The Art of Fieldwork

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FIELDWORK: THE BASIC ARTS
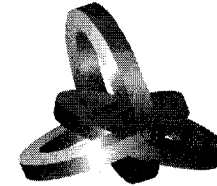
There may be kinds of information that are in fact vital to the task of anthropological analysis but that are fairly consistently excluded from our field notes—in other words that we have conventional criteria for identifying observations as data that are inappropriate for the kinds of hypotheses and theories we wish to develop in our analysis. The frequent assertion that anthropology is an art as well as a science might depend precisely on the unsystematic or unreflecting way in which we accumulate part of our basic data.

—Fredrik Barth
preface to *The Social Organization of the Marri Baluch*, p. x

This chapter is as close as I come to presenting a fieldwork manual. It brings me perilously close to dwelling on the techniques and strategies of fieldwork as craft. However, I focus on the less systematic aspects of the experience rather than on data-gathering per se. Behind every strategy or technique employed in fieldwork there needs to be sound human judgment—an artistic decision guided in large measure by what passes as ordinary courtesy and common sense. I have made "Courtesy and Common Sense" my first subheading, to highlight some pervasive elements in fieldwork before dealing with topics more customarily addressed in such discussions. Under the unconventional subtitles "Being There," "Getting Nosy," and "Looking over Others' Shoulders," I review fieldwork's major dimensions: participant observation, interviewing, and archival research.

Courtesy and Common Sense

On first thought, participant observation would seem to be the obvious choice as a starting place for discussing the basic arts involved in fieldwork.

On second thought, focusing on participant observation hopelessly confuses whatever is unique to fieldwork with the display of everyday courtesy and common sense.

A fieldworker can easily offend through inappropriate behavior, comment, or question. Fieldworkers are not clairvoyant, and they, too, are subject to making social errors. Thoughtful explaining to get out of a tight or embarrassing predicament that one shouldn't have gotten into in the first place is certainly not an art limited to researchers. Nor are those who do fieldwork necessarily gifted in the handling of human relations. I have heard colleagues reportedly successful at fieldwork ask rhetorically, "Can you imagine me doing participant observation?" and a voice inside me whispers, "Well, frankly, now that you mention it . . ."

Presumably the human-relations aspect of fieldwork is enhanced for those to whom such qualities as empathy, sympathy, or everyday courtesy and patience, come naturally. I see no evidence that such qualities can be taught or that they are particularly abundant among the practitioners of certain disciplines to the exclusion of others. For example, the consequence of anthropology's supposed humanizing message seems not, in my experience, to be any more or less evident in the everyday behavior of anthropologists than of ordinary folk. If it were, then to be a member of an anthropology department would be the envy of members of every other department on the campus.

The idea of participant observation, which James Clifford characterizes as a predicament transformed into a method (1988:93), can raise a straightforward question: How does one go about being artful when assuming so obvious a role? I recall a senior colleague in the 1960s who flatout rejected any proposal he was asked to review that explained, or attempted to explain away, the question of method with the simplistic response "participant observation." Michael Moerman, writing in the heyday of postmodernism, has observed that participant observation, "once anthropology's secret shame," had subsequently become "the fashionable focus of its self-absorption" (1988:68).

Nevertheless, participant observation will surely continue to occupy the preeminent role Russ Bernard ascribed to it, not only as the foundation of fieldwork but as the foundation of cultural anthropology (1994b:136). It is all-encompassing as a method, yet it is not really a method at all. Rather, as Bernard explains, it is a strategy that facilitates data collection in the field. And it is a strategy that covers all kinds of data collection, quantitative as well as qualitative, for as he notes, "All participant observation is fieldwork, but not all fieldwork is participant observation" (p. 137). Administering a structured interview, for example, or observing patrons in a market may require fieldwork, but they do not require participant observation.

Employing participant observation as a strategy in qualitative research requires common sense. It needs to be examined in terms of what brings fieldworkers into a setting in the first place and whether they are well situated to observe what they hope to observe. This is where many qualitative researchers get off on the wrong foot, hoping that simply being there will enable them to observe or experience what they are interested in observing and experiencing. A first question to ask is, Can whatever I want to study be seen by a participant observer at all? And, if so, am I well positioned to see what I hope to see? These questions need to be followed by another: What are my own capabilities for participating and observing in this situation? Many descriptive studies pursued through participant observation have elected a time-consuming approach with only an outside chance that the researcher proposing them will ever have the opportunity to see whatever purportedly is to be observed.

I remember talking with a student years ago who had heard of an Alaskan village where television was about to be introduced. Intrigued with the possibilities of ethnographic inquiry and the tradition of village studies, the student asked whether I thought ethnography would work as the appropriate research strategy for a study of the impact of television on village life and, if so, how I would approach it.

My personal reaction was, Why bother? The broad sweep of a community study did not seem warranted with such a narrowly focused question. With a well-funded project one might assign an ethnographer to every family, or, lacking such generous resources, one might assign a lone researcher to any household willing to have a longtime observer. In either case, the purest observer would not want to influence the results and

therefore would be hesitant to describe the study as one about TV's impact. Yet a live-in observer in a village household might prove far more entertaining than TV fare, the researcher's presence creating the very kind of distraction that dedicated participant observers try to avoid. It looked to me like a low-yield investment of researcher time to catch a few possible comments and to record some TV watching. Even then, at the end of the year, how would anyone actually assess impact? The proposed project seemed to illustrate what Fredrik Barth has described as a tendency to confound process and change (1994b:76).

Granted the village had been without TV before, but was the occasion for introducing it all that interesting? It was not the inefficiency of the research strategy that bothered me so much as the mismatch between the magnitude of the problem and the resources that would be assigned to study it. A year devoted to a study of village life in modern Alaska (or anywhere) ought to be a provocative experience and rich source of data. A commitment of that sort seemed to warrant a more imaginative scope of work than tracking TV viewing and attempting to assess—or guess—its impact. I gently asked whether the student could think of any other ways to get relevant information if the social impact of TV was the burning issue.

Another example illustrates the complex crossover (or heavy residue) from tightly designed quantitative studies to the creative use of qualitative ones. This time, sampling was the bugaboo. A student in a seminar I was presenting overseas was interested in studying what she called "discovery learning." In my suggestion that participants engage in some modest field research during the seminar, she saw an opportunity to try her skills at classroom observation. But she had become distraught over a major obstacle she foresaw, and she made a special appointment to discuss it with me. "I have always understood that any school or classroom in which I do observations must be selected by random sampling," she explained. "What if the school and teacher I happen to draw isn't using discovery learning?" Her faith in sampling procedures was as profound as her misunderstanding of when to apply them. Common sense should have guided her to a setting where she was likely to find the phenomenon of interest; questions of frequency and distribution were beyond the scope of her proposed inquiry.

I was intrigued that the student felt so rigidly bound to sampling procedures in spite of the fact that hers was to be an exploratory study. It sig-

naled that my explanations about qualitative research were not powerful enough to dispel her previously held beliefs about how research is supposed to be conducted. There was room for some teaching here, but there was also a challenge for me to try to learn what I could about the beliefs associated with research from my seminar participant. Might that be where the real art is in all inquiry: recognizing what might be learned as situations present themselves? If so, then, as anthropologist Mariam Slater once caricatured it (1976:130), whether or not you eat soup with a chicken head floating in it is rather incidental to the business at hand. What counts in fieldwork is what is going on in your mind.

Even to describe participant observation as a strategy may be going too far, except to prompt researchers to seek an opportune vantage point for seeing what they want to observe. The element of strategy turns on two complementary questions that need to be reviewed over and over:

- Am I making good use of this opportunity to learn what I set out to learn?

- Does what I have set out to learn, or to learn about, make good use of the opportunity presenting itself?

What is going on in the researcher's mind is critical to all this. If nothing is going on, not much is likely to come out of the experience except experience itself, with a possible residue of "empathy, a rapport high, and headnotes," in Roger Sanjek's terms (1990:238). This is not unlike actors whom we criticize for simply mouthing words rather than getting into their roles. (I address this issue more fully in part III.) It may seem strange here to separate mind from body, but the distinction helps to underscore the difference between what others observe us doing as we go about fieldwork—how we get around and conduct ourselves—from what is going on in our heads as we do it.

The way researchers move their bodies around does not make art out of fieldwork. Nevertheless, one can offer suggestions as to how to move about with sufficient grace to be received graciously by those with whom we hope to interact. I can identify at least four areas of social behavior that seem especially important for the successful and satisfactory conduct of fieldwork—its performance aspects, if you will. Alone or collectively

they are no more than the demonstration of everyday courtesy and common sense:

1. *Gaining entrée and maintaining rapport.* These two terms, joined so often as to have become a single and often trite phrase in fieldworker accounts, mask a great deal of the angst associated with fieldwork, especially among those who have never done it and who worry that they may not be successful in achieving its personal dimensions. I remember a young graduate student in anthropology who returned from a difficult (not impossible, just difficult) year of fieldwork in the Canadian Far North. He was anxious to communicate to his fellow students not only how terribly important this aspect of fieldwork was but also that these were critical aspects for the duration of fieldwork, not just a pair of tasks to be attended to first thing on arrival.

   Maintaining rapport presents a continuing challenge through the presence of an intrusive and inquiring observer forever wanting to know more and understand better. The long-term nature of fieldwork and the likelihood of both physical and emotional/intellectual isolation exacerbate interpersonal tensions. Fieldwork can be its own worst enemy; I know because I've been there. No one was stealing my mail during the year of my induction into fieldwork as village teacher. There simply were times when there was no mail to bring or only unimportant mail when important mail hadn't been sent. A couple of families were regularly relieving the school of a few gallons of fuel oil; I needed to maintain perspective more than I needed to maintain rapport, for I had not been sent to the village as an agent of the government with a primary responsibility for safeguarding the school's fuel supply.

2. *Reciprocity.* There is an art to gift giving. There is something of an art to gift receiving. These arts are by no means unique to the conduct of fieldwork, but fieldwork entails a subtle kind of exchange, one that often involves gifting across cultural boundaries where exchange rates may be ambiguous or one wonders what to offer in exchange for intangibles such as hos-

pitality or a shared life history. For example, whether, and how much, to pay key informants always presents problems. Grant-rich investigators are concerned that they may offer too much; resource-poor graduate students are concerned that any payment at all is a further drain on already overtaxed resources. Employing local field assistants or choosing a dwelling to rent or a family with whom to reside invariably puts researchers at risk of siding with factions or otherwise being accused of being partial, parsimonious, or extravagant—and perhaps all of these at once.

Conventional wisdom cautions fieldworkers to remain as neutral as possible, especially when new to a site, but even that option is not always open in the field. Conversely, one must learn how to manage being put upon by those who recognize the fieldworker's inherent vulnerability to requests when success depends on being able to make requests of others. If as fieldworker I am unsure what I may need from you by way of help or information at some future time, I have to be cautious in turning down requests you make of me at present. I dare not fully reveal how vulnerable I feel, lest you impose unduly. Such decisions are not made easily. Along with extending the depth of one's understanding, long-term commitment extends both the depth and the duration of one's vulnerability.

One-shot interviewers or pollsters have it easy. At most, they may be hit up for a cigarette or a ride to town. They don't stay around long enough for requests to start escalating, as they inevitably do over time. Questions such as whether to pay a standard rate for interviewee time ought already to have been worked out as a matter of project policy. Requests for food, money, medical assistance, or a job can put a resident fieldworker in an awkward bind; damned if you do, damned if you don't. In the abstract, a firm policy seems advisable ("Sorry, I just don't loan money—to anyone"), but in the world of diplomacy, everything remains negotiable, and fieldwork unquestionably requires the art of diplomacy. One seeks knowledge in the professional role of researcher but prays for wisdom in the personal roles that make it possible.

3. *A tolerance for ambiguity.* Another admonition that becomes trite in the saying, but essential in the doing, is to remain as adaptable as possible, to exhibit a tolerance for ambiguity. In terms of priorities, perhaps this point deserves first mention, yet one can hardly claim that all fieldworkers exhibit it or that only fieldworkers need it.

There is no way anyone can prepare another person for all the vagaries of fieldwork, any more than one can train or prepare another for the vagaries of life. Of course, there is no way one can pass on to another the quality of tolerance, either; merely mouthing it does not make it so. But there have been times in my own fieldwork (and life) when, with nothing more than the cliché to sustain me, I have managed to eke out just a bit more patience than I thought I could muster. Someday the admonition to develop a tolerance for ambiguity may be helpful in your own work (and life). Simply suppressing a too-hasty comment or reaction is a good step in this direction.

Fieldworkers would hardly go wrong to take tolerance for ambiguity as their professional mantra if it is not by nature a personal one. I have seen it treated exactly that way in a summer workshop designed to help prepare teachers for assignments in the Alaskan bush. I was not able to think of any other phrase that might someday prove more helpful. The workshop instructor used the expression so often that participants groaned every time he repeated it, and they presented him with a special T-shirt with that slogan on it. By the following winter, I assume that his message took on more significance as daylight hours and patience shortened, and the realities of bush living began to take their toll.

I have heard the phrase "life shock" in reference to a related problem. Those of us who make our entry into the real world via protected mainstream lives and respectable academic routes—the usual pool from which fieldworkers are recruited—are not necessarily well versed in the harsher realities associated with life itself. During the years we spend in the library studying about life, most folks are actually knocking about in it. We may never have witnessed anyone dying, the sort of thing gen-

teel folk do in hospitals, out of sight. We are even less likely to have witnessed a birth, especially in my day. The ragged and deformed may also have remained out of sight. All those statistics we read—poverty, illness, accidents, violence, abuse—may suddenly materialize for a fieldworker whose most traumatic experience to date has been a ticket for speeding.

The ambiguity comes in the meaning of human life, which proves not to be so universally revered as we have been schooled to believe. "How many children do you have?" you inquire of your Ndebele informant in southern Africa. "Six, maybe five," he responds, leaving you to wonder if he really does not know how many children he has. But that is exactly why he has answered with such calculated ambiguity. When he last saw his children, there were six. In the interim, something may have happened to one of them, even if they all were okay this morning. One does not want to provoke fate by taking anything for granted.

Not even natural disasters—fires, floods, earthquakes—shake us from our Western belief, or faith, that we are essentially in control. Our language comforts us: fireproof, earthquakeproof. Foolproof! Fieldwork can sorely test the belief that we exert such control. A tolerance for ambiguity is an essential element in the art of participant observation.

4. *Personal determination coupled with faith in oneself.* Self-doubt must be held in check as you go about your business of conducting research, even when you are not sure what that entails. In part this means being able to maintain balance in the face of what anthropologists have termed *culture shock*. Michael Agar describes culture shock this way:

> The shock comes from the sudden immersion in the lifeways of a group different from yourself. Suddenly you do not know the rules anymore. You do not know how to interpret the stream of motions and noises that surround you. You have no idea what is expected of you. Many of the assumptions that form the bedrock of your existence are mercilessly ripped out from under you. [1996:100]

And that's only half of it, because whatever shocks you probably was not what you originally set out to understand. The complexity of your task grows before your eyes; you want to understand more and more as you realize you understand less and less. At such times you cannot help wondering if any fieldworker before you has confronted anything quite like this!

Rest easy—no one about to undertake fieldwork can ever anticipate exactly what will be encountered or exactly what is to result from the experience. If we could, there would be no point in doing research this way, for our studies are constructed in the doing. Even hard-nosed experimentalists recognize, as Ludwik Fleck observed seventy years ago, that if a research experiment were well defined, it should be altogether unnecessary to perform it (1979[1935]:86). The more that is known about a topic, the less likely a qualitative broadside of the kind that results from fieldwork is well suited to explore it further. There is a becoming level of uncertainty in this work, and you must be prepared for the unsettling experience of constantly having to reset your course.

Should you feel so baffled by what confronts you that the only recourse you see is to record everything, you will realize that certain "everythings" take precedence over others. What do you see and hear that strikes you as important? How might you direct the attention of a newcomer to this setting? How can you best distill its essence for a reader who will only be able to see through your eyes or hear through your ears? Description is the starting point, square one. You need never be at a loss as long as you remember you can always go back to description when you feel overwhelmed.

Being There

Used in its broadest sense, participant observation is so all-encompassing that it can refer to virtually everything that qualitative researchers do in pursuing naturalistic inquiry, that cultural anthropologists do in pursuing ethnography, that sociologists do in pursuing a field study, and so forth.

Here I use participant observation in a somewhat narrower sense that makes it the complement to interviewing rather than inclusive of it. That still leaves it to cover any field activity not specifically related to some form of interviewing. Its essence is captured, although oversimplified, in the phrase "being there." In a chapter with that title, Clifford Geertz offers a lighthearted image of the proper role of the fieldworker:

> What a proper ethnographer ought properly to be doing is going out
> to places, coming back with information about how people live there,
> and making that information available to the professional community in
> a practical form. [1988:1]

Somewhere between "going out to places" and "coming back with information," every fieldworker has to achieve a workable balance between participating and observing. There is always a question of whether those two processes constitute discrete functions or are hopelessly intertwined in the very act of anyone being anywhere, but it is comforting to have our own special label for what we do to reassure ourselves that our being there is different from anyone else's. That self-conscious role is what we examine when we discuss participant observation—how we can realize the potential not simply of being there, but of being so agonizingly self-conscious about it.

How to participate effectively, how to observe effectively (especially that), how to keep the one from interfering with the other, and how to get others to act naturally while we try to appear nonchalant about our own presence—those are the confusions and challenges of the participant dimensions of the participant observer role. They, in turn, are confounded by the perennial problems of the process of observation. Those include what to look at, what to look for, and the never-ending tension between taking a closer look at something versus taking a broader look at everything.

Many sources are devoted to the topic of field observations and participant observation (e.g., Adler and Adler 1994, Bernard 2000, DeWalt and DeWalt 2002, Jorgensen 1989). In a paper titled "Confessions of a 'Trained' Observer" (HFW 1994a), I have joined these efforts to demystify that which cannot necessarily be explained. My purpose was to help neophyte fieldworkers recognize what the problems are, rather than to offer

simplistic solutions for resolving them. Each of us addresses the problems in specific ways in specific cases; there are more-or-less appropriate adaptations, not definitive answers. But no old-timer is going to forsake an opportunity to offer a bit of advice. My suggestions here underscore the dilemmas and inventory the options that confront the participant observer.

Doing Better Participant Observation:
Using Participant Observation Better

- *Focus your observations.* You may tell others you are just observing, and doing so may satisfy their curiosity, but do not believe for a minute that there is any such thing as just observing. A lens can have a focus and a periphery, but it must be pointed somewhere; it cannot see everywhere at once. Kenneth Burke's aphorism reminds us, "A way of seeing is also a way of not seeing" (1935:70). Our marvelous human eye has its scotoma, its blind spot; the analogy to fieldwork has been duly noted (see, for example, Crapanzano 1980:ix).

  When you are not sure what you should be attending to, turn attention back on yourself to see what is it you are attending to and try to discern how and why your attention has been drawn as it has. What are you observing and noting; of that, what are you putting in your notes, at what level of detail; and at what level are you tracking your personal reactions to what you are experiencing? Kleinman and Copp (1993) suggest that note taking is not complete until you go back over your notes to make notes on notes. The point is to ensure that you are coupling your analysis to your observations (rather than putting that task off until later) and to help you remain attentive to your own processes as a human observer. Don't worry about all that you are not getting; focus on what you are getting. Observe yourself observing.

- *Constantly review what you are looking for and whether you are seeing it (and if not, whether you are ever likely to see it).* You may need to refocus your attention to what is actually going on, and discard some overconceptualized ideas you brought into the field (such as "watching" decision making or "observing" discrimination). Begin by looking for recurring patterns or underlying themes in behavior or action. That should include patterns of things not happening as well as things that are happening. The latter kind of observations are most likely to be made comparatively: "Back home this would be a major source of stress, but here no one seems concerned." You will probably catch yourself becoming prematurely evaluative, particularly when righteous indignation tells you what people should be doing but are not. In case you don't recognize it, that's culture at work. But it's your culture, not theirs! Tracking your own "shoulds" and "oughts" may provide valuable insight into your processes as an observer.

  Another kind of comparative question that can help focus your observations is to reflect on what a fieldworker of another persuasion within your discipline, or schooled in a different discipline entirely, might find of interest in a setting. Take the economist's concern for the allocation of scarce resources, for example. Questions addressing the distribution of resources can prompt fresh insight for a fieldworker who may not have thought about what is in short supply in a seemingly affluent community (for instance, time) or what seems to be in abundance (perhaps time, once more) in a community stretched for material resources.

  In opportunities for fieldwork, watch also for recurring themes in your own evolving career that lend focus and continuity to it. A common thread running through my own work is a focus on cultural acquisition. In any setting where I am an observer, I find myself asking, What do people (individually, collectively) have to know in order to do what they are doing here? And how do they seem to be transmitting or acquiring that information, especially in any absence of didactic instruction?

- *Be prepared to discover that observation itself is a mysterious process.* At the least, it is something we do off and on, and mostly off. No one can remain acutely attentive for long. We compensate for that by averaging out our observations, reporting at a seemingly

constant level of detail that implies we are keener at this than we are. A realistic approach for the fieldworker is to recognize and capitalize on the fact that our observations—or, more accurately, our ability to focus on them—are something comparable to a pulse: Short bursts of attention are followed by periods of inattention or wandering.

Capitalize on the bursts. Be especially observant about capturing little vignettes or short (but complete) conversational exchanges in careful detail. For example, you could never capture all the conversation you hear, and you would neither want nor ever need to. But what conversation you do record needs to be recorded in sufficient detail that you can report it verbatim. Beginners often gloss their observational efforts in a way that leaves them with no reportable data. Every statement they record is paraphrased in their own words, rather than in segments of conversation as actually spoken. A guideline I suggest is this: What you do record, record in sufficient detail that, should the need arise, you can report it directly from your notes. I am not suggesting that you actually report that way— field notes don't usually make for great reading—but I urge you to make a record of pertinent information at that level of detail. Otherwise, why bother?

- *Assess your participation, your observations, and the information you are recording in terms of what you will need to report rather than the type of data you feel you ought to gather.* (For more on this idea of remaining goal-oriented, see chapter 9.) Keep asking yourself how you intend to use whatever data you are recording and whether you are recording it in a usable format.

- *Reflect on your note taking and subsequent writing practices as a critical part of your fieldwork work.* There is a balance to be struck with writing up field notes. For some observers, note taking is one (and perhaps the only) activity in which they feel they are really doing research. They may be tempted to overwrite because of the satisfaction note-making brings. I worry about them less than I worry about those who resent the time they must devote to writing and who procrastinate, thus mak-

ing the task increasingly formidable. If you are one of the latter, I suggest you try to discover how short you can make entries that nonetheless satisfy you for their adequacy, and then find a way to maintain that level of note-making as part of your daily routine (e.g., finishing up yesterday's notes while having your second cup of morning coffee).

However you approach it, you must make note-making sufficiently doable that you will always do it, rather than ever put it off. It may prove to be a chore, but it need not become a dreaded one if you follow the simple rule of keeping your entries up to date. There isn't much sense to going out and getting more information if you haven't digested what you took in last time. (For more on field notes, see Bernard 2000; Emerson, Fretz, and Shaw 1995; Sanjek 1990.)

Recognize that regardless of how much you write, most of what you observe will remain what Simon Ottenberg calls "headnotes" (1990:144–46). But some observations will make it into written jottings, whether simple or elaborate, and those jottings will prove invaluable. Your elaborated note-making also provides a critical bridge between what you are experiencing and how you are translating what you observe into a form you can communicate to others. Make a practice of including in your notes not only standard entries about day, date, and time, accompanied by a simple coding system for keeping track of entries, but also reflections on and about yourself—your mood, personal reactions, even random thoughts. These may later help you recapture detail not committed to paper but not lost, either.

Note taking is not the only kind of writing for you to consider at this stage. There is something temporary about any kind of notes that effectively says the real writing will come later. What is to prevent you from doing some of that real writing as fieldwork proceeds? Instead of putting everything in an abbreviated note form, take time to draft expanded pieces written in rich detail in such a way that they might later be incorporated into your final account. Disabuse yourself of the idea that as long as you are doing fieldwork, note taking is the only kind of writing you should do.

The key to participant observation as a fieldwork strategy is to take seriously the challenge it poses to participate more and to play the role of the aloof observer less. Do not think of yourself as someone who needs to wear a white lab coat and carry a clipboard in order to learn how humans go about their everyday lives. If you find you are comfortable only when distant and aloof, why insist on describing yourself as a participant observer? Perhaps a more formal approach will get you the data you want with less personal discomfort. If so, you can turn your focus to activities that get you data. Semistructured interviewing offers a good compromise. If that doesn't do it, turn to more structured forms of interviewing (to be discussed next) that lead to questionnaires and surveys. Consider the possibility that you may not have a natural affinity for fieldwork, especially if you begin to feel that it is getting in your way rather than helping you make your way. Genuine fieldwork entails more than data-gathering.

While I was preparing the original manuscript for this book I had the good fortune to correspond with Peter and Ellen Demerath, who were conducting fieldwork in Papua New Guinea. At the time they were more dramatically situated than any other beginning fieldworkers I knew, and I was anxious to solicit their thoughts on the essence of fieldwork while they were actually immersed in it. Peter's response gives a sense of the fieldworker's participation as performance, as making oneself believable.

> When I think of the "art" in fieldwork, and ways in which the artist rather than the scientist is called for, I think primarily of how much of what we are trying to do here is to present, or compose, both personas and projects that are appealing and attractive (or at least comprehensible) enough, so that people will talk with us and ultimately participate in our research. In this sense, perhaps much of the art of fieldwork lies in effective public relations.
>
> We find that we do many things—housework, pumping water, chewing betelnut, playing soccer and volleyball, chatting, greeting, poling a canoe, eating sea turtle stew after having just seen the animal slowly and painfully butchered—with an eye on how these things are perceived by the people here. We hope they will regard us and our actions as attractive (or non-threatening) to the extent that they will regard us as fellow human beings. It seems to us that the anthropologist must constantly attend to the "composition" of this public persona, and perhaps this is one

of the areas where the art of fieldwork is visible. [Peter Demerath, personal communication, February 1995; see also Demerath 2001]

Peter and Ellen did not go halfway around the world to chat, play volleyball, or pump water, and ordinarily they would have had no opportunity at all to pole canoes or eat sea turtle stew. They were doing what intuition and common sense guided them to do as "fellow human beings," participating in the activities of others in the hope that those others would participate in their research. Their strategy addresses the concerns reviewed at the beginning of the chapter: gaining entrée and maintaining rapport, reciprocity, a tolerance for ambiguity, and personal determination, coupled with faith in themselves. There are no guarantees. But any experienced fieldworker will recognize that this is what genuine participant observation entails.

Getting Nosy

A ready topic for debate among experienced fieldworkers is whether interviewing or participant observation is the key dimension in the work. Which is more important? Which logically should precede the other when you are initiating a new inquiry? Again, the best answer seems to be, It depends. Interviewing, to be presented here as a complement to participant observation, includes a broad spectrum of activities, but it is easier to define. Participant observation is the residual category that includes anything that is not some kind of interviewing.

I emphasize a distinction between the two in recognition of the profound difference in what fieldworkers do when engaging in participant observation (used in the sense of experiencing) and interviewing. It is the difference between passively accepting whatever comes along—information that is virtually handed to us—and aggressively seeking information by getting nosy.

In the simple act of asking, the fieldworker makes a 180-degree shift from observer to interlocutor, intruding into the scene by imposing onto the agenda what he or she wants to know. That does not mean questioning is a sinister business, but there is a quantum difference between taking what happens to come along and taking charge of the agenda. The difference might be likened to the contrast between being served a hosted

meal or ordering from an à la carte menu. In the first case, one takes what is offered; in the second, one states one's preferences.

There are artful ways to conduct interviews, artful ways to ask questions, artful ways to make informants more comfortable when using a tape recorder, and artful ways to check the accuracy of informant responses. Decisions about how much to record from informal conversations, how much to transcribe from formally recorded ones, or how long to conduct interviews in the course of an inquiry all require judgment calls. One needs to develop a sixth sense about which data may ultimately prove most useful, with the long-range objective of accumulating less data rather than more. I will highlight a few points deserving of special mention, but I offer no magic formula for helping a poor interviewer become a better one. We all can improve our interview style by attending as carefully to our own words recorded in transcribed interviews as we attend to the words of our interviewees.

Longtime fieldwork allows a researcher to develop a keen sense of what, when, and under what circumstances it is appropriate to ask a question and when it is better to remain quiet. That requires distinguishing between what you wish to know and how to go about making your interests known. Sometimes it means holding questions for later; sometimes it means holding questions forever; as often, it means recognizing the moment to raise a question because circumstances open a window of opportunity on a normally taboo, sensitive, or seemingly irrelevant issue.

I recognize a cultural norm that guides my own behavior in this regard, one that makes all fieldwork a dilemma for me and rears its head on every occasion when I want to interrupt with a question, even in ordinary conversation: Do not intrude. In *Halfway Home*, novelist Paul Monette describes the reluctance to intrude as "the first WASP commandment." This is why the most thorough and inquisitive of researchers might be aghast at the suggestion that they ought to seek the same level of intimate information about their own colleagues or students at home that they feel professionally obliged to achieve in the field. Anthropologist Fred Gearing reveals the uneasiness he felt from the first moments of his introduction to fieldwork:

> During the next several days I sought out certain Indians, and we talked. Our conversations were typically low-keyed, filled with long

silences. I never quite felt that I was intruding, but was never fully confident that I was not. [1970:9]

Asking does more than merely intrude, however—at least when it goes beyond exchanging pleasantries of the day. Even exchanging pleasantries can lead to unexpected awkwardness, as when a friendly Thai asks, "Where are you going?" in the custom of a people for whom this, rather than our innocuous "How are you?" is the proper greeting in passing. Our questions as fieldworkers become increasingly intrusive as we seek to understand what is going on. Too easily we may put informants on the defensive by insisting or implying that they should be able to explain not only what is going on but why. In framing our questions we also tip our hand in ways that subtly influence the future course of our work. Although we routinely insist that we are interested in everything about the lives of our informants, our questions belie our claim by revealing that certain "everythings" are of far greater consequence than others.

Years ago, while writing a methodological preface to their pioneering study of male sexual behavior, which turned out to be a spectacular chapter on interviewing in general, Alfred Kinsey and his colleagues pointed out that although their questions were on sensitive topics, the very act of questioning can make any topic sensitive (Kinsey, Pomeroy, and Martin 1948:chapter 2). Through interviewing, we risk turning any topic on which we have expressed interest into a sensitive one, inadvertently alerting informants to issues of special concern to us. As well, local issues of purely academic concern may be fraught with political or economic overtones for respondents. We cannot naively assume, for example, that informants are delighted to be asked about the value of their personal possessions, the size of their livestock herds, or the amount they pay in taxes (Christensen 1993).

Let me offer an illustration of the difficulties in obtaining sensitive information. I was invited to comment on a redrafted proposal for researching condom use in AIDS prevention among minority populations. Indicative of the influence qualitative approaches now exert—even among agencies that insist on final reports with totally quantifiable results—researchers applying for a grant had been directed to augment their essentially quantitative approach by including semistructured interviews among their data-gathering strategies. I pointed out that the way the interview schedule had been

designed required researchers to introduce the topic of condoms early in the interviews. As a consequence, interviewers were likely to lead respondents to give answers along socially acceptable lines that did not necessarily square with actual behavior.

An underlying question is one of the most difficult in nondirective interview strategies: how to learn what you want to know without framing questions in a way that you, rather than your informants, introduce and pursue certain topics? How can the context remain theirs rather than your own? In this case, with one-time interviews, some possibilities presented themselves. Interviewers might, for example, have asked respondents to identify (free list) all the safe-sex practices they could think of, returning to those of special interest to project personnel later in the interview, perhaps prompting with other practices not mentioned. Or they might have provided a comprehensive list of their own, burying items of special concern to the researchers, as, for example, a list that included but did not specifically highlight condom use. In addition, specific questions on the topic might have been introduced near the end of the interview, so that interviewers (and coders) would be able to track when, where, and how the topic was formally introduced.

It has taken years for me to become so bold that I risk the disapproval of dental hygienists by looking them directly in the eye and stating flatly that I do not now and never intend to floss! Why would a minority respondent, answering intimate personal questions about sexual practices, want to disappoint a researcher by claiming to be socially irresponsible about the risk of transmitting a disease as devastating as AIDS? Further, if you tell interviewers what you think they want to hear, maybe they will go away sooner. Interviewing is not all that difficult, but getting people to tell you how they really think about things you are interested in learning or how they think about the things that are important to them is a delicate art. My working resolution to the dilemma of assessing what informants say is to recognize that informants are always telling me *something*. My task is to figure out what that something might be.

What interviewing can do, of course, is introduce efficiency into fieldwork. That efficiency can reach a point in which fieldwork itself—the participating kind that is the focus of this discussion—may be eliminated altogether. If the questions to be asked can be tightened up enough, perhaps the principal investigator need not enter the field at all. Research assistants, even contract pollsters, can get the needed information.

One cannot do participant observation without being there, although, as pointed out in the previous chapter, fieldwork consists of more than just being in the field. One can conduct fieldwork through extensive interviews that do not assume or require residency on the part of the fieldworker.

Most qualitative researchers consider participant observation and interviewing to be complementary, but that does not require drawing on them equally or necessarily drawing on both of them in every study. Fieldworkers invest more heavily in whichever of the two better accommodates their research style and their research question. Some fieldworkers do little or no formal interviewing, maintaining instead a casual, conversational approach in the manner of Gearing's "low-keyed conversations." Michael Agar takes the opposite view on behalf of his ethnographic concern with meanings: "Ethnographic question asking is a special blend of art and science. . . . Ethnography without questions would be impossible" (1996:95). If his statement is too strong to apply to all fieldwork, we must at least recognize that fieldworkers who ask or are allowed no questions are tempted to become their own informants.

I take interviewing to include any situation in which a fieldworker is in a position to, and does, attempt to obtain information on a specific topic through even so casual a comment or inducement as, "What you were telling me the other day was really interesting . . ." or "I didn't have a chance to ask you about this before, but can you tell me a bit more about . . ." To categorize the major types of asking in which fieldworkers engage, I offer the following list. Descriptive titles make the categories seem obvious, yet each is worthy of the scholarly attention it has received in an extensive literature devoted to specific aspects of interviewing:

- Casual or conversational interviewing

- Life history/life cycle interviewing

- Semistructured (i.e., open-ended) interviewing

- Structured interviewing, including formal eliciting techniques
  ○ Survey
  ○ Household census, ethnogenealogy
  ○ Questionnaire (written or oral)

- Projective techniques
- Standardized tests and other measurement techniques

The list could easily be expanded or collapsed, depending on one's purposes. My bias toward ethnographic research shows through with the inclusion of two categories. One is the category for household census and ethnogenealogy, once a mainstay in initiating community studies and still a good starting place when conducting them. Another is the category for projective techniques. That category accommodates the once-fashionable fieldwork practice of collecting Rorschach or Thematic Apperception Test protocols (see, for example, Henry and Spiro 1953), as well as more recent interests in projective interviewing such as the Spindlers' Instrumental Activities Inventory (1965) or Robert Textor's work in ethnographic futures. There has been a longtime practice of asking informants straightforward, but nonetheless projectively intended, questions about the foreseeable future: Ten years from now, what do you think things will be like? Note also the intentional ambiguity of the word "things," leaving the respondent to define what he or she has in mind.

Work in educational settings leads me to include as a separate category the kind of tests associated with schooling, thus the category "standardized tests and other measurement techniques." For the fieldworker, however, such measurement techniques should be regarded as a special type of interview. What makes standardized tests different from other forms of interviewing is that the interviewee supplies an answer already known to the person administering the test. As a general rule, fieldworkers ask questions to find out what informants know and know about, not to test knowledge. The questions we ask, the manner in which we ask them, and what we do with the information are intended to signal our interest in and regard for what people know, not what they do not know.

In spite of experiencing too many years under the tyranny of testing in their own lives, practitioners of the art of fieldwork never, never put down those among whom they study. Fieldworkers attuned to the art of teaching as well as to the art of fieldwork are able to follow that practice in the classroom as well. It is critical to keep in mind that testing is a special kind of interviewing, designed for assessment in terms of normative standards. Although fieldwork cannot help but have evaluative overtones, formal testing arises out of a quite different tradition. One

can only hope that fieldworkers make nontraditional use of whatever test data they collect.

One way we show appreciation for what informants tell us is the respect accorded to the information they provide. I felt I had conveyed that idea to two African field assistants assigned to help me conduct a questionnaire survey in my study of the beer gardens of Bulawayo (HFW 1974). As soon as we started interviewing, however, I heard each of them roar with laughter at responses to the questions they posed, in marked contrast to the studied reactions they had displayed during an earlier practice session. Out in the real world—we were conducting our interviews in municipally operated beer gardens—their better judgment had taken over. It was risky to ask anything of total strangers, they explained, and if you wanted to keep respondents talking, you had better make sure they understood how appreciative you were of their responses. They weren't laughing at their respondents, they wanted me to understand, they were laughing with them. And how were my somber interviews going, they inquired tactfully?

The convenience of gathering any type of systematic interview data is always undertaken at the risk of losing rapport, although we can never anticipate exactly what anyone's reaction will be. For every individual too busy to talk, someone else may be reluctant to bring the interview to a close. For someone annoyed with questions too personal, another may insist on volunteering far more, and far more personal, information than that requested. Adherents of particular approaches have their stories to offer as testimonials. Chances are, approaches and questions that make the researcher uncomfortable will have a similar effect on respondents.

I know that fieldworkers have sometimes gone out of their way not to appear too inquisitive, too pushy, too calculating in their approach. They are careful not to appear like teachers giving examinations, journalists tracking down a story, or government agents ready to impose more taxes or exert more control. Most people are uncomfortable with the idea that a file is being kept on them, a universal and growing discomfort as we realize how commonplace this has become in an age of information processing. The experienced fieldworker is not likely to make his or her first appearance at the door with a questionnaire to be answered. The researcher who does show up with a questionnaire is not likely to stick around to learn any more than what is asked on the questionnaire form.

Do I seem to be advocating a fieldwork approach in which slow is beautiful and fast is bad? Frankly, when thinking about what fieldwork can and cannot accomplish, that is my position. Issues surrounding the topic of interviewing help me to clarify it. There are things one can learn quickly by asking direct questions revealing of what one wants to know. There are things one can ask directly without much assurance about the answer. There are things about which we do not ask, guided by our own standards, or about which interviewees do not offer answers, guided by theirs. And there are underlying questions, often the kinds of questions that undergird social research, that can neither be asked nor answered directly: What is your world view? Why do we have schools at all? When everyone seems so dissatisfied, why do you continue to support your form of government?

In a hurry-up world, with technologies that devour information byte by byte, there is increasing pressure to get the facts and get on with it. Fieldworkers are in an excellent position not only to get facts but to put them in context. Nevertheless, fieldwork is a grossly inefficient way simply to gather factual data. When time is of the essence—as it is so often perceived to be—then fieldwork as discussed here is out of the question, even when field-based research for collecting necessary data is essential. Thus, to repeat Bernard's maxim, "All participant observation is fieldwork, but not all fieldwork is participant observation" (1994b:136).

It is only the integrity of the label "fieldwork" that I seek to protect, however. No mandate says that if you can't devote at least a year, you shouldn't bother to go into the field at all. I agree with Bernard when he insists on participant observation in the conduct of all scientific research about cultural groups. He argues that "it is possible to do useful participant observation in just a few days" (1994b:140). A few days do not constitute a participant observation study, but they are days well spent, nonetheless.

Contemporary fieldworkers have responded to the need for speed by incorporating survey-type techniques into their standard repertory, although there is nothing new about having to compress a heavy dose of fieldwork into a short period of time. As with any human activity, there are times when everything seems to be happening at once or when a brief foray is all that time or resources will allow. Robert Redfield was so pleased with a three-day field survey he conducted in 1941 with his then student and field assistant Sol Tax that he titled it "Report of a 3-Day

Survey" and coined the term *rapid guided survey*. Nevertheless, the researchers had a clear idea of the information they sought, for their fieldwork was then in its seventh year (see Rubinstein 1991:297, 304). They also attributed their success at least in part to sheer luck.

Rapid appraisal, or rapid rural appraisal (RRA), became more commonplace in development projects in the Third World during the 1970s and 1980s when "appropriate technology" was the buzzword. RRA itself has been recognized as a form of appropriate technology. Today there are numerous variations on RRA in both name and application, including rapid anthropological assessment, rapid ethnographic assessment, and ethnographic reconnaissance. Practicing anthropologists have their own handbook, *Soundings* (van Willigen and Finan 1991; see also Beebe 1995, 2001; Handwerker 2001), that outlines and illustrates a number of "rapid and reliable" research methods. These procedures can retain something of a fieldwork flavor in what is described (or rationalized?) as an iterative and exploratory team approach. In this approach, the research begins with (but moves rapidly beyond) preliminary observations and semistructured interviews with key informants. These preliminary data are used to guide the construction of appropriate survey or questionnaire instruments with the entire process to be completed in a limited time.

To an old-time and old-fashioned ethnographer like me, words like "ethnography" or "fieldwork" join uneasily with a qualifier like "rapid." Then again, I've never been in a hurry to do things. My motto, "Do less, more thoroughly," may be nothing more than rationalization for my preferred and accustomed pace. Perhaps I envision a fieldwork entirely of my own making, having mistakenly accepted pronouncements about its duration (such as one year at the least, preferably two) as minimum standards when today's fieldworkers regard them as impractical and unnecessary. Bernard now proclaims three months the minimum time "to achieve reasonable intellectualized competence in another culture and be accepted as a participant observer" (Bernard 1994b:151). I agree that any amount of time a researcher can devote to participant observation should prove useful for gaining a sense of context.

But I am concerned whenever participant observation is simultaneously portrayed and faulted as a quickie exercise. Similar efforts have been directed at determining how few informants one really needs in gathering technically reliable information about a cultural domain (e.g., Bernard

1994b:chapter 8; Romney, Weller, and Batchelder 1986). It is hardly surprising that these researchers are strong advocates for the efficiency of formal procedures and structured interview schedules. I hope Bernard has not inadvertently foreshortened the acceptable period for fieldwork for those who will carefully misread his statement to reassure themselves that the three months he says is adequate to establish oneself in the field is all the time one needs to devote to a study.

Although I am not an advocate for finding faster ways to do fieldwork, neither am I committed to making fieldwork more time-consuming simply for its own sake. Time in the field is no guarantee of the quality of the ensuing reports. Nor need efforts to speed things up and find ways to get better data in less time be seen as detracting from efforts to make interviewing a better art as well. With that in mind, I offer some suggestions about interviewing, accompanied by a reminder that this topic has been well served in the vast methods literature, including early statements still brimming with cautions and insights (e.g., Paul 1953; Spradley 1979) and more recent how-to chapters and monographs (e.g., Bernard 2000; Douglas 1985; Gubrium and Holstein 2002; Rubin and Rubin 1995; Seidman 1991). My comments relate especially to semistructured interviewing of the sort that virtually all field researchers employ, whether constructing a rapid survey or embarking on a long-term inquiry.

Doing Better Interviewing: Using Interviews Better

- *Recognize listening as an active and creative role.* I once heard the late educational historian Lawrence Cremin revered for his capacity as a "creative listener," a phrase that lingered in my mind as both an unusual compliment and a wonderful insight into the art of interviewing. Creative listener! Certainly that includes being an attentive listener. It implies even more a listener who is able to play an interactive role, thereby making a more effective speaker out of the person doing most of the talking. An interview ought to be a satisfactory experience for listener and speaker alike.

    I regard myself as a listener, but that is not the same as being a creative listener. I confess that I frequently tire of listening, although surely Cremin must sometimes have experienced

that same feeling, especially after assuming the role of college president. There are a few individuals for whom I seem to play the role of creative listener, and there are a few individuals who play that role for me. On either side of such conversations, I find the interaction not only satisfying but intensely stimulating. Creative listening seems a wonderful talent for any fieldworker to strive continuously to develop, especially one who intends to use semistructured interviewing as a major field technique.

- *Talk less, listen more.* If the idea of creative listening seems too elusive, try simply talking less and listening more during any interview. As an easy first step, practice waiting one thousandth of a second longer before intruding on a momentary pause to introduce a comment or new question. Interviewers are reminded to distinguish between a pregnant silence and a dead one. A lengthened pause on the researcher's part may be enough to prompt the interviewee to pick up the conversation again. Our own conversational patterns display a certain inertia. A conversation in motion tends to remain in motion; silence poses a threat. We become our own worst enemy during the interview process by rushing in to fill the pauses. If the researcher does not immediately plug the gap, the interviewee is likely to do it instead, without even realizing why.

- *Make questions short and to the point.* If it is necessary to repeat, do exactly that. Do not expand or elaborate, for in doing so you are likely either to start an answer or to change the question. This is usually done inadvertently, in the spirit of helping both the respondent and the dialogue. If you study interview protocols—and I urge you to examine your own—you are likely to discover that a simple question usually becomes two or three competing and increasingly complex ones through the course of any solicitous prompting that follows.

- *Plan interviews around a few big issues.* Successful interviewers return again and again to develop dimensions of an issue, rather than detailing myriad little questions to ask. For initial interviewing, James Spradley recommended what he termed *Grand*

Tour questions (1979) of the sort, So, tell me something about yourself, or How did you happen to get here? The interviewer might then have several major topics in mind to which attention can be turned repeatedly in minor variation. For example, family and kin might be the central topic in the interviewer's mind, to be translated into more detailed questions about each family member, sometimes with an ambiguous prompt like, Can you tell me anything more about that?

- *As soon as possible after an interview, write it up.* Transcribe the interview, if it was taped, or index its contents (topics discussed and their location on the tape) if you do not intend to make full typescripts of each interview. If it was not taped, flesh out your brief notes while your informant's words remain fresh in your mind. Then study the transcript or listen to the tape to see how you are doing as an interviewer and to immerse yourself in what you are learning from and about your informant (for an excellent example, see VanderStaay 2003). If time allows (as it should), do not proceed with the next interview until the present one has been processed. Always be thinking about how you intend to use the information, both for the immediate purpose of guiding future interviews and for your eventual incorporation of the material into your final account.

- *Anticipate and discuss the level of formality you plan for the interview.* If you intend semistructured interviews to be more formal than earlier conversations, explain any shifting ground rules so your informant understands what may otherwise appear as a personality change that has suddenly come over you. Formal taped sessions can provide opportunity for a different kind of exchange, one in which the person being interviewed is clearly in the know, and the researcher is the person who wants to find out. Michael Agar calls this the "one-down" position, where the fieldworker assumes a subordinate role as learner, as opposed to the "one-up" role assumed by the scientifically oriented hypotheses tester (1996:119).

  Recognize nonetheless that the person with the tape recorder ought to remain in charge. You need to decide whether you can

live with that. Perhaps you will have to give way to egalitarian urges to make the exchange more evenly reciprocal. If so, be advised that when you listen to the tape you may discover that you were the one being interviewed.

I have always felt that a formal interview is and ought to be a special, asymmetrical form of conversation, one party seeking information, the other providing it. Work toward achieving that format if it suits your style and purposes. Explain that in your formal interviews you want to record your interviewee's words and explanations even if your informant wants it understood that some comments may be made off the record. Stop the tape recorder any time your interviewee prefers to speak off the record, desires a break, or wishes to discuss the interview process with you. You might also suggest that if your questions prompt similar questions that your informant might like to ask of you, they can be noted for discussion later.

Conversational approaches in tape-recorded interviewing are less efficient. They may not be necessary if your informant understands how you distinguish between ordinary conversation and a formal interview in which you take special care to record an interviewee's exact words. You may have to overcome an urge to be more casual, but both you and your informant need to remember that your association, while friendly, is essentially professional. Someday you will go away, and the interview will go with you.

Make informants aware of the importance of the interviews to your work by your actions as well as your expressions of appreciation. Better to err on the side of being too formal than to create the impression of being too casual. Try to use a tape recorder, if possible. Augment recordings with brief notes, if possible. Conduct the interview in private, if possible. Formalize the occasion by arranging an appointment yourself (rather than through an assistant or secretary), if possible, perhaps even suggesting in advance the major topics you would like to discuss. And leave the tape recorder running after the formal interview ends, if possible, in anticipation that although the interview is finished, your informant may not be.

If such formality seems the very antithesis of the kind of interpersonal exchange you want to foster, then follow your intuition to find a style more suitable. There is no rule against being more interactive, no rule that somewhere in your report you must include the words of your informants. Perhaps you did not want to become the kind of fieldworker who "captures" someone else's words. As integral as formal interviewing is to fieldwork in general, you must always consider the possibility that it is not for you.

- *If you are not under the gun to work through your interview data as rapidly as possible, see how long you can hold off before you develop a questionnaire or a tightly structured interview schedule.* The question of when and how interview schedules are developed reveals a major difference between fieldworkers and survey researchers. The survey researcher typically enters the field with a prepared schedule. Fieldworkers are more likely to administer such an instrument near the conclusion of the field research, when they know the questions that have yet to be asked and have a clearer idea of how best to ask them. The exception might be a household census or similar inventory through which the researcher also introduces the research project, gathers relevant basic demographic data, and looks for knowledgeable informants willing to be interviewed in depth. Even under those circumstances, try to keep the interview open. Ask as few questions as necessary and include an open-ended question or two to invite respondents to express what is on their minds or to provide context for the research topic.

A maxim directed at quantitative researchers (although too seldom heeded) holds in our work as well: Behind every question asked, there ought to be a hypothesis. We don't have to be that sticky about formalizing hypotheses, but data should never be gathered simply for the sake of gathering them or because it is so easy to add another question or two. If it doesn't really matter whether respondents own their own homes, graduated from high school, or have ever been arrested, don't ask. If it does matter, give them the opportunity to explain and include

their explanations in the information you record. That's the difference between hit-and-run surveys and the fieldworker who intends to stick around to try to figure out how things fit together.

- *Invite informants to help you become a better researcher.* Agar's notion of the interviewer in the one-down position can be extended to the research process itself. Keep in mind that your interviewees have views about your interview techniques as well as about the scope of your questions. Don't fish for compliments, but direct questions like Do you have any suggestions about these interviews? may prove immediately helpful and lend insight into how the interviewee is feeling as a participant in the research process. A further question can get directly at content: Are there topics we might explore that I haven't asked about? Should you get no response at first, you nonetheless are emphasizing the extent of your interest and effort at thoroughness and your respect for the intelligence of your informant.

- *Search for patterns in responses.* Search not only for what is there but for cut-off points (see Henry 1955:196) in discussion or topics consistently skirted or avoided—on your part as well as on the part of your informants. Don't forget to go back through all of your interviews if you work with an informant over a period of time. I discovered that informants often gave valuable information and clues as to what they felt was important in early interviews, but when everything was new and coming at me so fast, I failed to pick up on such clues the first time around.

In studying interview protocols, I have also found it useful to distinguish between what informants are telling me and what else, if anything, they may be trying to tell me. In one sense, everything an informant tells you can be taken as a fact—a linguistic fact, if no other kind. But informants make choices, sometimes leading us, sometimes leading us astray. Occasionally I have found myself anticipating what they would say next, as a way to assess whether my informant and I were on the same wavelength. I believe it important to be able to quote back to informants, in

their own words, topics mentioned or alluded to in earlier conversations. There may also be times when an ambiguous reference to an earlier topic is a more appropriate way to reintroduce it. That approach keeps you from leading the discussion or from phrasing questions in such a way that the only response needed is a yes or no.

- *Do not become so committed to the qualitative dimensions of responses that you fail to count and measure those aspects that need to be counted and measured.* Keep your research purposes clearly in mind in deciding what and how much to analyze. Carefully recorded language, for example, lends itself to rigorous analysis, but the rigor can throw up a smoke screen of carefully conducted, but totally inappropriate, analyses, lending an aura of science but indicative of a poor artistic choice. Behind every decision intended to advance science lies an opportunity for exercising sound human judgment.

Looking over Others' Shoulders

Data-gathering is not limited to information that fieldworkers gather through participant observation and interviewing while actively on site. There are additional, often critical, sources of information, especially, but not limited to, personal documents and other written records. A discussion of this third category, archival research, concludes this review of the basic arts of the fieldwork part of fieldwork.

I used to think there was a degree of art involved in searching out information in a library; today, I am willing to concede that task to science. I watch in dismay as students run enormous computer searches on unfamiliar topics, perhaps hoping that if they can press the right combination of keys at their terminal, information will spew forth like coins from a slot machine. Given the exponential increase in recorded information, we can be thankful that the technologies that helped create problems are also available to help resolve them.

There is still some art to using archives, however. The most obvious art clearly parallels the problem one faces in the field: deciding how wide a swath to cut, how deep to burrow; in short, deciding what counts. "No depth of commitment and sense of responsibility will ever be enough to

permit any individual to do what is there to be done," Margaret Mead cautioned fieldworkers years ago (1970:258). Today it is quite thinkable that a fieldworker determined to get a thorough grounding in library research might, in Mead's words, be so "attracted by the inexhaustibility of the task" (p. 258) as never to leave the library at all. As with everything else about fieldwork, one needs to recognize how to focus and when to stop.

Libraries and the general proliferation of information are everybody's problem, but those attracted to fieldwork probably are not going to get stuck in the library. We still hear arguments about whether we should go into the field well informed, having consulted what others have said, or do a library search only after forming our own impressions. I believe the better argument can be made for being well informed, as long as being informed is accompanied by the same healthy skepticism befitting all scholarly research. That is the first of the three suggestions discussed below for making the most artful use of secondary sources.

Making the Best Use of Others' Work

- *Be as skeptical of anything you read as you are of anything you are told.* A lesson we learn too well as schoolchildren, that printed texts are sacred texts, must be cast aside in scholarly pursuit. Most certainly, earlier fieldworkers' reports may no longer be correct, even if they were accurate at one time. Skepticism is absolutely essential in all aspects of fieldwork, including any use of printed sources.

    However, a skeptical stance does not give license to demean all prior efforts. Academics sometimes get carried away in their truth-seeking zeal. It is tempting, especially for younger scholars, to find fault with earlier reports and bring down the elders. I think it far more constructive, and more consistent with a spirit of inquiry, to take the position that earlier researchers did not get it quite right, just as future researchers will probably show that we did not quite get it right, either. If it is any comfort, know that fieldwork's greats continue to take a licking. Hear this passage and reminder from Clifford Geertz:

    > Firth, not Malinowski, is probably our best Malinowskian. Fortes so far eclipses Radcliffe-Brown as to make

us wonder how he could have taken him for his master. Kroeber did what Boas but promised. [1988:20]

A healthy skepticism must always be maintained, even when everything seems to be checking out perfectly, past with present, established landmark studies with our own embryonic inquiries. While Ron Rohner and I were doing fieldwork among the Kwakiutl, Ron discovered an excellent informant in Chief Bill Scow and was sometimes surprised at how consistently Bill's accounts validated the early work of Franz Boas. But one day Ron's question stumped Bill, and Bill explained, "I can't answer that one, Ron. I'll have to look it up." Only then did Ron realize that the old informant and the young anthropologist were using the same references. An earlier descriptive ethnography had now become a prescriptive one!

- *Look far afield for all you might include as the work of others.* Sometimes anthropologists join the "stack rats" to do their work entirely through library scholarship, but fieldworkers are more likely to be sensitive to any suggestion that they never, or hardly ever, go to the library. Whether they spend much time in the library or not, most fieldworkers make use of a vast array of materials in addition to the customary library resources. (See a useful guide for conducting original archival research "with quality and dispatch" in Hill 1993.)

  Personal documents are especially high on the list of non-library sources: correspondence, diaries, travelers' journals, any sort of written accounts that might never find their way into a formal collection but can be invaluable to understanding everyday life or special events. Government records, newspaper accounts, surveyor reports—there is no end to the possible resources to be considered. Similarly, fieldworkers examine and frequently collect artifacts of all sorts in addition to textual documents.

  Fieldworkers need to think creatively about available sources of information that are not ordinarily regarded as data, to avoid falling victim to habits that find us invariably gathering the same limited information in the same limited ways. In my study of a school principal, for example, I was interested in getting some sense of how the principal's professional relationships with other teachers and administrators overlapped with his personal relationships with family and friends (HFW 1973). An opportunity to get some hard data on the topic materialized when his oldest daughter announced her forthcoming wedding. I asked the principal if he would review the wedding list and say something about everyone invited, paying particular attention to invitations extended by the parents rather than the bride herself. I might have obtained similar information by going over the list of people to whom the principal and his wife regularly sent Christmas cards. Personal documents such as these are not likely to end up in the Smithsonian, yet they are a ready source of data about social networks. Wouldn't a list of the telephone numbers frequently dialed or a directory of e-mail correspondents provide similar insight into professional or personal networks?

- *Think about new ways to use data easily at hand.* The previous point emphasized looking at sources of data easily overlooked, so that we do not take too constricted a view of what constitutes data. The complement to that is to be equally creative about using readily available data in unusual ways.

  It may, for example, be easier to document, even to discern, patterns or trends by looking at the frequency or space devoted to certain kinds of events in the local newspaper over a period of years than by relying solely on the impressions of older informants. Margaret Mead was able to give a historical perspective to her interest in child training by comparing the topics discussed in government manuals over several decades. The changing tables of contents in introductory texts in fields like anthropology, psychology, or sociology provide an excellent basis for watching the evolution of those disciplines. Old catalogues or photographs offer evidence of changing fashions in clothing, hairstyles, and the like. That such sources of data exist is hardly a revelation, but it doesn't hurt to remind fieldworkers

that participant observation and interviewing are not the only ways to get information. Such extraneous sources also invite researchers to compare what they are being told with sources less susceptible to reinterpretation with a knowing backward look.

This chapter has reviewed some basic issues in fieldwork with the assumption that data-gathering is always guided by sensitivity on the part of the fieldworker. Potential problems are recast as challenges to recognize and reckon with. I turn next to examining some related problems from what might be called the dark side of fieldwork. Given the focus of the book, I refer to them as the "darker arts."

FIELDWORK: THE DARKER ARTS
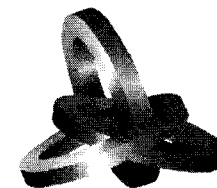
It is necessary to be relentless in ferreting out the dark side of fieldwork, for only then can the other side, the rebirth of the anthropologist, be fully comprehended and understood in a rigorous manner.

—John L. Wengle
Ethnographers in the Field, p. 169

Cultural analysis is intrinsically incomplete. And, worse than that, the more deeply it goes the less complete it is. It is a strange science whose most telling assertions are its most tremulously based, in which to get somewhere with the matter at hand is to intensify the suspicion, both your own and that of others, that you are not quite getting it right.

—Clifford Geertz
"Thick Description," in *The Interpretation of Cultures*, p. 29

It took me a long time to discover that the key to acting is honesty. Once you know how to fake that, you've got it made.

—John Leonard
quoted in J. Douglas, *Investigative Social Research*, p. 55

One of the definitions of art reviewed in chapter 1 referred to trickery, cunning, or artificiality in behavior. Among the definitions of artist, we recognize the trickster, clever at deceit, or the

CHAPTER SEVEN
THE ART OF (CONCEPTUAL) SELF-DEFENSE
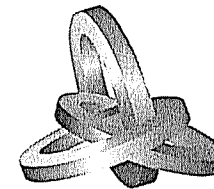
C ertain problems keep recurring not only in the dialogue between quantitative and qualitative researchers but among qualitative researchers themselves. Fieldworkers need coping strategies for dealing with them. But they need not seek definitive answers that resolve such issues for all times, for these are the debates that surround the inquiry process itself. They are critical issues in the research dialogue, yet they are potentially subverting of that process whenever turned full force against any particular research effort, qualitative or quantitative.

My advice to beginning researchers is to be informed as to the substance of these debates rather than to be drawn prematurely into them. Leave them for others, or for yourself on a day when you are prepared to deal with issues on a grand scale rather than confronting a modest research task immediately at hand. Think of these issues as on a par with environmental protection, social justice, a world without war, or the ultimate answer to a question like, What is art? There are myriad issues, ethical, methodological, and philosophical, about which you may be asked or challenged to take a stand. When you are, you will be expected to have a thoughtful position, not to come up with the answer. Any of the dilemmas identified in the previous chapter can be posed either as a broad challenge to qualitative research in general or as a focused one addressing how you propose to approach your particular topic. The issues identified in this chapter are methodological in nature, the sort likely to occur in dialogue (or, sometimes, interrogation) between qualitative and quantitative researchers.

Do not get lured into believing that the entire rationale for qualitative approaches now rests on your shoulders alone or that until you have satisfactorily resolved each of these methodological perplexities, you may not proceed with your own research.

The issues to be raised here concern the scientific method, objectivity and bias, neutrality, reliability, validity, and generalization. The closely related issue of theory introduces the chapter that follows.

Scientific Method

There is not such a thing as a Scientific Method.

—P. B. Medawar
The Art of the Soluble, p. 148

In chapter 4, I inventoried some important techniques currently employed in the more scientific approaches to fieldwork. I did not inquire into the broader issue of "a" or "the" scientific method itself. What about the criticism one hears that fieldwork is the antithesis of scientific method, that quantitative approaches have all the method and fieldwork has none?

A method is a procedure, a technique, a way of doing something.

Fieldwork is a way of doing something.

As a way of doing something, fieldwork includes several rather standard techniques, all of which can be adapted for any particular setting as needed. All fieldwork techniques can be subsumed under the single heading "Participant Observation" or under two major headings if "Participant Observation" and "Interviewing" are paired off to become the dynamic duo of field research. The choice, as discussed in chapter 5, depends on whether one considers interviewing as the complement to participant observation or a major facet of it. Here I treat them separately.

The approaches to fieldwork are, in their almost infinite variations, alternatives rather than sequenced steps, choices among strategies rather than the selection of proper techniques. As George Homans observed years ago (1962), research is a matter of strategies, not of morals. Qualitative approaches avoid any semblance of the rigid, step-by-step sequence generally associated with tight research designs. They are intended to allow researchers to follow a suitable course of inquiry rather than to dictate in advance what that course should be. In essence, qualitative research, as Becker states it, is designed in the doing (1993:219). Although that makes fieldwork difficult to explain in the abstract, anyone who has engaged in it recognizes that in practice it can proceed no other way.

We should rejoice that we are not encumbered by the scientific method in pursuing our work, even while we may feel a bit of envy in recognizing how convenient and self-validating such recipes might be when trying to teach (or having to convince) others about how we proceed. It is easy for us to forget that scientists themselves are not particularly encumbered by the scientific method. As observers have pointed out, there is no particular incentive for those assumed to work under its aegis to tarnish their idealized reputation for systematic work, since it does not get in the way of practice. Paradoxically, the very idea that "real" scientists relentlessly follow the scientific method provides the cover that permits them to be more imaginative (and at times just plain bumbling), while the complementary idea that qualitative research is not guided by rigorous methodological doctrine is held up as one of our major shortcomings.

On the wall of my office I once hung a sign with the words of biologist Paul Weiss: "Nobody who followed the scientific method ever discovered anything interesting" (quoted in Keesing and Keesing 1971:10). It is hardly surprising that a biologist's comment about the scientific method is offered as solace for cultural anthropologists torn between wanting their discipline to be, as Eric Wolf stated years ago (1964:88), both the most scientific of the humanities and the most humanist of the sciences. Except perhaps when lecturing or conversing with colleagues outside their discipline, cultural anthropologists have emphasized the results of their studies, be they findings or interpretations, rather than their methods. In recent years, however, method has come to assume a more prominent role in their dialogue, a preoccupation that has tended to divide them into two camps, those concerned with methods and those concerned with those concerned with them.

Such methodological preoccupation has also found many anthropologists going farther afield, deeper into modes of analysis rather than deeper into fieldwork to achieve methodological sophistication. In the days when the phrase "participant observation" was explanation enough, advice as to

how to go about it tended to be offhand: "Hang around." "Talk to folks." "Try to get a sense of what is going on." It was always pragmatic, sometimes too much so, as reflected in what Jean Jackson calls the "take-a-big-stick-for-the-dogs-and-lots-of-marmalade" jokes (1990:24). Sometimes these were humorously profound, as with Radcliffe-Brown's purported advice, "Get a large notebook and start in the middle because you never know which way things will develop" (quoted in Rubinstein 1991:14). Any such advice was intended to bolster confidence in the fieldworker. It was not intended to reassure fieldworkers or their critics that they were going about their work in the right way.

Nevertheless, fieldworkers do become self-conscious whenever method is at issue. Scientific methods in general, and the essentially mythical Scientific Method in particular, continue to hang as specters over our efforts. What is our equivalent? The unscientific or nonscientific method? The humanistic method? The rejection-of-method method or the absence-of-method method?

It may be comforting to keep in mind that even the most scientific of research procedures, regardless of how systematic and objective, can be neither perfectly systematic nor ultimately objective. Descriptive studies of how laboratory science proceeds remind us that on close inspection the investigative process is (of necessity) totally susceptible to human judgment, a product of social construction subject even to plain old down-and-dirty politics (see, for example, studies by Fleck 1979; Latour 1987; Latour and Woolgar 1986; Woolgar 1983). It is the insistent demand of outsiders for the guided tour of the laboratory and a proven formula for discovery ("just tell me the steps you follow") that traps researchers of all persuasions into portraying as a neat, linear, logical sequence what is, in fact, a dialectical process in which all critical judgments are made by humans. "All worldly truth," Jack Douglas states boldly, "rests ultimately on direct individual experience" (1976:6).

So exactly what are we being defensive about? Insight, intuition, imagination, luck—yes, even serendipity—each is critical to any discovery process, ours no more than theirs. The phrase "scientific breakthrough" nicely credits scientists for maintaining control, always knowing where they are going, although I recall Professor Aubrey Haan suggesting years ago that "scientific fall-through" might be the more appropriate phrase in

most cases. The critical art in all observation is achieved not in the act of observing but in recognizing when something of significance has been observed.

Fieldwork proceeds that way, too, not simply through observation but in recognizing when something of significance—of potential significance—has been observed. The difference is that we try to exert as little interference as possible. We typically deny any suggestion of our own power and authority even when made uncomfortably aware of our advantaged status in the settings we study.

Tight research designs strike me as a good strategy for researchers who need to exert control over what they study, both control *of* and control *for*. Qualitative approaches represent a different way to achieve a different kind of understanding, one that appeals to those who find satisfaction in the discovery of what is going on without the hope of achieving the authority of cause-and-effect studies. Every way of knowing has its place. Science cannot proceed without controlled experimentation, but neither science nor controlled experimentation can reveal all we seek to understand about ourselves and our fellow humans.

Objectivity and Bias

"Objectivity" is perhaps best seen as a label to hide problems in the social sciences.

—Michael H. Agar
The Professional Stranger, p. 91

The process of forming links between ideas in the observer's mind and what one has observed is dialectical: Ideas inform observations and observations inform ideas. The prime mover in the process is the researcher. Whatever constitutes the elusive quality called "objectivity," mindlessness is not part of it.

Observation cannot proceed without an idea in the observer's mind of what to look at and for in qualitative research any more than in quantitative. This runs counter to claims made on behalf of objectivity (and, by extension, to warnings about the evil influences of bias) and stated in strong terms, such as this declaration by ethologist Konrad Lorenz, writing on

behalf of an outdated position that claimed more for observation than it could ever hope to achieve:

> It is an inviolable law of inductive natural science that it has to *begin* with pure observation, totally devoid of any preconceived theory and even working hypotheses. [1950:232]

Another ethologist, C. G. Beer, my source for the above quotation (1973:49), cogently presents the counter view to that of Lorenz, whose position he dismisses lightheartedly as the "doctrine of immaculate perception." Beer cites philosopher of science Karl Popper, who argues that "preconceived theories or working hypotheses must always be involved in scientific observation to enable the scientist to decide what is to count as a fact of relevance to his investigation" (p. 49).

Malinowski tried to put preconceived ideas to rest more than eighty years ago, dismissing them as pernicious, in contrast to what he called "foreshadowed problems," which he endorsed as "the main endowment of a scientific thinker" (1922:9). For a long while the distinction caught the attention of other fieldworkers, but it proved too facile. Beer and other observers were more instructive in equating preconceived ideas with the working hypotheses essential to scientific observation. Scientists like the word "hypothesis." I take pleasure in substituting a different label, "bias."

Rather than dismiss bias as something we should guard against, I have come to think of it not only as something we must live with but as something we cannot do without. Bias reflects prior judgments that speed us along toward new objectives without having to reconsider every decision we have already made along the way. Think of it as comparable to selecting among the options in your computer's Preferences menu, allowing you to proceed without having to rethink every previous choice you have already made, choices that constitute your modus operandi.

Bias itself is not the problem, but one's purposes and assumptions need to be made explicit and used judiciously to give meaning and focus to a study. As long as it is fully explicated, bias should never get in the way. It offers an answer to the criticism voiced by insiders who claim that only they can understand their own group. Bias requires us to identify the perspective we bring to our studies as insiders or outsiders and to anticipate how that affects what we report.

Its counterpart, prejudice, is our true foe, judgment formed without examining its roots. If you can distinguish your prejudices from your biases, let the former guide you away from topics on which your opinions are likely to interfere with, possibly even obscure, your discoveries. But covet your biases, display them openly, and ponder how they help you formulate the purposes of your investigation and show how you can advance your inquiries. With biases firmly in place, you won't have to pretend to complete objectivity either. Try instead for what Margaret Mead described as "disciplined subjectivity" rather than a pretense of objectivity (quoted in M. C. Bateson 1977:71). No artist could wish for more!

Neutrality

Whereas traditional researchers cling to the guard rail of neutrality, critical researchers frequently announce their partisanship in the struggle for a better world.

—Joe Kincheloe and Peter McLaren
"Rethinking Critical Theory and Qualitative Research," p. 140

At one time I harbored the misconception that neutrality was another essential element in descriptive research. Neutrality held that in order to be fair, one had to regard all humans with equal esteem—the anthropological proclivity for "deferred judgment" run amok. An experienced fieldworker, John Connolly, raised for me the question of whether one really needed to be neutral in order to be objective. I was having enough trouble trying to sort out what being objective meant, especially in the subjective and sensitive business of humans observing and interpreting the behavior of other humans. But I admit I was relieved to realize that having likes or dislikes—a rather human quality in which I have been known to overindulge—did not perforce exempt me from doing fieldwork.

I recall my dismay that Jules Henry had allowed himself what seemed too free a rein in presenting his "passionate ethnography" of American society, *Culture against Man* (1963). A decade later, Colin Turnbull was roundly criticized not only for his negative portrayal of the Ik (Turnbull 1972; see also Grinker 2000) but for revealing personal disaffection for them, violating the anthropological canon of deferred judgment. Since

those works were published we have seen innumerable instances in which personal preferences have provided anthropologists the impetus for writing their accounts.

A wave of postmodernists even insisted that the only understanding a fieldworker could gain in the course of research was of himself or herself. For a while, it seemed that anthropologists might become so taken with describing their own feelings that fieldwork would be nothing more than a vehicle for self-understanding. Perhaps a time of self-reflection (bordering on self-absorption) was inevitable after so long a period in which fieldworkers were not expected to demonstrate any feelings at all. In that earlier day anything recorded privately in diaries or personal correspondence was usually published as a separate memoir or remained privileged information forever (see Bruner 1993).

I take deep and genuine interest in the people and settings I have written about. I have learned to recognize and to appreciate in those feelings a source of energy for conducting my studies. My feelings have not always been positive, and I have never known any group of people that did not have its share of rogues and rascals, most certainly including some of my associates in academia. Nevertheless, I cannot imagine initiating a study in which I had no personal feelings, felt no interest or concern for the humans whose lives touched mine, or failed to find in those concerns a vital source of inspiration and energy. Neutrality is another of those topics we must be able to address without having to embrace.

Reliability

Reliability preoccupies those who hold anthropology to be a behavioral science, and who thus place severe limits on what the ethnographic method should include. It is a valuable quality in laboratory, medical and product safety research, and in some social research operations.

—Roger Sanjek
"The Ethnographic Present," p. 620

Reliability remains beyond the pale for research based on observation in natural settings. That is unfortunate, for it is difficult to escape the suggestion that if our work is not reliable, then it must be unreliable. Its tech-

nical meaning in the lexicon of researchers, as Jerome Kirk and Marc Miller define it, is closer to "replicability" or "consistency," "the extent to which a measurement procedure yields the same answer however and whenever it is carried out" (1986:19). In order to achieve reliability in that technical sense, a researcher has to manipulate conditions so that replicability can be assessed. Ordinarily, fieldworkers do not try to make things happen at all, but whatever the circumstances, we most certainly cannot make them happen twice. And if something does happen more than once, we never for a minute insist that the repetition be exact. As James Fernandez observes, "We anthropologists have long had the Heraclitean understanding that we cannot step into the same stream twice" (1994:136).

Reliability and its partner, validity (to be discussed next) are frequently cited as critical components of research and are sometimes described as complementary aspects of objectivity (Kirk and Miller 1986:19). It is awkward to have to admit to strict adherents of the quantitative tradition that fieldwork does not lend itself to reliability. But I have never been all that convinced that reliability necessarily serves quantitative researchers well, either.

The problem with reliability is that the rigor associated with it redirects attention to research processes rather than to research results. Similarity of responses is taken to be the same as accuracy of responses. The problem with equating them is that one might obtain consistent temperature findings consistently in error due to a faulty thermometer, obtain consistent responses to survey questions that make no sense to respondents, or obtain consistent ratings among raters trained to look for the same things in the same way, in each instance achieving a high degree of reliability because of unreliable data. The strain for identifying consistency in findings thus yields to establishing consistency through procedures. Reliability is, therefore, an artifact.

We need to recognize the circumstances that render reliability essentially irrelevant as a central concern in fieldwork; we do not need to apologize for it. Kirk and Miller recommend that we handle the problem through carefully documented ethnographic decision making (1986:73). I heartily concur as to the value of documentation, but I am not convinced that it solves the issue of reliability. Nor am I convinced that we need to address reliability at all, except to make sure that our audiences understand why it is not an appropriate measure for evaluating fieldwork.

That is not to say that reliability in this technical sense is out of the question. Certainly some of the systematic data we gather are amenable to statistical treatment. For anyone concerned primarily with reliability, however, I think the more systematic methods for data-gathering have far greater appeal than the kind of fieldwork I am advocating in which we try to be right but do not turn to statistical manipulations to validate our claims.

Validity

What the ethnographic method aims to achieve are accounts that support the claims they make. In terms of validity, there *are* better and worse ethnographic accounts.

—Roger Sanjek
"The Ethnographic Present," p. 621

Although fieldwork should yield highly valid results, I have argued elsewhere against the relevance of validity as a criterion measure in qualitative research (HFW 1990). Yet I find validity to be a more robust concept than reliability, one to confront boldly if we must confront it at all. Whether to confront it brings us again to the issue of whether we are willing to accept the language of quantitative researchers as the language of all research, or whether different approaches, like different art forms, warrant different evaluative criteria. To me, a discussion of validity signals a retreat to that preexisting vocabulary originally designed to lend precision to one arena of dialogue and too casually assumed to be adequate for another.

As originally employed in its technical sense, validity asks whether a researcher has measured what the research purports to measure. That issue is of vital significance, yet in practice validity is nowhere near as rigorous as reliability. Instead of generating coefficients that allow numerical comparisons, validity is more akin to a property like neatness, where one thing may be recognized as neater than another but nothing achieves absolute neatness.

But validity has taken on wider meaning; today it is associated more closely with truth value—the correspondence between research and the real world—rather than limited to measurement. To illustrate: The underlying question of the validity of an IQ test is related not only to performance as revealed by an individual's test score, but to the larger issue of whether the test has tapped into something as complex as intelligence.

Clearly, our "I was there" approach to research positions us well in terms of the potential truth value or warranted assertability of our reports. They should be substantially accurate and substantially complete—in spite of the fact that sometimes they are not. We can, and often do, make the validity claim. Anthropologists Pertti and Gretel Pelto offer an argument on its behalf:

> "Validity" refers to the degree to which scientific observations actually measure or record what they purport to measure. . . . In their field research anthropologists have invested much effort to achieve validity, for we generally assume that a long-term stay in a community facilitates the differentiation of what is valid from what is not, and the assembling of contextual supporting information to buttress claims to validity. [1978:33]

A question that remains is whether we need such a claim at all. Anthropologists like the Peltos have worked on behalf of a more scientific anthropology and thus a more systematic approach to fieldwork. They strive for validity. Fieldworkers as strongly committed to the *art* of fieldwork might instead be content to remind a reader that while their stay in the field was long, it could never be long enough. They would be less insistent about their ability to differentiate between what is valid and what is not, holding instead that whatever information they provide offers illustration but in no way constitutes proof.

Validity can be dismissed, but does not go away; qualitative researchers may find comfort in Russ Bernard's observation that validity is never demonstrated, only made more likely (1994b:42). Fieldworkers need to be able to speak to the issue of what they do on behalf of making the truth value of their accounts more likely or more *credible*, Egon Guba's suggested alternative term for internal validity (1981). Similarly, they must be able to address issues of external validity and generalization, or *transferability*, again Guba's suggested alternative term. Such issues can and should be addressed, but they are better regarded as an invitation to dialogue rather than as a barrier to research. We can demonstrate our willingness to join the dialogue; we

need not be distracted or intimidated by it. Pulling off such a feat while one is still discovering which terms must be addressed requires some artistry at game playing. One needs time to figure out how the game is played before deciding whether and how to participate.

Generalization

Whatever the approach, ethnography is always more than description. Ethnography is also a way of generalizing. This way differs from the standard scientific model, however, and in some ways is closer to the arts. . . . As in good literature, so in good ethnography the message comes not through explicit statement of generalities but as concrete portrayal.

—James L. Peacock
The Anthropological Lens, p. 83

Although issues underlying any of the topics addressed here pose serious problems, debate about them often takes on a sophomoric quality, with neither side really listening to the other or appearing to comprehend the existence of an alternative view. Objectivity, for example, is often argued as being attainable or unattainable; there is no middle ground. Qualitative researchers need to understand what the debate is about and to have a position; they do not have to resolve the issues. As Howard Becker notes about all such epistemological issues underlying research, "If we haven't settled them definitively in two thousand years, more or less, we probably aren't ever going to settle them. These are simply the commonplaces, in the rhetorical sense, of scientific talk in the social sciences, the framework in which debate goes on" (1993:219).

Generalization is another of these epistemological issues, but I find it more worrisome than those already reviewed. It raises a fundamental issue in qualitative work where we invariably look at one of something or at a single case. Even when we are cajoled into increasing our *N*s, perhaps to do three, four, or five little case studies instead of devoting rapt attention to one, we are always disadvantaged by our inability to generalize. That disadvantage raises the critical question, What can we learn from studying only one of anything?

My immediate and perhaps too-glib answer to that question bespeaks my strategy toward all such questions rooted so solidly in a positivist orientation. What can we learn from studying only one of anything? All that we can!

The quick counteroffensive is a good device, and the response "All we can" is enough to cut short a diatribe, although admittedly it is only the beginning of an adequate answer. Most certainly we need to demonstrate how our cases contribute to some larger picture. We are particularly in need of such explanation, given the paucity of our efforts to date to aggregate myriad case studies into some bigger picture. Once again we find ourselves figuratively trying to fill the Grand Canyon with popcorn, one piece at a time.

Were the question posed in slightly different terms to ask, How do you generalize from a qualitative study? you might answer candidly and succinctly, "You don't." That is a safe and accurate answer. It is the basis on which American anthropology was founded under Franz Boas. With an empiricism directed toward rigorous historical particularism, Boas insisted that no generalizations were warranted from the study of any particular society.

As a discipline, anthropology was founded on the horns of a dilemma that committed it to the detailed study of individual societies while professing passionate concern for all humankind. The inevitable resolution was, and is, to maintain two camps, one more inclined toward postpositivist scientific practices that examine frequencies and distributions from which generalizations are deemed to be warranted, the other more attentive to interpretivist meanings and symbols played out in the course of individual cases and lives. Depending on purposes, to some extent the two can be reconciled, but a preoccupation with eclecticism obscures attention that should be directed toward purposes themselves.

I am inclined to treat generalization as something desirable yet always beyond my grasp. I have never studied more than one of anything and always at a particular point in time. Someone who recalled hearing me make that statement asked, "Didn't I hear you say you have never studied more than one person at a time?" With a major effort devoted to the ethnography of one school principal (1973) and another devoted to an account of the Sneaky Kid (1994b, 2002), that impression (generalization?) might have seemed warranted, but it was not correct. What I meant to say, and

here make a matter of record, is that the unit of study in my various efforts at field research has varied: one individual, one village, one institution (urban African beer drinking), the implementation of one educational innovation in one school system. Whatever can be learned from a well-contextualized study of a single case is the contribution that each of those studies has to offer.

If you are interested in averages, frequencies, distributions, and the like, my accounts are not a good source. If you want to know about an instance of something I have studied, my reports should be a rich resource, and that suggests a reasonable criterion by which to judge them. In each of those studies I make a few generalizations, implicate a few more, and leave to readers the challenge of making further ones depending on their own concerns and prior experiences.

Years ago my attention was called to a statement about generalization that I have always kept as a guideline. It was penned by Clyde Kluckhohn and Henry Murray (1948:35) to introduce their coauthored chapter on personality formation and, except for now-outmoded gender language, still represents an elegant way to think about the individual and society, the nexus between the one and the many.

> Every man is in certain respects
> a. like all other men,
> b. like some other men,
> c. like no other man.

In any fieldwork I have conducted, I have substituted my unit of study into Kluckhohn and Murray's aphorism and thereby felt some freedom in offering whatever generalizations seem warranted. I regarded the Kwakiutl village and school of my first fieldwork to be a village and school in certain respects like all other villages and their schools, in certain respects like some other villages and their schools, and in certain respects like no other village and its school. There seemed little point in spending an entire year as a village teacher and a participant observer in village life, then devoting another year to writing up an account, if nothing was to be learned that might be of relevance to other villages and their schools as well.

At the same time, I did not want to claim that the village was typical or representative. There were many ways in which it did not seem typical

even among other Kwakiutl villages, let alone villages in other First Nations communities. Nor was I able to exert any influence over my assignment other than to put myself in the hands of Lyman Jampolsky, director of Indian education in British Columbia at the time, to request an appropriate village placement for me: a single male teacher (thus a one-teacher school) in a region under Anglican rather than Catholic jurisdiction. To an American raised under the strict separation of church and state, it seemed shocking that my religious affiliation was a major criterion affecting my teaching assignment.

Like many qualitative researchers, I pretty much had to take potluck as to where I would be assigned, making the best of whatever opportunities the assignment afforded rather than trying to find a site that I could defend as typical. Instead I needed to specify how the village and school to which I was assigned fit within some broader set of categories. This idea, a sort of artful end run around the sampling problem, followed advice written by Margaret Mead in 1953 to explain how researchers in natural settings can address issues of sampling when sampling itself is neither practical nor possible. Her essay concerned the issue facing Kluckhohn and Murray quoted above: How can we arrive at statements about groups of people when we meet them only individually, and thus, how can we deal with representativeness?

> Anthropological sampling is not a poor and inadequate version of sociological or sociopsychological sampling, a version where n equals too few cases. *It is simply a different kind of sampling,* in which the validity of the sample depends not so much upon the number of cases as upon the proper specification of the informant, so that he or she can be accurately placed, in terms of a very large number of variables. . . . Each informant is studied as a perfect example, an organic representation of his complete cultural experience. [Mead 1953:654–55; italics in original]

Mead turned the sampling issue on its head, suggesting that in fieldwork we ask how the instances we have to report fit into some larger picture. Unable to control the sampling procedure itself, as fieldworkers we redefine the problem to fit the circumstances under which we are likely to obtain not only our informants but our research sites. We do not presume to identify the typical informant or village or setting; instead, we ask how our

informant or village or setting fits into the larger scheme of things. To what extent is the one in some important ways like the many?

This way of approaching generalization asks the researcher to make what seems an essentially artistic choice between emphasizing how the single case informs more generally or how its uniqueness must be cherished. In my Sneaky Kid account, I came down on the side of the former, emphasizing that although his story was unique, it was not an isolated case. I drew on Clifford Geertz for the authoritative footnote: "The important thing about the anthropologist's findings is their complex specificness, their circumstantiality" (Geertz 1973:23). That "complex specificness" remains the heart of the matter, the characteristic of a fieldwork approach. While the effective story should be "specific and circumstantial," its relevance in a broader context should also be apparent. The story must transcend its own modest origins. The case remains particular, its implications broad (HFW 1983a:28).

Some fieldworkers play a more cautious hand, underscoring that they have not tried to find the typical informant and do not want to detract from the uniqueness of the case. I know of no better example than the way anthropologist Sidney Mintz introduces his key informant, Taso, in *Workers in the Cane*. This is a statement to which I referred students who got caught up in this question of typicality:

> He is not an "average" anything—neither an average man, nor an average Puerto Rican, nor an average Puerto Rican lower-class sugar cane worker. He has lived just one life and not all of that. He doesn't think of himself as representative of anything, and he is right. His solutions to life's problems may not be the best ones, either, but he seems satisfied with his choices. I have tried to put down his story in the context of what I could understand about the circumstances under which he lived and lives. [1974(1960):11]

It is interesting to realize how persuasive a powerfully written statement can be. Mintz steered clear of seeming to write about "some other men" in his portrayal; the story was Taso's own. When invited to write a preface for a reissue of the account a number of years later, however, Mintz confounded the question, seeming to want to portray Taso as average but not typical. (I think I might have wanted to write it the other way around: typical but not average.)

In fact, except for his very unusual intelligence, Taso might be described as quite average in nearly every way. This, then, is the autobiography of an average man. But I tried to make clear when I first wrote the book that this emphatically does not mean that Taso is "typical," representative of others, or ordinary; and in these regards, the book—and Taso's own words—must stand on their own account. [Mintz 1974:ix]

In preparing our cases we, too, want to have it both ways. Each case is unique, yet not so unique that we cannot learn from it and apply its lessons more generally. We are provided a way out of our seeming ambivalence if we resist the trap of an either/or position, keeping in mind Kluckhohn and Murray's aphorism, broadly restated, that every case is, in certain aspects, like all other cases, like some other cases, and like no other case.

Self-Defense versus Getting Defensive

I have identified certain topics—scientific method, objectivity and bias, neutrality, reliability, validity, generalization—because they are problematic in field-oriented research. Individually and collectively we need to be thoughtfully aware of them, to have a sense of the underlying problems they point to and a working resolution for them. My call is for fieldworkers to be well coached in the art of self-defense, intrigued with, rather than defensive about, epistemological issues.

Neophyte researchers also need to be attentive to the research climate in which they propose their inquiries. You are not likely to be lauded for your creativity in conceptualization if your audience is hammering away at you about objectivity and reliability. I have not meant to give false hope that such methodological issues can be brushed aside. When they are placed more like barriers than hurdles, challenges to help you achieve better clarity of purpose, you should weigh the wisdom of pursuing a qualitative approach at that place or in that moment. Artists and fieldworkers alike must find a receptive audience for their efforts.

The issue of theory follows hard on the heels of topics discussed here. Issues concerning theory cannot be sidestepped by the individual fieldworker, regardless of how basically descriptive or atheoretical he or she might claim to be. I turn to that topic to begin a new chapter and continue this discussion of fieldwork as mindwork.

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