to time, people from Mexico or from Greece will call my house on the phone, just to say "hi" and to keep the relationship going.

Or their children, who happen to be doing graduate work at a university in the United States, will call and send their parents' regards. They'll remind you of some little event they remember when they were seven or eight and you

came to their parents' house to do some interviewing and you spilled your coffee all over yourself as you fumbled with your tape recorder. People remember the darndest things. You'd better be ready when it happens.

Many field workers have been called on to help the children of their informants get into a college or university. This is the sort of thing that happens 20 years after you've "left" the field. The fact is, participant observation fieldwork can be a lifetime commitment. As in all aspects of ordinary life, you have to learn to choose your relationships well. Don't be surprised if you make a few mistakes.

The Front Edge: Combining Methods

More and more researchers these days, across the social sciences, have learned what a powerful method participant observation is at all stages of the research process. The method stands on its own, but it is also increasingly part of a mixed-method strategy, as researchers combine qualitative and quantitative data to answer questions of interest.

Laura Miller (1997) used a mix of ethnographic and survey methods to study gender harassment in the U.S. Army. (Keeping women out of jobs that have been traditionally reserved for men is *gender* harassment; asking women for sex in return for a shot at one of those jobs is *sexual* harassment. Gender harassment need not involve sexual harassment, or vice versa.) Miller spent nearly two years collecting data at eight army posts and at two training centers in the United States where war games are played out on simulated battlefields. She lived in Somalia with U.S. Army personnel for 10 days, in Macedonia for a week, and in Haiti for six days during active military operations in those countries.

Within the context of participant observation, Miller did unstructured interviewing, in-depth interviewing, and group interviewing. Her group interviews were spontaneous: over dinner with a group of high-ranking officers; sitting on her bunk at night, talking to her roommates; in vehicles, bouncing between research sites, with the driver, guide, protocol officer, translator, and guard (Miller, personal communication).

It turns out that "forms of gender harassment" in the U.S. Army is one of those cultural domains that people recognize and think about, but for which people have no ready list in their heads. You can't just ask people: "List the kinds of gender harassment." From her ethnographic interviews, though, Miller was able to derive what she felt was just such a list, including:

1. *Resistance to authority* (hostile enlisted men ignore orders from women officers)
2. *Constant scrutiny* (men pick up on every mistake that women make and use those mistakes to criticize the abilities of women in general);

3. *Gossip and rumors* (women who date many men are labeled "sluts," women who don't date at all are labeled "dykes," and any woman can easily be unjustly accused of "sleeping her way to the top");
4. *Outright sabotage* of women's tools and equipment on work details; and
5. *Indirect threats* against women's safety (talking about how women would be vulnerable to rape if they were to go into combat).

This list emerges from qualitative research—hanging out, talking to people and gaining their trust, and generally letting people know that you're in for the long haul with them. If you are trying to develop programs to correct things that are wrong with a program, then this list, derived entirely from participant observation, is enough. An education program to counter gender harassment against women in the U.S. Army must include something about each of the problems that Miller identified.

While ethnographic methods are enough to *identify* the problems and processes, ethnography can't tell you *how much* each problem and process counts. Yes, enlisted army men can and do sabotage army women's tools and equipment on occasion. How often? Ethnography can't help with that one. Yes, men do sometimes resist the authority of women officers. How often? Ethnography can't help there, either.

Fortunately, Miller also collected questionnaire data—from a quota sample of 4,100 men and women, whites and blacks, officers and enlisted personnel. In those data, 19% of enlisted men and 18% of male noncommissioned officers (like sergeants) said that women should be treated exactly like men and should serve in the combat units just like men, while just 6% of enlisted women and 4% of female noncommissioned officers agreed with this sentiment. You might conclude, Miller says, that men are more supportive than women are of equality for women in combat roles. Some men with whom Miller spoke, however, said that women should be given the right to serve in combat *so that, once and for all, everyone will see that women can't cut it.*

Are men really what Miller called "hostile proponents" of equality for women? Could that be why the statistics show so many more men in favor of women serving in combat units? Miller went back to her questionnaire data: About 20% of men in her survey said that women should be assigned to combat units just like men were—but almost to a man they also said that putting women into combat units would reduce the military's effectiveness.

In other words, the numerical analysis showed that Miller's concept of "hostile proponent of equality" was correct. This subtle concept advances our understanding considerably of how gender harassment against women works in the U.S. Army.

Did you notice the constant feedback between ethnographic and survey data

here? The ethnography produced ideas for policy recommendations and for the content of a questionnaire. The questionnaire data illuminated and validated many of the things that the ethnographer learned during participant observation. Those same survey data produced anomalies—things that didn't quite fit with the ethnographer's intuition. Ethnographic and survey data combined produce more insight than either does alone.

14

Field Notes: How to Take Them, Code Them, Manage Them

Those who want to use qualitative methods because they seem easier than statistics are in for a rude awakening.

—Taylor and Bogdan 1984:53

Anthropologists collect many kinds of data. We collect audiotapes of musical performances and of recitations of folk tales and myths; videotapes of ceremonies, dances, and everyday activities; photographs; newspaper clippings; transcriptions of formal interviews; caches of personal letters. But all anthropologists take field notes. In this chapter, I focus on field notes—how to write them, how to code them, and how to manage them. Many of the