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Doing Participant Observation

Becoming a Participant

In the previous chapter, we discussed the tension between the roles of observer and participant inherent in the role of participant observer. They appear to (and do) require different sets of skills. In this chapter, we will discuss the strategies and skills necessary to successfully adopt the role of participant. The following chapter will focus on the skills necessary to be a good observer.

Becoming a successful participant in another cultural setting involves a series of practical, logistical, and emotional processes. Some aspects of these processes have been the subject of much anthropological literature, while other dimensions are still part of the "mystery" of doing field research. This chapter will begin with a discussion of how you should approach a new field situation, particularly emphasizing the importance of getting formal and informal clearance to do research. We then discuss the importance of building rapport, establishing close, trusting relationships with people in another cultural setting. Building rapport does not often go smoothly, so we also discuss making mistakes and how a beginning field researcher can successfully recover from errors they will inevitably commit.

ENTERING THE FIELD

Gaining entry to a field site and beginning the process of building rapport can be a daunting experience for new researchers and experienced researchers in new settings. *Entrée* can be either overt or covert. In covert *entrée* the researcher does not make explicit that s/he is engaged in a research project. Covert *entrée* may be a reasonable choice in research that involves

nonparticipation or passive participation of the most limited kind, and it is defended by some as necessary under some circumstances (Lugosi 2006). As we express in chapter 11 we believe that taking a covert participatory role is not ethical. In research that includes participant observation, we believe that it is imperative to make the research nature of the relationship clear.

In general, the initial approaches to gaining entrance to a research setting are similar for many different kinds of fieldwork, including fieldwork that will employ participant observation as a method and fieldwork in virtual settings and employing computer mediated communication. First, in research outside the researcher's home country either a special visa for research or a formal research permit may be required. It is important to begin the process of obtaining them well in advance of fieldwork. Even when a special visa or permit is not necessary, it is common courtesy to check in with members of the local community of scholars and researchers. New researchers in a particular setting may want to carry letters of introduction from colleagues with experience in that setting.¹ In recent years the Institutional Review Boards for the Protection of Human Subjects in Research (IRBs) for most U.S. universities and colleges are requiring the research conducted abroad also to be reviewed and approved for the protection of human subjects in the country in which the research will take place. While the method of participant observation is almost always "exempt" in IRB terms, the researcher will need to identify an appropriate institution to review it, and secure a written confirmation that it has been reviewed and approved. In our experience this can take a good deal of time in some countries.

After taking care of issues at the national level, the next step is to identify local leaders and organizations who represent the community in which the research will take place, or who have access to the setting in which the research will take place. That is, the local gatekeepers. While some field situations do not have gatekeepers, this is uncommon (Schensul, Schensul, and LeCompte 1999). Contacting gatekeepers may mean contacting local officials, such as mayors, county executives, presidents of key local community committees, etc. In studies of older adults in communities in Kentucky, Kathleen and her research team made first contacts with the County Judge Executive of each county, and then with the county extension agents, representatives of community development organizations, and leaders of community organizations such as homemakers' clubs, retired teachers' organizations, and the directors of the Senior Citizens' Centers. For a project in a Pittsburgh community that would draw heavily on community participation in the research process (community-based participatory research?) anthropologist Kimberly Rak worked to identify leaders in a housing project community and secured their support of the project before approaching other community members. Leaders of the housing project community then became key collaborators on the project. Leith Mullings (Mullings

2000; Mullings and Wali 2001; Mullings et al. 2001) and her colleagues discuss the steps that they followed in order to carry out the Harlem Birth Right Project, a research project examining stress and reproduction that included participant observation with African American women in Central Harlem. In order to gain *entrée* into a community that was very suspicious of research, Mullings and her colleagues formed alliances with organizations such as the New York Urban League and set up an effective working community advisory board to introduce the research to the community and to guide the specific questions and approaches to the research.

In research in Latin America, we have always identified the organizations that promote the most inclusive meetings of community members. Sometimes this has been regularly organized community political meetings. In Ecuador, for example, the most inclusive meetings were those of the Rural Health Insurance System. In Mexico, we attended meetings of the members of the agrarian reform organization (*ejido*). At community meetings we asked for time on the agenda to present the overall goals of the project, the kinds of methods (in general terms) that we would be using, and to ask permission to work in the community. In each case, we have spoken with leaders before presenting the project at community meetings.

While it has sometimes taken several meetings to secure permission, to date we have never been denied permission to work in a community. However, this does happen. When it does, it may be possible to discover the reasons why the community or community leaders were reluctant to agree and those concerns can be explicitly addressed and the decision changed. In some instances, an alternative research site must be chosen. In still others, it may be that the specific goals of the research need to be examined to understand why a community might be reluctant to allow a particular project to go forward.

Nicole Constable (2003) when using CMC as a critical component to her study of global romance and "mail order" marriages was encouraged by the moderator of an online group to become a member of the group as part of her research. He suggested that she lurk for a few days, and then make her presence as a researcher known to the wider membership. When this was done she received a number of hostile messages—she was "flamed."³ She finally decided to discontinue her participation in that group. When she withdrew from the group she received a larger number of responses supporting her participation and encouraging her to stay. In Constable's case the "gatekeeper" was supportive, but a number of participants were not, resulting in her withdrawal. However, the supportive moderator initiated an offshoot group of about 40 members who were comfortable with the research and who welcomed her as a participant.

In studies of institutions such as schools, clinic, hospitals, religious groups, or voluntary organizations, gaining *entrée* begins with the hierarchy

of the institutions. For example, this would be principals and superintendents in educational research; hospital officials and chiefs of service in hospital based research. It is our experience that in larger cities some frequently used research sites, such as schools and hospitals, have research review boards that review proposals before permission to carry out research is granted.

No matter the setting, it is important for the researcher to carefully explain the purposes of the research project in terms that are comprehensible to the people who will be included as participants.

Entry
Entrée and rapport are facilitated if the community understands and accepts the purposes of the research. Full disclosure of research purposes is an important ethical principle as well as a key to entrée and rapport. By having a socially recognized purpose, the researcher assumes a less ambiguous role within the group studied. Fieldworkers often find that if they do not actively present themselves in an appropriate role, the community may assign them to an inappropriate one. (van Willigen and DeWalt 1985:20)

Gaining permission is the first step for carrying out research. Gaining access to specific institutions, places, and events may take more time. Often the researcher will find that this is facilitated by particular individuals who essentially take the ethnographer under their wing and help to introduce him or her to their society or group.

FIRST CONTACT

A number of ethnographers have noted that some people in any research setting are much more likely to be open and curious, and to even approach the ethnographer early in fieldwork (Agar 1996; Pelto and Pelto 1978). As a stranger, it is very easy to accept the hospitality and assistance of the first welcoming person or group that appears, but the ethnographer has to be cautious because they may then find it difficult to interact with other groups and individuals. Agar has written that the first people to approach the fieldworker are often either "professional stranger-handlers" or "deviants" (Agar 1996).

Ph?
Professional stranger-handlers are individuals who are delegated (or delegate themselves) to check out the new person in their midst and limit his/her access to information and situations that might prove uncomfortable to the group. "They can find out what the outsider is after and quickly improvise some information that satisfies him without representing anything potentially harmful to the group" (Agar 1996:135).

Agar described his experience with professional stranger handlers in two research settings. When he began working in Lombardi he was quickly

approached by an older man named Sakrya. Sakrya was the first person to approach him, and he materialized any time Agar was doing something "bizarre in the early days of fieldwork, like drawing a map or measuring the size of *tanda* huts" (1996:135). Sakrya also informed Agar that there was no room in the *tanda* for him to live. Several months later, when he had determined that Agar was trustworthy, Sakrya was also the person who introduced him to members of the community and arranged for people from his kin group to act as paid assistants.

The other example Agar gives is from his research in a federal narcotics hospital in Lexington, Kentucky. One of the inmates named Jack first approached Agar and posed the question on everyone's mind: "How do I know that you're not a fed?" When Agar replied, "Try me," Jack took the challenge and began to act as his guide around the unit. He made sure that Agar would not see anything they didn't want him to and fed him with misinformation, until Agar proved that he could be trusted. Later Jack became an important key informant and friend. Agar reported that both Sakrya and Jack were individuals who were respected insiders, "natural public relations experts," and held the trust of the members of the community.

The "deviants" who may approach the fieldworker early in research are different. They are individuals who are, for one reason or another, alienated from or marginal to the community or group. Agar defines them as "members who are on the boundaries of the group in some low-status position" (1996:136). Dentan (1970) described spending some time in the company of a man who approached him early in his fieldwork, who turned out to be the Semai equivalent of the "village idiot."

In our experience, some of the first contacts are often made by people who are "opportunists." They are adept at discovering what resources the researcher might have and how those resources can be diverted to themselves and their families. We have found that these individuals are often from the mid range of the socioeconomic system, neither the most affluent nor the poorest. For example, in our early work in Mexico, after making contact with the president of the *ejido* (the agrarian reform community), we were steered by him to a "research assistant"/guide from his own faction. This individual was younger and a bit more educated than the other farmers, and he was not shy about asking for loans, for help in buying a tractor, and for other resources. After a time, we were able to firmly establish the "economic rules" of our relationship and Pedro became a helpful and savvy collaborator in our research.

Having acknowledged the potential pitfalls and the potential benefits of dealing with those who contact us first, it is important to emphasize that in a number of cases the first contacts can turn out to be outstanding informants and knowledgeable insiders. Finding a sponsor who can introduce and vouch for you can be a key to gaining entrance as a participant. The

experience recounted by Whyte (1996a, 1996b; Whyte and Whyte 1984) in his research on a street corner society shows the importance of having a sponsor. Whyte was introduced to his sponsor, Doc, by a social worker. "Somehow in spite of the vagueness of my own explanations the head of girl's work in the Norton Street House understood what I needed. She began describing Doc to me" (Whyte 1996b:75). She made an appointment for them to meet and left them alone. Whyte went into a long explanation of what he wanted to do. "Doc heard me out without a change of expression, so that I had no way of predicting his reaction. When I finished, he asked: 'Do you want to see the high life or the low life.' Whyte replied: 'I want to see all I can.' And Doc answered: 'Well, any night you want to see anything, I'll take you around. I can take you to the joints—gambling joints—I can take you around to the street corners. Just remember that you're my friend. That's all they need to know'" (76). Thus was forged one of the most effective relationships in participant observation research. Doc was not only an intelligent, interested person, but he was personally powerful in the world of Cornerville. Constable (2003) found that recruiting a sponsor from among the online community she was interested in resulted in the creation of a productive online space for her research.

Finding and recruiting a sponsor can be an effective way to enter a community, meet people, and learn the culture. The trick is finding the right kind of sponsor. We suggest that the right kind of sponsor is someone who is in a respected but relatively neutral position in the community, and with whom a relationship of mutual trust can be developed. Agar expressed the response of many of us to William Foote Whyte's relationship with Doc: "We should all be so lucky" (1996:137). Constable's discussion-group-moderator-sponsor was able to identify a subgroup of group members who were comfortable with her research.

Guimaraes (2005), in participant observation in a Brazilian internet multimedia online sociability platform called *Palace*, early on met (through his avatar) a key member of the community who was organizing an online motorbike tour of *Palace*. His sponsor then introduced him to other members as "an anthropologist researching *Palace*" and also assured the other members that Guimaraes was "a nice guy" (149). Guimaraes said:

while talking about the organization and the pleasures of motorbike riding and traveling (either online or offline), it was possible to introduce myself as and ethnographer and establish rapport with some key members of the group as many of the organizers were Gods or Wizards.⁴ (149)

The members of *Palace* came to accept Guimaraes as a participant and researcher.

Agar (1996) noted that in his fieldwork among the Lombardi and in the narcotics hospital, his first contacts eventually became keys to his being

accepted into those settings after he had proved his trustworthiness. In our case, Pedro provided a particular perspective on the community and provided us with an entrée into the community. When we recognized that he was allied with one faction, we were soon able to cultivate contacts in the other organized faction and with the nonaligned group. In chapter 5, we return to this theme when we discuss the issue of sampling in participant observation.

Langness (Cohen et al. 1970), however, suggests that in some settings it may never be possible to avoid or overcome being identified with one faction or another. His warning reinforces the need to be clear at the outset about which questions are the most important in the research, and which groups are most important for getting at that information. While relationships with informants present themselves and develop in their own time and in their own pattern, beginning fieldwork with some idea about the types and range of people who will be included among first contacts helps the researcher to move beyond the limitations they might impose. Eventually, it will be important to establish a working relationship, a rapport, with a much larger number of people within the society or group.

ESTABLISHING RAPPORT

The establishment of "rapport" is often talked about as both an essential element in using participant observation as a tool as well as the goal of participant observation. As Villa Rojas (1979) has written about his and his collaborators' field research in the Mayan region of Mexico, "Our close contact with local people has always led to excellent rapport, the only basis on which really reliable information can be obtained" (59). The definition of what constitutes rapport, however, is an elusive one. Merriam Webster's Online Dictionary defines rapport as "relation marked by harmony, conformity, accord or affinity." In our own thinking, we have often used a definition for which we can no longer find the citation. In this formulation, rapport is a state of interaction achieved when the participants come to share the same goals, at least to some extent—that is, when both the "informant" and the researcher come to the point when each is committed to help the other achieve his or her goal, when informants participate in providing information for "the book" or the study, and when the researcher approaches the interaction in a respectful and thoughtful way that allows the informant to tell his or her story.

Nader (1986) suggested a more one-sided view. She wrote, "Rapport, pure and simple, consists of establishing lines of communication between the anthropologist and his (sic) informants in order for the former to collect data that then allows him (sic) to understand the culture under study" (113).

Jorgenson (1989) focuses on the need to develop a situation of "trust and cooperation" between the researcher and the people in a research setting. As Jorgenson notes, the degree to which trust and cooperation (rapport) are established influences to large extent the degree to which information gathered in participant observation is accurate and dependable. Jorgenson suggests that the researcher periodically assess the extent to which s/he feels that a situation of trust and cooperation exists between the researcher and the people with whom s/he is working. Schensul et al. (1999) note that the development of trust, which can be mobilized throughout the fieldwork process, is one of the key benefits of participant observation.

How does one establish rapport? To large extent, rapport is built in much the same way as any other personal relationship. It is built over time. It requires that the researcher put effort into learning appropriate behavior in a setting; showing respect for people in a setting; being a good and careful listener; and being ready to reciprocate in appropriate ways. It means that if the researcher expects informants to tell the truth, at least as they see it, the researcher must also be prepared to tell the truth.

Guimaraes (Guimaraes 2005) argues that the establishment of rapport in online, virtual settings is very similar to offline settings. He notes that the same skills "of knowing how to listen to an informant, learning the proper way to behave and so on are as valuable online as offline" (151). He exercised these strategies in gaining acceptance in *Palace*.

Reciprocity is a critical component of establishing rapport. Clearly, there is a range of level of reciprocity. It certainly includes telling the truth when the researcher is asked about the research, his/her goals in research, or his/her life stories. Telling the truth can pose problems for the researcher but it is our feeling that if the researcher cannot tell the truth about the goals of the research project, it probably should not be carried out. However, deciding how much beyond the general research question and objective to share can be problematical. We should worry about excessively influencing the outcome of the research. In general, it is not necessary to share specific hypotheses, or to attempt a short course on social science research and theory.

Answering personal questions can also be a problem. There are times and situations in which women researchers, for example, may want to leave the impression that they are married or at least in a relationship "back home" to deflect some of the almost inevitable sexual solicitation. We feel that it is acceptable to leave this impression, even when it is not entirely true. (Although, to be frank, if the spouse or boyfriend isn't right on the scene, a fictitious one isn't much help with this aspect of living in the field.) Apart from this, we feel it important to answer questions about religious beliefs and practices, values, opinions, and so forth truthfully. If you feel that you have to lie, how can you expect informants to be frank and open with you? However, the researcher can put their opinions and personal information

in the most neutral way possible. For example, when asked about what we think of politicians either of the country in which we are working or our own, we have learned to say: "I agree/disagree with his/her policies" rather than "S/he is a great/horrible leader/crook." When the researcher truly respects the point of view of the informant, it is quite easy to answer religious and other belief questions by saying, "I don't believe that, but I certainly understand why you do."

In many cultural settings, we have found that it is entirely appropriate to ask how much money a person earns. In fact, it makes gathering income information in Mexico a bit easier. The answers may not be truthful, or at least not accurate, but the informants are usually not offended by the question. However, the question often is turned around. In the kinds of research setting in which we have worked, our income, even as graduate students, was incomprehensibly large for a rural, resource-poor farmer to deal with. Over the years, we developed a strategy of not giving a dollar (or peso, or sucre) amount, but to say something like, "I must be frank with you, I am well paid for my work with the university. We are very comfortable." This has seemed to work in most settings.

Another common question is "What's in this for you?" In other words, people want to know what you will gain from this project. Again, honesty is the only viable approach. (And since most of us will not experience large direct economic gain from any project, the researcher can almost always reply that there is no real money involved.⁵) We generally put the answer to this in terms that are understandable in the local setting. For example: "We will write a book that will tell our colleagues what life is like here. This will help us finish our degree," or "get a job," or "keep our job," etc. Sometimes the answer is, truthfully, that we will be able to use the information to help find support for community projects or the development of better policies.

Being truthful with informants does not extend to questions about others in the community. The protection of confidentiality always comes before any other consideration in fieldwork. Researchers must be assiduous in not sharing with other community members' personal information that may have been learned in doing fieldwork. Betraying confidentiality, even inadvertently, is not only unethical, but it ALWAYS comes back to bite you. It can mean, at minimum, the loss of an informant, but can also end a research project, and even affect a career long term.

Engaging in reciprocity may mean sharing personal goods and providing services (e.g., transportation) as they are appropriate in the setting. Responding to numerous requests to share goods or provide services can be very irritating, especially when the requests are beyond the level appropriate in the researcher's home setting. Sometimes an accommodation needs to be made. For example, Rabinow found it easier not to have a car during fieldwork than to respond to constant requests for rides. In Temascalcingo,

we also found ourselves serving as the local taxi service but this never became especially problematical.

Reciprocity may mean arranging for material returns to individuals or communities. Malinowski distributed tobacco to informants. In research in 1976 in the Brazilian Amazon, Kathleen and her colleagues brought glass trade beads to distribute to members of the community, and before leaving gave away many personal goods such as soap, flip-flops, and batteries. Guimaraes helped to build and (virtually) decorate *Palace* servers and rooms.

We have always tried to avoid directly paying informants (it seems antithetical to the notion of participant observation) but we have given resources to communities in other ways. In Honduras, people asked whether we could provide the community with a calculator and we were happy to oblige. In the Andean region of Ecuador, our research project sponsored a soccer tournament among the research communities and provided food during the games and a trophy for the winner. We also employed community nutritionists in each region to work with community members, provide community education, and train local health workers. We then supported modest salaries of those health workers. In another project, we provided money to start a rotating credit fund for each community in the project. We routinely employ local assistants and guides. The cost of these kinds of activities is often low for the researcher, but significant for the community and individuals.

Collings (2009) discusses this issue in a report on a recent phase on ongoing research with Canadian Inuit. As part of a staged approach to gaining official, community, and individual agreement to carry out the research he attended a community meeting to present his proposed project:

I began by explaining the project's aims, methods, and expected results. Not three minutes went by before a hand was in the air. What kinds of benefits will we see from this research? What good will your research be to us?

Oooooh! I was excited on hearing a question I expected, even hoped, to hear. Trying not to sound too smug, I launched into my prepared answer: A database on subsistence production and food networks would be invaluable for community-based decisions on wildlife management and hunter support programs. I had hardly started in on my explanation when I noticed a woman tap the questioner on the shoulder. She whispered but loudly enough that everyone in the room could hear her. "It says on page 3 he is going to give us lots of money." The questioner turned back to me. "OK, it's good then." There were no more questions. The letters of support were ready the next day. (138)

Collings goes on to note:

In retrospect, I should not have been surprised that money would be the deciding factor in obtaining a permit, although money as an issue did not occur to me until I began filling out ARI's licensing paperwork some time later. The

first section on the application form asks for the investigator's address and affiliations. The first question in the second section? How much money from the research will contribute to economic development? That is, how much money is the investigator going to spend in the community? Only later on the application form were applicants required to explain and justify the research project. To me, the message was quite clear: From ARI's perspective, the most important issue in community-based or collaborative research, at least in the western Canadian Arctic, is financial. (Collings 2009:20)

Even though, as he further notes, collaborative/participatory research is becoming increasingly common.

Probably the most common things field researchers provide to informants are photographs. We have made it a practice to provide families with copies of pictures taken of them. When it is possible, we will respond positively to requests to take "family portraits." We have found that sharing photographs makes it less awkward for us when we are taking other photographs to document life in communities.

Finally, reciprocity often includes sharing research results with the community. This is especially important if the community can make good use of the information. In the project in which we employed community nutritionists, we incorporated the results of our study into community education. In other studies, we have been able to assist community members in writing proposals for support, using, in part, information from the studies. Several online researchers have contributed their expertise to building or maintaining the online environments.

This discussion of the process of developing rapport and coming to be accepted in a community begs the question of how long it takes to achieve. In reality, a lot depends on the ability, characteristics, and experience of the ethnographer, the circumstances and characteristics of the group being studied, and the kinds of information that one wants to obtain.

Rapid research methods (RAP, FES) are also based on the establishment of rapport, but under more constrained circumstances. When field work is a matter of weeks rather than months, the definition of rapport is a bit more narrow, but not qualitatively different. We have often, for example, participated in rapid appraisals (Kumar 1993) in which the objective is to obtain a quick impression of farming techniques, agricultural problems, or significant kinds of illnesses in a community. In this situation the degree to which either party understands or shares the goals of the others is, of necessity, quite limited. In these situations, the ethnographer has to achieve an "instant rapport" that is at least sufficient to put informants at ease to answer the questions being asked. Rapport is still based on developing good communication relationships that emphasize listening on the part of researchers (Cerneja 1992; Chambers 1992; Slim and Mitchell 1992). At the minimum, it requires that the researcher explain in simple format the

goals of the study and treat the responses of respondents and informants with respect and attention to the protection of their rights. The development of rapport by being on the ground, listening, and participating can take place even in emergency settings (Slim and Mitchell 1992). As rapid assessment research has come to include participatory research methods in both rural development and health research, rapport as the sharing of each other's goals has come to include the development of shared goals for programming planning and implementation developed through interaction between the researchers and the participants in research (Chambers 1992; Reason and Bradbury 2008).

For other kinds of topics, much more rapport is required before much information can be gathered. During our fieldwork in Temascalcingo (B. R. DeWalt 1979; K. M. DeWalt 1983) we were unable to get people to talk about topics such as witchcraft or traditional curing practices until we had spent over six months in the field. In other situations in which the people being studied are for some reason very suspicious (e.g., harsh exploitation by outsiders, situations of violence or deprivation, extreme isolation), it may never be possible to achieve a substantial amount of rapport. It has become relatively standard in ethnographic inquiries to think of a minimum of a year of fieldwork as necessary to gain sufficient insight from participant observation, but this is only a general guideline. Certainly, the longer that the investigator is in the field, the higher the level of trust between community and investigator, and the better the quality of the information is likely to be.

To make the point again, rapport exists when both investigator and informants come to share common goals or move to develop joint goals for the research. The participants in the setting or events under study must come to agree to help the investigator, however they understand the project. This means that it is the responsibility of the investigator to describe, at least in general terms, what the project is and what he hopes the product will be (e.g., book, report, or proposal). Clearly, this does not mean explaining the nature of social research or anthropological theory. However, it does mean explaining what one is about at the outset of research and every time anyone asks. Near the end of the research, it may be appropriate to share findings with people in the research through a presentation to the community.

Establishing rapport also means that the researcher must come to accept the goals of the community. At the most basic, this means telling the story to others accurately and fairly, having and showing respect for the participants, and engaging in reciprocity. It can include explicitly discussing goals with informants and finding ways in which the results of research can be useful to the community. It may mean recruiting members of the community to participate in formulating questions and even collecting information. In recent research on the coast of Ecuador, in which we were

investigating the impact of income-generating projects on women's social power in rural communities, the questions in which we were interested were, in part, a response to the issues identified by women in the study communities. Members of the study communities were consulted before the beginning of each research phase and community members were recruited into the field teams to serve as guides and assistants.

For most investigators using participant observation, incorporating the concerns and goals of the people with whom we work is not difficult. In fact, the greatest strength of the method is that the researcher attempts to gain the point of view of the participant. Part of the iterative nature of participant observation is that the questions continually evolve as new information is gathered and new insights reached. The big caveat here is that individuals within a research setting have many different goals. Understanding and sharing the goals of the community and individuals within it does not mean laying aside the analytical stance that is also inherent in the method. It does mean that the reporting of research results should not place any individual in harm's way. The Code of Ethics of the American Anthropological Association (American Anthropological Association 2009:4) states:

Anthropological researchers have primary ethical obligations to the people, species, and materials they study and to the people with whom they work. These obligations can supersede the goal of seeking new knowledge, and can lead to decisions not to undertake or to discontinue a research project when the primary obligation conflicts with other responsibilities, such as those owed to sponsors or clients. These ethical obligations include:

To avoid harm or wrong, understanding that the development of knowledge can lead to change which may be positive or negative for the people or animals worked with or studied;

To respect the well-being of humans and nonhuman primates;

To work for the long-term conservation of the archaeological, fossil, and historical records;

To consult actively with the affected individuals or group(s), with the goal of establishing a working relationship that can be beneficial to all parties involved.

Sharing goals may also come to mean helping communities to achieve their goals. Many researchers have made the information they obtained available to communities and many have taken on activist roles. Wax writes of her experiences in a Japanese relocation camp during World War II: "I, as a field-worker, came to participate in this struggle and my behavior and attitudes . . . came to resemble those of a fighter in a resistance movement" (Wax 1971:174). In participatory research programs, researchers and participant develop at least some joint goals.

BREAKING THROUGH

Many fieldworkers find that they can point to a single event or moment in which the groundwork for the development of true rapport and participation in the setting was established (e.g., Nader 1986; Stack 1996; Sterk 1996; Whyte 1996a; Whyte and Whyte 1984). Clifford Geertz (1973) elegantly describes the event that allowed him and his wife to begin to gain acceptance by the community and to establish true rapport in the Balinese village in which they worked. The Geertzes had been in the village for about a month, during which time the villagers treated them as though they were not there. They were rarely greeted, people seemed to look right through them; people would move away when they approached. It was truly an anthropologist's nightmare.

Their breakthrough came as a result of a police raid on an illegal cockfight they were observing. Although the Geertzes could have stood their ground and presented the police with their credentials and permissions, they choose to run away with the rest of the villagers when the cockfight was raided. In Geertz's words:

On the established anthropological principle, "When in Rome," my wife and I decided, only slightly less instantaneously than everyone else, that the thing to do was run too. We ran down the main village street, northward, away from where we were living. . . . About halfway down another fugitive ducked suddenly into a compound—his own it turned out—and we, seeing nothing ahead of us but rice fields, open country and a very high volcano, followed him. As the three of us came tumbling into the courtyard, his wife, who had apparently been through this sort of thing before, whipped out a table, a tablecloth, three chairs, and three cups of tea, and we all, without any explicit communication whatsoever, sat down, commenced to sip tea and sought to compose ourselves. (415)

When, moments later, the police arrived, the Geertzes' adopted host was able to provide a lengthy and accurate description of who they were, what they were doing in the village, and what permissions they had. In addition, he noted, the Geertzes had been in this compound all afternoon sipping tea and knew nothing about the cockfight. The bewildered police left. The Geertzes found that after that point they were enthusiastically incorporated into the community.

An even more dramatic account of a single event that lead to fuller participation is provided by Kornblum (1996) who was called on to stand with "his" gypsy family when they were attacked by Serbians in their camp outside Paris. In a moment of crisis, he became one of them.

As I found my way to the main road through the camp, my worst fears were confirmed. It seemed there would be a pitched battle, for the Boyash men

and women were grouped about 50 yards away from a much larger group of Serbian men. Both groups were heavily armed. I saw Cortez flick open his switchblade. Tony was holding a shotgun. The Boyash women kept up a steady barrage of violent oaths and insults. As we slowly advanced toward the Serbians I attempted to find a place in the second ranks, but Persa was there again to shove me to the front. (1-2)

He relates that participating in this (eventually nonviolent) encounter resulted in a subtle change in his relationship with the Gypsies. "Persa and I never discussed that incident . . . but I could tell it had changed her opinion of me from one of disdain to one of guarded respect" (2).

After spending some time studying prostitutes in Amsterdam, Sterk (1996) studied prostitutes in New York in the mid-80s just as the magnitude of the AIDS epidemic had become clearer. Sterk had spent several days "on the stroll" in New York but none of the prostitutes would speak with her. Finally, one challenged her; she explained that she was from Amsterdam and wanted to know something about the prostitution in the United States. Her knowledge of the life was challenged, and apparently she answered appropriately, as she was befriended by the prostitute, Ann. Later Sterk found that her already acquired knowledge of the street from her previous experiences gave her credibility with prostitutes and pimps.

Robert Dentan (1970) describes the day he and Ruth Dentan arrived in the first Semai village in which they would work in Malaysia. They had found an isolated village with relatively little contact with "pale people." While people seemed to view him as "a particularly bizarre museum specimen," they had seen "pale" men before, but never a "pale" woman. However, when Ruth said that she wanted to learn to live like a Semai woman, she was whisked away.

A swarm of giggling and chattering women immediately gathered around her, swept her up the ladder into our house, stripped her naked, wrapped her in a sarong, stuck flowers into her hair, painted her face, put a machete in her hand, and took her off to collect firewood. (103)

While it appears that it took Robert Dentan a bit longer to be incorporated into the village, Miss Housefly (the translation of "Ruth" as rendered in the Semai language) was quickly incorporated.

Stack (1996) found her acceptance into the community in an African American section of a Southern city happened more slowly but was facilitated by the breaking down of her car. When her car broke down Stack decided not to fix it. Like Rabinow she has been using the car, in part, to transport community members, giving herself a visible role. Without a car, she was more like the people of the community in which she was working, was less visible and more a part of the community. Lack of mobility resulted in her spending more time in people's homes and led, she writes, to her eventual acceptance into the community.

We found in our work in Temascalcingo that the quality of the information we were receiving improved after our return from a three-week trip to the United States to renew our visas and take a break from field work. Until that point, people seemed to be unsure of our interest in them and their lives. We had been able to carry out fieldwork, but did not feel that we had made close contacts or were incorporated into the flow of daily activities. The fact that we had visited our own culture and families, but had returned to renew our stay in Mexico, demonstrated our commitment to the people in this Mexican community. Everywhere we went, people expressed "*Que milagro*" (what a miracle) and greeted us with greater warmth and affection than they had previous to our break. Suddenly, our previous questions about sensitive subjects like witchcraft were answered in detail rather than tossed-off and evaded. The experience of improved acceptance after returning from a trip away from the research setting is so common in the literature that we have come to think of it as one of the most important ways to enhance acceptance.

To summarize, in a number of these cases, the breakthrough in acceptance was achieved when the researchers provided evidence that their relationship with the community was important and serious TO THEM; when they demonstrated a more than passing commitment to a community. We returned to the community of Temascalcingo against the expectations of community members who had assumed we would not. Clifford and Hillary Geertz acted like Balinese villagers when they could have acted like privileged foreigners. Kornblum risked violence to stand with the people with whom he was working. While these dramatic examples are more vivid, rapport generally is established slowly, simply by continuing to live with and interact with a group of people and conducting oneself in a way that shows the researcher's commitment to the community.

TALKING THE TALK

One of the hallmarks of participant observation since Malinowski described it has been the use of local languages in the research setting. Earlier ethnographers, focused on collecting texts, often worked with interpreters. Participation, however, requires being able to communicate effectively in the local setting; being able to follow informal conversations; being able to understand and join in jokes, etc. Discussing how to learn a language, especially an unwritten language, is well beyond the scope of this volume. However, we would like to note that quite a few of the languages that were unwritten and inaccessible before going to the field for researchers in previous generations are now available for study before fieldwork begins. As a result of the work of the Summer Institute of Linguistics (SIL International

2010)⁶ and the Wycliffe Bible Translators (Wycliffe Bible Translators 2010), grammars and vocabularies exist for a large number of languages. For example, in 1976 Kathleen spent a few weeks with the Northern Kayapo in Brazil. Only one woman in the community spoke any Portuguese. The only way Kathleen was able to achieve any level of communication in that short period of time was through using the notes of a Bible translator who was working on the Kayapo language.

It is not only worth the time and effort to learn the local language, it is imperative. Debra Picchi (1992) recounts how she relearned this important lesson during fieldwork with the Bakairi Indians in Central Brazil. She went to Brazil as an ecological anthropologist with interests in demography, modes of production, and labor organization. After several weeks in the community, she overheard a conversation held in Portuguese about her.

"She doesn't speak well," Cici, a young mother of two complained loudly to her friends. "I think she must be as stupid as those giant anteaters that wander around the jungle."

Domingas, a frail old women, answered Cici softly, "You ask too much. She has been with us only a few weeks. You cannot expect her to speak Bakairi as well as we do. Be patient and she too will be a human being."

Maisa, another young mother spoke up, "I don't know. I'm worried. The *Alemao*⁷ really is not learning very fast at all. Geraldina told me that she tried to explain to her the story of how the jaguar copulated with a human to produce our people. Geraldina said she didn't think that the girl understood two words of what she told her." (146)

Picchi reflected that she had not been studying the Bakairi language diligently, as the people with whom she had been working also spoke Portuguese, and she was, at that point, more interested in collecting demographic and production data. However, she also realized that the conversation had been staged for her benefit, and carried out in Portuguese to make sure she understood. After further reflection, she realized they were telling her that she would not be considered a true human being in their world until she could communicate in their language; and, finally, "the women were trying to tell her that a grasp of the language was a necessary precondition for an effective study of their tradition" (148).

It is difficult for some people to become fluent enough in a new language to carry out effective participant observation. It takes time and it takes study. But it is sometimes also a problem to work in the researcher's first language. In working within the United States we sometimes know that we will be working in a dialect of English that is sufficiently different from the one we learned as children as to cause some problems. There are times, however, when we think we are communicating but are missing subtle regional and local differences in language.

In the study of nutritional strategies of older adults in rural Kentucky we had been collecting recipes for several weeks before we came to realize that the word "seasoning" meant something different to our informants than it did to us. When we heard seasoning, we thought salt and pepper. It took a bit observation of cooking to realize that "seasoning" referred to some type of fat. Traditionally, it was lard or bacon grease. In the 1990s, it was more often solid vegetable shortening or vegetable oil. Not only did we need to rewrite the recipes, we had to edit the program for calculating nutrient content of dishes. Two tablespoons of lard in the green beans represent a lot more calories than salt and pepper.

All communication in research settings should be approached as cross-cultural communication in which one of the tasks of the researcher is to understand the local language. The formal elicitation of local terms for phenomena of interest is a common activity of fieldwork. In some types of research it is a research end in itself. There are a number of approaches to formal elicitation (e.g., Spradley 1970, 1980; Weller and Romney 1988; Werner and Schoepfle 1987a, 1987b). However, even if the presentation of data from formal elicitation is not a goal of the project, taking time to elicit local terms or to follow-up on statements that are not understood is an important aspect of learning to talk the talk.

How far should talking the talk go? Again, understanding local meanings is critical to understanding what is going on. However, there are times when using the language of the setting can come back to bite the researcher. Depending on the role that the researcher comes to have within a community it may or may not be appropriate to talk like the participants. Whyte found that his informants did not like him using profanities, even though they did all the time. For him to do so was not deemed appropriate.⁸

WALKING THE WALK

The heart of adopting the method of participant observation is to behave appropriately enough to be accepted as a participant at some level and to participate in the daily activities of people with whom the researcher is working. By behaving appropriately, we mean learning what constitutes good manners and practicing them to the best of your ability. It can include proper, polite speech; appropriate reciprocity; table manners; appropriate levels of eye contact: all the many niceties (as defined by each culture) that make up day to day interaction. Fortunately, learning how to behave well enough (never perfectly) is not so very hard. Most of us do learn to do it without dire consequences, even when we make mistakes (see below), and we do so at least partially unconsciously (tacit knowledge?). With enough interaction most researchers begin to adopt some mannerisms and learn to

be polite in the local context just by being open to the possibility of doing so. Recall the quotation from Mead (1970b) in chapter 1. She writes that she unconsciously picked up the manners and body language of the various peoples with whom she worked. However, there are some key areas in which problems seem to occur.

North American and European researchers working in another cultural setting routinely have problems with dealing with lack of privacy. Appropriate behavior in many settings includes constantly being with others. Like many others, Robert and Ruth Dentan (Dentan 1970) found they had problems with privacy. As it happened, even defecating was a social event for the Semai. While Robert found he could occasionally slip away to defecate alone, it was an even more gregarious activity for women. Ruth Dentan was not often able to "go it alone."

Ruth almost always had a few companions. As she once remarked, "We squat there in a row in the water, with our sarongs up, like a bunch of ducks." The day Ruth discovered the value of the gregariousness came when she managed to slip off alone and found on her way back a tiger between herself and the settlement. She ran upstream, tried to find another path to the settlement, lost the path, and at last decided that if she had to die she wanted to die in the water. On her return, fortunately, the tiger had gone, leaving only a few paw marks in the sand. After that she preferred having the women accompany her (105).

Intellectually, the Dentans understood that yearning for privacy was partially "pale person's ethnocentrism," which, as good cultural relativists, they were trying to leave behind in their fieldwork. Nonetheless, the Dentans still felt they needed some privacy to remain sane. They instituted a "taboo day" once a month in which they told people that they needed to be alone. The concept of Sabbath was understood by the Semai as a result of contact with missionaries. The taboo day worked pretty well, although even with this in place the Dentans rarely had a completely alone day; but the monthly respite allowed them to manage in the field. Understanding the rules of reciprocity are another arena in which researchers in cultural contexts other than their own find they have problems. Again, as we will note below, Dentan found it difficult to participate in the flow of reciprocity expected in Semai society.

Eating local foods is another aspect of behaving appropriately. Our rule of thumb is to attempt to eat everything we are served, unless we feel that the threat to health is so great that it is worth insulting people. For us, not liking it is not much of an excuse, although it is possible to avoid some items that are particularly distasteful. Bill was able to avoid drinking *pulque*, a mildly alcoholic beverage made from fermenting the sweet sap of the century plant, common in rural Mexico at the time. It was made in homes under somewhat less than fully sanitary conditions (in some homes we

observed people shooing away a dog or a goat from the open terra cotta *pulque* barrel). *Pulque* has a flavor and consistency reminiscent of mild, thick vinegar. While Kathleen came to rather like it, Bill never did. After a bout of amoebas in his liver, he was able to avoid drinking *pulque* as everyone in the countryside understood that one should not drink anything alcoholic while treating liver disease. Bill's liver disease lasted for years, conveniently being used as an excuse whenever he wanted to avoid drinking. Kathleen can't stand the taste of coconut water, a refreshment that is commonly offered in coastal Ecuador. She gets down enough to be polite.

In recent research with Kichwa-speaking people of the Ecuadorian Amazon, Kathleen, two students, and a guide arrived, after a 45-minute climb, in a village in the middle of a local "day of the mother" celebration. The principle food of Napo Kichwa is a "manioc beer"—*aswa*. While a mild *aswa* is consumed every day, it is allowed to ferment to a more alcoholic state for celebrations. As we arrived the *aswa* began to be served. *Aswa* is typically prepared and served by women.⁹ In this case it was men serving women as part of the celebration. The two students looked at Kathleen quizzically. Judging that it was not one of those times when a potentially hazardous food could be turned down, she made a mental note to look for the antibiotics when the group returned to their lodgings. The *aswa* was pretty good (as *aswa* goes), the interview with the Kichwa health promoter was interesting; and two hours later the trip down the mountain went a bit more rapidly with all slightly tipsy. Miraculously, no one had even the hint of tummy upset the next day!

It is a good idea to try to check out local table manners early in the field experience. Several of our acquaintances report being overfed in parts of India, before they came to understand that their hostess would continue to fill their plates until they left a bit of food uneaten. Like good North Americans, they were "cleaning their plates," a signal in India that one was still hungry. Saying "no" to another helping is thought to just show polite coyness. In some cultures it is impolite to arrive in a home at a mealtime, as it means that the guest will have to be fed whether or not the resources will allow.

Wax (1971) reports that she realized that she had unknowingly been insulting the Japanese American residents of the relocation camp, because she was unfamiliar with social conventions. When she did become aware, she was able to do well enough to establish many close relationships. We do know people who seem never to be able to give in to local expectations for behavior or who cannot at least in some cultural settings. Frankly, as we noted in chapter 2, they should find another field site or use a different methodological approach.

How do we learn enough to participate appropriately? The most important condition is to want to do so, that is, to be committed enough

to using participant observation as a method that the researcher will try to learn. Coupled with a nonjudgmental attitude toward expectations of social life, being open to learning is almost enough for most researchers. A second condition is a genuine respect for the community and the people with whom the researcher will be working. The researcher has to approach participating and observing any particular situation with an open mind and a nonjudgmental attitude. That is, while the activities in which the ethnographer is taking part may be extraordinarily exotic or mundane, a good field researcher must react to the goings-on with sensitivity and discretion.

The researcher cannot be too shy. In order to participate in activities, one must go out and find activities in which to participate. We sometimes get told "no," but unless the researcher asks to go to the event, work in field, hang out on the street corner, gaining access will not happen very quickly. Almost all people love to tell their story and to share their experiences with those who take an interest in them. While we should be sensitive about intruding into situations where we are not wanted or welcome, if an ethnographer shows genuine interest in learning more about behaviors, thoughts, and feelings, he or she will be a welcome guest at most activities. As Picchi's example regarding the language of the Bakairi, the people with whom we are working generally want us to fit in with them, to become part of the community to the extent possible. They are usually ready to teach us how, no matter how painful and humiliating it may be for us.

Which activities and events to choose to participate in is dependent on the focus of the research project, primary research question, and the limits to participation allowed by the community for each particular researcher. The classic ethnographer tries to participate in as many different activities as possible. However, most contemporary researchers have a more focused agenda, and limited time.

MAKING MISTAKES

The book *Nutritional Strategies and Agricultural Change in a Mexican Community* begins with the following paragraphs:

It was May 3, 1973, the Feast of the Holy Cross—an important feast day for the community of Puerto de las Piedras in the town of Temascalcingo, Mexico. We had been invited to the chapel for services after which an offering was to be made to the Virgin of Guadalupe, followed by a ritual meal. The offering to the Virgin consisted of a handmade basket filled with cigarettes, chocolate, candy, breads, and fruit. The basket was to be thrown off the bridge which straddles the Lerma River, below the chapel.

After the basket had been dropped into the muddy waters of the Lerma, amidst a cloud of burning incense, the office holders in the lower levels of the

religious hierarchy served a ritual meal to the men and women occupying the highest levels of the hierarchy. Large bowls of rice, mountains of tortillas and piece upon piece of turkey in a rich mole . . . were heaped before each of the five couples receiving the meal. Even the gringos were offered a few tacos of turkey and mole. (K. M. DeWalt 1983:1)

Let's stop right there. What the writer does not tell the reader in this passage is that the events that culminated in the description reproduced above also included one of the biggest mistakes in participant observation made by the ethnographers, who were a very young Kathleen and Billie DeWalt. As noted in the written description, food was offered to the two inexperienced ethnographers. However, clear in the field notes, but not included in the published description, was the fact that the ethnographers declined to eat the food offered to them. This event took place early in our fieldwork. Both of us felt very intimidated and anxious about what we were supposed to do on this occasion. It was clear to us that we had witnessed an important event in the ritual life of the community and we did not want to intrude on a ceremonial meal that was to be shared among the community's authorities. Our interpretation was that people were just being polite in inviting us to the feast; based on our norms from the United States, we thought that the polite thing to do was to decline and withdraw from the situation. It subsequently became clear to us that our behavior had offended our hosts.

We heard about this *faux pas* for some time thereafter from a number of people, beginning with one of our key informants who stopped by the very next day to chide us for being so rude as to decline food at a ritual meal. He noted that "the other anthropologist" (a colleague of ours working in a community across the valley) always ate everything she was offered.

Looking back on these events, rereading 40-year-old field notes, we cannot imagine how we could have made such a mistake. Eating all that is presented is probably a cardinal rule of anthropological fieldwork. In our defense, however, we had only been carrying out fieldwork in the community for a few months, our language ability was still rudimentary, and we had not yet begun to pick up on the nuances of "expected behavior." We had been invited by one of our key informants to attend the ceremony and meal, but were unclear as to his role in this event. (As we will note later, he was not only delegated by one of the faction leaders to be our "professional stranger handler," but it was also clear that he expected to gain personally by his association with the North Americans.) The event was clearly for cargo holders in the religious hierarchy. We were afraid he was trying to show off his relationship to the gringos by arriving with us. Our field notes record how much we felt, at that time, like interlopers out of place in these events, gringos. We were afraid that we were taking food out of the mouths of people,

who, compared with North Americans, including relatively poor graduate students (as we were at the time), were living on the economic margins.

Our declining to eat a few tacos of turkey and *mole*, however, resulted in the feeling among community members that we were disdainful of their poor food, worried about contamination and illness, aloof. In fact we loved the food, and were not particularly concerned about health. We were uncomfortable and unsure of our participation in the life of the community. We were feeling-out the experience of participant observation and finding our role as ethnographers in the community. As time went on, we became more comfortable with the role of participant observer, feeling less like interlopers, joining, with enthusiasm in any community event or activity to which we were invited.

Even semidisastrous, as it was, the experience of joining in the service, the offering of gifts to the Virgin, and the observation of the meal and the roles of the various cargo holders in the event gave us a much better understanding of the role of the cargo system in the community, the structure of exchange among community members, the role of key foods in both the offering to the Virgin and the maintenance of the highest level of cargo holders. We could never have gained this insight from interviewing alone. To merely talk about the amount of food exchanged in the meal would not have captured the nature of the presentation of truly vast amounts of food, the impact on the families receiving food, the impact on their networks of relatives and *compadres* who would receive leftovers from the feast the next day.

This story also illustrates another point that experience teaches. That is, while this important breach of etiquette probably affected our development of rapport with community members for a time, in the end, our refusal to eat was not a fatal mistake. Over the ensuing months in the community, we had the opportunity to apologize to various individuals involved in the event. We were able to use the time honored (and accurate) excuse that we didn't know what was appropriate; we were inexperienced young gringos with good intentions, but poor manners who needed to be taught appropriate behavior. Our long-term relationship with the community was not appreciably compromised by this event—and our initial *faux pas* resulted in us learning quite a bit about expected behavior in the community.

Bourgois (1995) committed a far more serious breach when he offered a newspaper article in which he was featured, to Ray, a man Bourgois describes as "crucial not only to my continual access to crack scene, but also my physical security" (19–20). Bourgois did not realize that Ray was illiterate, and embarrassed him in front of the people from whom he expected respect. In essence, Bourgois disrespected one of his key links with the world he was studying, a world that revolved around the concept of respect. Bourgois describes how he was still feeling uncomfortable in a situation in which he could be taken as a narcotics agent. He saw the article

as reinforcing his identity as a "real professor" and researcher. It was a moment of camaraderie. It took a year to regain the relationship with Ray that he had enjoyed earlier.

Dentan (1970) in talking about fieldwork in two different Semai communities talks about the number of *faux pas* he and his wife made in their early days with the Semai. He likens it to the kinds of mistakes that Semai children make. However, he writes "Since from the Semai point of view we were always rather inept, we could not help feeling very grateful and very affectionate toward them when they came to accept us as people much like other people." (88). Like our experience above, Robert Dentan (1970) writes: "I do not think I would ever have understood the economics of reciprocal food distribution among the Semai if we had not participated so fully in the system that I twice got into hot water for breaking its usually unspoken rules" (92). In one of these instances, after watching one of his Semai friends loading himself down with food from his limited supplies, Dentan asked the friend to wait a week or so before taking any more. The error, as Dentan later realized, was that he had obviously made an overt calculation of the amount of food his friend took. In the rules of reciprocity and food exchange, no such calculation should be made. The friend did not speak to Robert Dentan for several months, although he would come to visit Ruth Dentan.

As Whyte has emphasized: "It is important to recognize that explorations in the field are bound to confront one with confusing situations and conflicting pressures, so that some errors are almost inevitable, but few errors are serious enough to abort a project" (Whyte and Whyte 1984:11). The important thing is to learn from our mistakes.

Whyte describes his role in a local election in Cornerville, where like many of his informants he managed to vote several times for the local candidate in an election. While his Cornerville friends were doing the same, they felt it was inappropriate for Whyte to do so. In another instance, he used rough language, common in Cornerville, but again, his friends let him know that they might talk that way, but it was inappropriate for him.

In some settings, some mistakes can place the ethnographer in physical danger. Philippe Bourgois may have placed himself in danger by disrespecting Ray. Lincoln Kaiser (1970), in carrying out his study of the Vice Lords of Chicago, misread a cue in the behavior of gang members on the streets of Chicago and failed to leave the scene of a possible shooting. As it happened, no shooting took place, but Kaiser makes it clear that he several times felt that not understanding what was going on around him may have placed his life in danger.

Linda Kent (1992) found that a mistake barred her from a fieldwork setting with Irish Travelers (tinkers) in Mississippi. In previous research she had come to know Gypsies well and written a master's thesis on a Gypsy

headman. For her doctoral dissertation, she decided to study the Traveling People or Tinkers of Ireland and set out to do a preliminary study of Tinkers in the United States. She settled in Memphis and identified a community of Travelers across the border in Mississippi and began to gain access and build rapport. However, when a local television station in Memphis asked to interview for a human-interest story about Gypsies, she agreed. In the interview she was identified as an expert on Gypsies who had come to Memphis "to devote her life to the study of Gypsies." Two days after the interview was aired, she received a call from the local priest telling her that she was no longer welcome among the Travelers. Didn't she know, he asked, that the Travelers and the Gypsies are sworn enemies? If she was there to study Gypsies, why was she hanging around the Travelers? This event effectively ended her study of the Travelers in the Memphis area.

Mistakes in fieldwork are probably unavoidable. We just don't learn local expectations quickly enough to avoid them. As several of the cases above illustrate, however, making mistakes is often a vehicle to a deeper understanding of behavior and meaning.

In our experience in training fieldworkers, we have noted that the greatest fear of novice researchers is that people will not accept them or speak to them, or that people will be offended by the fieldwork. The experience of the majority of fieldworkers who use participant observation as a technique is exactly the opposite. Personally, it has always amazed us the extent to which people are interested in including us in events, telling us their stories, and how quickly rapport can be established in many settings. We strongly believe that most people underestimate the extent to which people value someone else's interest in their lives and the extent to which people enjoy being "teachers" to eager "students." Our admonition is that if you can overcome your shyness or fear of knocking on that first door, the rest is easy.

As we have seen, however, becoming a participant places the researcher in a unique research role, one where gaining rapport and partaking in a local setting—immersing oneself in a new cultural context—put unusual demands on the social skills and life of the investigator. The payoff is large, a much more nuanced and in-depth understanding of a complex setting than other methods of fieldwork alone can provide. But the cost may also be high. It includes the cost in time of developing the kinds of relationships that grow in trust and cooperation, reciprocity that is ongoing and personal, making mistakes in trying to fit into a new cultural setting, and the psychological costs of having to adjust to a new setting and then readjust on the return to home. Each researcher must find his/her appropriate balance and rhythm in participating in the life of another community, but understanding what to expect and some of the methods that others have used to find their way will help even the novice researcher to eventually find their way.

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NOTES

1. The availability of qualified collaborators varies across countries of course, but wherever possible we recommend recruiting local social scientists as research partners. We now rarely engage in fieldwork in another country without the real participation of local researchers as co-investigators. In several settings, we have long-term collaborators with whom we design and carry out research projects on a regular basis. Bill has even begun to insist on having local researchers as partners when doing consulting research (e.g., DeWalt, et al. 2000; DeWalt, Olivera, and Correa 2000).

2. Community-based participatory research (CBPR) is a "collaborative approach to research that equitably involves all partners in the research process and recognizes the unique strengths that each brings. CBPR begins with a research topic of importance to the community, has the aim of combining knowledge with action and achieving social change to improve health outcomes and eliminate health disparities." W. K. Kellogg Foundation Community Health Scholars Program (2010).

3. Wikipedia defines flaming as "hostile and insulting interaction between Internet users."

4. In *Palace*, Gods and Wizards are highly regarded members of the online *Palace* community.

5. In those cases in which substantial income can accrue from the study, the researcher may wish to consider sharing royalties with the community in an appropriate way. We know of writers who have funded scholarships for community students, contributed to community development projects, construction of parks, libraries, etc.

6. The sixteenth edition of *Ethnologue: Languages of the World* published by SIL includes over 6,900 language descriptions.

7. The term *Almao*, literally "German," is used by the Bakairi to refer to all non-Brazilian outsiders.

8. We have found that several different informants in several different settings have found it amusing to teach us words that, unbeknownst to us, have multiple meanings. The entertainment factor comes into play when we are prompted to use them in an ambiguous setting. Researcher in role of clown!

9. To prepare *aswa*, cooked manioc is chewed and mashed. The mash is left to ferment for up to seven days. The fermented mash is then mixed with water (from uncertain sources) and drunk, solids and liquid both. This is another acquired taste, and, furthermore, the water with which it is mixed is most often contaminated. Moreover, since this was a "mothers' day" celebration, the *aswa* had been prepared by men rather than women (YIKES!).

The Costs of Participation

Culture Shock

One of the hallmarks of using participant observation as a key technique in an unfamiliar cultural context is the experience that has come to be known as "culture shock." While classically referring to the culmination of unease felt by the participant observer as a result of not being able to successfully operate in a new cultural setting, getting all the cues wrong, dealing with not being able to anticipate proper behavior, dealing with behaviors of others that are, to the home culture of the fieldworker, inappropriate, shocking, dirty, immoral, or just plain different, but are perfectly acceptable within the context of the community in which the fieldworker finds herself, culture shock surely includes what we would call homesickness, and in many cases, the loneliness of leaving all loved ones behind for a while. But culture shock is more than homesickness and loneliness. The demands of trying to operate successfully in a very different cultural context exact an additional toll. For example, Goodenough (1992) describes the complex learning he had to master just to defecate politely on Onotoa.

Anthropologists Cora DuBois (1951) and Kalervo Oberg (1954) are credited with coining and popularizing the term "culture shock" in the 1950s, although, as we will see below, the syndrome they named has been a well-described hallmark of ethnographic fieldwork since the beginning of the enterprise. Although an anthropologist, DuBois introduced the term at a conference dedicated to international education. Interestingly, much of the recent research on culture shock as a syndrome has been conducted in educational and study abroad contexts (Ward, Bochner, and Furnham 2001).

Oberg discussed culture shock as "an occupational disease of people who have been suddenly transplanted abroad. Like most ailments it has its own etiology, symptoms, and cure" (1954:1). Oberg was prompted to discuss the

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issue of culture shock with expatriate women in Rio de Janeiro after working with a group of international volunteers on a health project, but was also presumably drawing on his own experiences as a migrant and anthropologist outside his native British Columbia. Oberg attributes culture shock to the anxiety that accompanies the loss of familiar signs and symbols (cues) of social intercourse when individuals move into different cultural contexts. Most of the cues of social intercourse are subtle and may be unconscious. The "sojourner"¹ is a "fish out of water" (Oberg 1954:1). He further argues that as a result of frustration and anxiety the sojourner experiences the new culture as culturally "bad" and the home culture is romanticized as "good."

Following such researchers as Hall (1959, 1966), Ward et al. (2001) identify a series of cultural differences in the way that people communicate that appear to be related to the feeling of being a "fish out of water" for the sojourner. These include differences in etiquette, such as the way in which questions are phrased and requests are refused; ways of resolving conflict, such as the differences in the ways in which people in collectivist and individualist cultures negotiate; nonverbal communication, such as mutual gaze, the degree of bodily touching, and the appropriateness of gestures; rules and conventions, such as punctuality and reciprocity; and forms of address. All of which have been shown to vary across cultural settings and to contribute to dis-ease in communications. Oberg suggests that the symptoms of culture shock include:

excessive washing of the hands; excessive concern over drinking water, food, dishes, and bedding; fear of physical contact with attendants or servants; the absentminded, far-away stare (sometimes called the tropical stare); a feeling of helplessness and a desire for dependence on long-term residents of one's own nationality; fits of anger over delays and other minor frustrations; delay and outright refusal to learn the language of the host country; excessive fear of being cheated, robbed, or injured; great concern over minor pains and eruptions of the skin; and finally, that terrible longing to be back home, to be able to have a good cup of coffee and a piece of apple pie, to walk into that corner drugstore, to visit one's relatives, and, in general, to talk to people who really make sense. (Oberg 1954:2)

Oberg also posited four phases of culture shock that, for many researchers, remain the basis for thinking about the way in which it unfolds. For Oberg, the stages are analogous to an illness:

1. The honeymoon stage in which the sojourner is "fascinated by the new";
2. The "crisis" in which the sojourner becomes hostile and aggressive toward the new context, and critical of the place and the people, often moving to stereotyping;

3. The "recovery" during which the sojourner begins to grasp the new context, to "open a way" into it; gains experience in getting around and communicating and jokes about previous hardships; and, finally,
4. The "adjustment," at which time, the sojourner becomes more competent and moves from accepting foods, drinks, habits and customs and begins to enjoy them.

Drawing on Oberg, a number of researchers studying culture shock among different types of travelers posit a U-shaped curve for the process of adjustment in which at the beginning the traveler is positive and euphoric, followed by a period of much discomfort and distress, and then again returning to a positive state. However, Ward et al. (2001), in a comprehensive review of the literature on culture shock among several different kinds of sojourners such as students studying abroad, missionaries, diplomats, and business people find little support in the literature for a U-shaped curve. For the most part, sojourners appear to be anxious and distressed early in the process and become less so with time. Some longitudinal studies even find that many sojourners experience an inverted U-shaped curve of experience in which they return to a higher level of distress after a period of improvement. However, it does appear that about 10 percent of sojourners do conform to Oberg's U-shaped curve for culture shock.

It appears that culture shock is common among all kinds of travelers, even those traveling for only a short time (tourists), although tourists are increasingly insulated from the conditions that foster culture shock through carefully scripted guided tours. However, sojourners who are participant observers experience several other sources of discomfort that contribute to the experience of culture shock. One of these is the feeling of always "being on," that is, playing the role of being the participant observer and never able to escape, always having to be alert as an observer, when the natural human role is to relax into being a participant. We have often had to repress our irritation when children in communities in which we have worked have stared at us for hours on end, sometimes hanging over house windows or outdoor shower stalls to see what we were doing. Another aspect of always "being on" is the result of being an identifiable outsider in a particular setting, often one perceived to have more resources than members of the local community. This is compounded in cultural settings in which asking those with resources to help out is acceptable. It includes being asked, sometimes continuously, for gifts of food, medicine, tobacco, transportation, and so forth. Paul Rabinow (1977), working in Morocco, describes how much easier his life became after his car blew up and he no longer had to give people rides or, worse, turn them down. Many ethnographers complain about the difficulty of dealing with demands on a daily basis. Finally, "being on" also includes the results of cultural and linguistic incompetence

of the neophyte. Some researchers tire of being laughed at or occasionally ostracized for their language and cultural mistakes.

Another element of culture shock for the participant observer is that the fieldworker is not just trying to live in a new setting, but also feels acutely the need to be accomplishing the task of research in this setting—a granting agency is paying for the work and expects a report, an advisor is asking for field notes, an employer is looking for the data, the researcher's degree and career may be on the line, and the researcher is paralyzed with loneliness and anxiety.

For the researcher, as for the conventional sojourner, the characteristics of culture shock include anxiety, depression, anger, and frustration. There comes a day when we just want to be able to defecate in private (Good-enough 1992), throw the toilet paper in the toilet, look another person directly in the eye, communicate effectively without being laughed at by the people with whom we are trying to communicate, etc. Just coping with the logistical difficulties of getting into a new context is a challenge that is often unanticipated by the novice fieldworker. Finally, being alone in the field, at least in the beginning of fieldwork, before rapport is developed, is as lonely as it gets (Mead 1970a).

Paul (1953) calls culture shock one of the costs of the participant observation method. Perhaps understating the extent of the experience for many fieldworkers, he notes:

The strains of making constant accommodations, of living in the public spotlight, of denying his own preferences, all deplete his patience. He may be able to conceal his exasperation from the people, but he cannot escape the unpleasant effects of suppressed resentment. The first weeks of field work are often trying. The investigator may want to quit and go home, staying only because he is ashamed to give up. (440)

Rosalie Wax describes her entrance into a Japanese resettlement camp in 1943. After an arduous two days of travel, Wax arrived at the Gila camp outside Phoenix in heat that she found "incredible." Alone, in the camp with little preparation for the field experience Wax finally found her room ("cell") by process of elimination from a number of other rooms. She writes:

It contained four dingy and dilapidated articles of furniture: an iron double bedstead, a dirty mattress (which took up more than half the room), a chest of drawers, and a tiny writing table—and it was hotter than the hinges of Hades. Since there was no one around to ask where I might get a chair, I sat down on the hot mattress, took a deep breath, and cried. I was too far-gone to be consciously aware that I was isolated or to wonder why I had left a beautiful and comfortable university town to stick myself in this oven of a concentration camp. 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 barracks that contained two toilets and a couple of wash basins. I washed my face and told myself that I would feel better the next day. I was wrong. (1971:67-68)

Wax goes on to eloquently describe the weeks of ostracism, despair, and perceived failure that followed her arrival at Gila. Few would speak to her, and when she did have conversations, she frequently insulted her potential informants, because she did not understand the conventions of conversation with Japanese Americans. She was clearly not trusted; lonely beyond belief; ready to leave at any moment and sure that the entire project was a failure. All the while the supervisor of the project she was supposed to be carrying out was demanding data. She writes: "Every time I returned to my stifling room after a series of futile 'interviews,' I just sat down and cried. I took long walks in the desert in temperatures of 110°-120°, and cried" (1971:72).

Wax eventually found a way to cope with the situation and to make some progress with her research while she became acculturated to life in the camp, and gained the trust of the residents. She moved from her original room to one even less comfortable in the ruder barracks to which the Japanese were assigned. She avoided the staff. She enlisted the help of another social scientist in the camp and found someone to introduce her to several people from whom she took "Japanese lessons" as a ruse to get to know them better. Finally, she devised a set of "red herring" studies. These were plausible concrete data studies, which, while they had little to do with her real research project, were innocuous enough to be nonthreatening to respondents. She became much more like a formal interviewer than participant observer. She developed structured questionnaires and interviewed women on how evacuation to the camps had affected their lives. She interviewed people on social stratification in Japan. The red herring studies allowed her to meet a number of people in a role they could easily understand. And she says "meanwhile I was gradually pushed into the role of a willing learner . . . friendly respondents now began to instruct me in some of the less confidential aspects and attitudes of center life as well as the rudiments of Japanese etiquette" (77). In the end, Wax found her work in the resettlement camps to be one of the most rewarding of her life. She then describes the difficulty of reverse culture shock upon reentry into her life as an anthropology graduate student.

Mead (1970a) also suggests the new fieldworker begin with more structured data collection to be able to show progress through the difficult first weeks. She writes: "The ability to register progress of some sort is often essential to the field worker's morale, and progress may be noted in counting up the thousands of words of vocabulary mastered, a tantalizing problem solved, a bad cut healed instead of turning into an ulcer . . ." (249). Even while describing the plight of the new and lone field worker she also notes

that a person alone in the field may, in fact, be incorporated more quickly into daily life, learning the language more quickly, perforce socializing with "villagers." It is easier for the community to take in a lone field worker, to feed and house him or her. Even Malinowski (1961 [1922], 1967) writes about the

feelings of hopelessness and despair after many obstinate but futile attempts had entirely failed to bring me into real touch with the natives or supply me with any material. I had periods of despondency when I buried myself in the reading of novels, as a man might take to drink in a fit of tropical depression and boredom. (4)

Indeed, his diary (Malinowski 1967) makes frequent reference to his reading of novels when he was feeling ill or despondent. Deborah Picchi (1992) also relied on novels for the difficult times, especially when she was not feeling well. However, as she had to travel by bush plane to the Bakairi Indian settlement in Mato Grosso, Brazil, she was only able to bring a few novels and had to ration them for the really bad times. We also have to admit to turning to novels at times to escape the community into which we had placed ourselves.

The first few weeks may not hold the only or even the worst patch of culture shock. Paul (1953) agrees with Oberg that, once the logistical problems are long over or at least adequately accommodated,

a letdown may set in long after the work has gotten well underway. The investigator may school himself to accept physical hardship, he may even gain ascetic satisfaction from enduring deprivation, only to be assailed unexpectedly with a craving for a shower, or a soft bed or a home-cooked meal. More insidious than the material discomforts are the petty and subtle aggravations of social participation. (440)

Sometimes a particular event acts as a trigger for culture shock (or more like a match in dry tinder). Mead talks about how she "burst into tears of helpless resentment when after sitting up all night with a very sick Balinese child, I went home for a moment, and came back in the chilly dawn of the mountain morning and was bitten by the family dog" (Mead, 1949:444-48).

While Wax attributes the depth of her despair on her first day and many subsequent days to a lack of experience, we have found that even after many years of fieldwork in several different settings, we still experience some form of culture shock in a new setting. However, it is less than it was in our earlier experiences and lasts a shorter period of time. Since we expect it, we have become rather adept at identifying the stages as we pass through them: initial elation at finally BEING THERE; loneliness; irritation that nothing works as it

should; anxiety that we just can't get our work done, the project will fail for sure; planning to abandon the project; and, finally, the feeling that "we can do this," "it's not so bad," "we are getting the data," "we can cope." . . . While it appears that some field workers never do become comfortable, most do and most, given enough time, find the experience good.

COPING WITH CULTURE SHOCK

Culture shock is a virtually universal experience for investigators pursuing the method of participant observation. As a result, a number of fieldwork narratives discuss the ways in which investigators have dealt with culture shock. Perhaps the most important coping strategy is to realize that the experience of intense anxiety and hopelessness is temporary. As Oberg noted to the expatriate women of Rio de Janeiro so many years ago, "There is a great difference in knowing what is the cause of your disturbance and not knowing" (4). Following Oberg and more contemporary culture shock researchers (Ward et al. 2001), knowing what to expect and understanding that there are cultural differences is one of the key strategies for coping. Today it is called cultural training, but it is in essence the notion of cultural relativity central to anthropology since its beginning.

While some fieldworkers never feel entirely comfortable in the field setting, most of us move through the period of culture shock in a reasonable amount of time. The day comes when the researcher finds that s/he looks forward to talking with neighbors, now friends, and has figured out how to meet the needs of everyday life with the resources available at hand, no longer feels like a fool in attempts to maneuver in the local social setting, and has figured out how to deal with constant demands. While there may well be periods of despair after this point, they can be dealt with. As we have noted earlier, and Ward et al. (2001) also note, some people embrace a new setting to the extent that they adopt it in place of their original one—they "go native."

As Wax and Mead note, it often helps to find research tasks that can be done easily, with little additional stress. As we have noted earlier, we have found that in community studies carrying out a census early in the process helps to introduce us to the community and offers a simple data collection exercise that can allow us to feel that some progress is being made.

While we do not suggest abandoning the field during the adjustment period (we do know the occasional researcher that never returns), it can be very helpful to plan one or more "vacations" from the field when the researcher can leave the community for a short period. Sometimes this is a trip back home to take care of business. Other times it is just a weekend in another town where the researcher does not have to be "on" all the time. In our initial

fieldwork experience in rural Mexico we were able to take an occasional weekend trip to Mexico City, about four hours away. There we hung around tourist sites and listened to tourists speaking English, ate hamburgers and french fries (We LOVE Mexican cuisine, but we are talking about *culture shock*, here), and took multiple hot showers. A vacation from the field can also allow for a more objective review of notes, other data, and a bit of analysis. Not all research settings (for example, when you are working on a Micronesian atoll) allow for a weekend trip away. Sometimes it takes more effort to get away from the field, but it can be an effective way of coping. Also as we have noted, the researcher often finds after returning from a trip away, that he or she is greeted as a long lost friend or relation. It often marks the deepening of rapport and the relationships with the community.

Finally, as with the hamburgers in Mexico City or the Pop's ice cream shops in Honduras, cultural comfort food helps. There are some foods we try to take with us and ration out over time. For many anthropologists of our generation, this includes peanut butter. We have found that anthropologists working all over the world have reinvented all of the peanut butter based recipes known. We were able to drive to fieldwork in Mexico and took a case of Campbell's chicken noodle soup (again, it is not about cuisine; it's about comfort). Kathleen still usually packs a couple of bags of M&Ms in a suitcase.

Having a companion or family in the field may help. While it is true, as Mead notes, that the needs of a nonprofessional companion may increase problems, and some of the greatest sources of anxiety for us in the field have been the logistics of trying to meet the needs of children with local resources, couples and families may find that some members of the family have an easier time of integration into the community bringing along the rest of the family. Dentan notes that his wife, also an anthropologist, eased his own entrance into Semai society.

Novels DO help. Kathleen prefers science fiction. Bill likes mysteries and novels of place.

✓ In coping with culture shock, many people have often assumed that having a significant other and/or a family in the field is a great advantage. In our experience, as the following section shows, a family can be both a source of solace and familiarity and a hindrance in doing research.

PARTICIPATING AND PARENTING: CHILDREN AND FIELD RESEARCH

While fieldwork is traditionally portrayed as a solitary endeavor, in reality many researchers bring their families, including their children, to the field with them. The presence of children in the field shapes the research

experience in a number of distinct ways. Children can help ease the loneliness and isolation characteristic of fieldwork in foreign cultures. However, children also present a number of challenges to researchers in the field. As part of the trend to demystify anthropological fieldwork, a number of researchers have written about their experiences with children in the field. While each fieldwork experience is unique, a number of themes emerge regarding the effect of children on participant observation.

Many researchers report that their children had a positive impact on participant observation. Bringing children to a field site can lead to increased rapport with the research community. A solitary researcher showing up in a remote area to live alone for a period of a year or more may seem extremely bizarre in many cultures. In most cases, the people who are being studied are able to relate more easily to a researcher living with his or her family. Mimi and Mark Nichter (1987) believed that the presence of their young son made it easier for villagers to relate to them during their research in a rural Indian village. Bourgois (1995) describes how his son's cerebral palsy was diagnosed in a clinic in El Barrio, and that his son's ability to negotiate the neighborhood, rolling his walker over trash and crack vials in the streets of El Barrio, helped to establish Bourgois as a community member.

Also, the presence of children accompanying a researcher can signify his or her adult status. In many cultures a childless adult, especially a married childless adult, may be viewed as strange, dangerous, or the object of pity. During our first field experience in Temascalcingo, it was a concern for many people that we had been married for more than a year without having a child and without signs of Kathleen being pregnant (see also Klass and Klass 1987). During their first field experience in the Sudan, Carolyn Fluehr-Lobban and Richard Lobban (1986, 1987) reported that people had trouble accepting their status as a married couple because they had no children. Many Sudanese doubted that they were truly married but their return to the field ten years later with their daughter Josina reassured their friends and acquaintances.

Bringing children into the field can also open new areas of information to the researcher. Most researchers who bring young children in the field often receive a constant stream of advice about childcare from friends and neighbors. While an overabundance of friendly advice can be exasperating, it can also teach researchers about the culture they are working in. Mimi and Mark Nichter reported that they gained valuable insight into rural Indian ideas about child development from villagers' comments made about their son's "constitution" (Nichter and Nichter 1987). This advice can also challenge the researcher's unexamined cultural biases and assumptions. Renate Fernandez (1987) learned that children sleeping alone in their own room was viewed as a type of social deprivation in rural Spain. Researchers can also learn about a culture from the way people react to their children. When

informants are enculturating children, they are also teaching the researcher about their culture. During Diane Michalski Turner's (1987) fieldwork in Fiji, the villagers with whom she lived devoted a lot of time teaching her two-year-old daughter to become Fijian. By watching how villagers interacted with her daughter, Michalski Turner was able to learn not only how one becomes Fijian, but also about Western/Fijian power relationships.

Children can also help gather data that is inaccessible to adults. G. E. Huntington (1987) reports that her nine-year-old daughter was an invaluable source of information about Hutterite children's informal culture. As a result, Huntington learned how Hutterite children engage in very different behaviors in front of adults and when they are among other children participating in their own culture.

Bringing children into the field, however, also has its disadvantages. Some researchers report that the responsibilities of childcare forced them to miss out on certain opportunities. Reflecting on her research in Jamaica, Joan Cassell (1987) relates how she frequently missed nighttime events because she felt compelled to stay home with her two children. Young, unruly children can also disrupt meetings and interviews. In Nancie Gonzalez's (1970) account of her research in Guatemala she talks about how her young son spilled soda pop on the president of the university's rug which did not bode well for the rest of the meeting. Perhaps the biggest disadvantage of bringing children to the field is the amount of time that researchers devote to child care (and hence lose to the field). Many researchers, especially those who are solely responsible for child care, report that the presence of children severely curtailed the amount of time they could devote to field work. Melanie Dreher, who brought three children to rural Jamaica to conduct post-doctoral research with her, states, "I suspect it took me twice the time to accomplish half the work that I would have normally accomplished" (Dreher 1987:165). During fieldwork among an indigenous tribe in the northwest Amazon, Christine and Stephen Hugh-Jones (Hugh-Jones 1987) had to devise alternating fieldwork schedules so that one of them would always be available to supervise their two children.

Our personal experiences with children in the field have been generally quite positive. Our two fair-haired children were an instant magnet everywhere we have traveled in Latin America and led to the opening of many doors for us. We also found that, early in our careers when we were relatively poor graduate students or assistant professors, we could more easily afford childcare and household help in Mexico or Honduras than we could in the United States. We did have some unpleasant brushes with illness, but, in the end, nothing of long-term consequence. We know of others who have had seriously ill children and have even lost children in the field through illness or accident (see Howell 1990). More importantly, as our children moved into their teens and began to have obligations and wishes

of their own, it became more difficult for us to take them to the field. More to the point, it was not worth the complaining to which we were subjected. During these years, we began to schedule our time in the field separately so that one of us stayed at home with the children, while the other was engaged in doing field research. A telling incident reminded us of some of the difficulties of children in the field. After our youngest child was a teenager Kathleen returned from several months of fieldwork in coastal Ecuador saying it was the best field trip she had ever had. When Bill asked why, her first reply was that it was the first time in the field that she did not worry about children, whether they were in the field with her or left at home. It must be said, that while we enjoyed having our kids in the field it was a big relief to carry out fieldwork without having to attend to the needs of children.

Whether children help or hinder participant observation depends on a number of factors. It seems that there is a significant effect depending on whether the researcher is returning to a field site or arriving for the first time. Most researchers who are arriving in a field site for the first time with their children seem to experience more problems than veteran field workers (Cassell 1987; Michalski Turner 1987). The age of the children also shapes the field experience. Very young children, while requiring more care, adapt more readily and experience less severe culture shock (Fluehr-Lobban and Lobban 1987; Nichter and Nichter 1987). Older children seem to have a more difficult time adapting to new and foreign cultures (Scheper-Hughes 1987). The field situation itself also shapes the experience with the children. Bringing children to a field site where they already speak the language is easier on both the children and the parent than introducing them to a culture where they are only able to communicate with their family (Cassell 1987; Hugh-Jones 1987). The presence of another parent to share childcare responsibilities certainly facilitates a fieldworker's time in the field with his/her children (Fluehr-Lobban and Lobban 1987; Scheper-Hughes 1987).

REVERSE CULTURE SHOCK (REENTRY SHOCK)

Finally, keep in mind that "return culture shock" is also a problem. Wax writes that when she was beginning work on *Doing Fieldwork* (Wax 1971) she asked a young colleague recently returned from fieldwork in Melanesia what was the most important thing to tell students who had never been to the field. He replied: "Tell them to keep a personal diary in addition to their field notes and tell them that returning to your own society can be as difficult as trying to enter a strange one" (Wax 1971:172-73).

Ward et al. (2001) discuss a growing literature that confirms that reverse culture shock, sometimes called "reentry shock," is extremely common in conventional sojourners. Judith Martin (1984) suggests that one of the

reasons for reverse culture shock is that it is unanticipated. New travelers rarely anticipate that they will experience distress upon returning home. Subsequent research has supported this hypothesis (Ward et al. 2001). In addition, there are some demographic differences in the experience of reentry shock. Women are somewhat less likely to experience it. Older people are also less likely to experience reentry shock, and adolescents appear to be the most vulnerable.

In our experience, readjusting to life in North America after extended fieldwork in economically marginal rural communities in Latin America includes a period of time in which the everyday activities of life seem wasteful, too quickly paced, and unresponsive to the "real problems" of the world. (All of which, upon reflection, is true in our everyday North American lives.) Furthermore, many seasoned researchers, and new researchers, once they get over the anxiety of being in the field, find field research to be a very enjoyable break from the kinds of work we do while not in the field. We find fieldwork energizing and intellectually exciting. It helps us gain a perspective on our own lives, to realize that the academic politics that swirl around us are petty and insignificant, and that most of the problems of North Americans are internally generated rather than externally driven. We experience a "let down" upon return to home base.

Wax reports that it took her several years to reengage with her life as a graduate student after her time at the relocation camp during World War II. We find with graduate students returning from the field that the most obvious result of reverse culture shock is an inability to get down to analysis and write-up of research materials. More experienced researchers seem to move through the readjustment period more quickly. Now, it is true that many researchers have a bit of trouble getting to the analysis after the data is collected. These are two very different kinds of tasks. However, the reentry into the home setting after immersion in another cultural setting seems to add significantly to this period. For most, the period of readjustment to home is a few weeks or months. But it does occur, and the wise researcher anticipates this phase of research as well.

NOTE

1. Oberg used the term "sojourner" to describe people, like researchers, who are in a different cultural context for a limited period of time and expect to return home at the end of a defined period. The term has been picked up in the more general literature (e.g., Ward et al. 2001) to refer to such travelers as international students, business people, diplomats and some missionaries, as compared with short-term travelers such as tourists and permanent travelers such as immigrants and refugees.