are two important issues to consider when judging the validity of one's interpretation.

First, is there supporting evidence to back up my claims? As a researcher, it will often seem obvious to us that the bulk of our data is pointing to a certain set of interpretations. Of course, our interpretation of that data is what is leading us to our concluding interpretation. But I believe it is important to have supporting evidence. If I claim that the US government portrayed the Congo as Y, which thus enabled it to act in Z manner, I need to provide evidence of both Y and Z. If I cannot, then my claims should be taken as highly speculative. I would argue that this is the reason one needs to do as much historical research as possible. But am I slipping rationality and empiricism back in? I reiterate my distinction between empiricism as *method* versus *philosophy of knowledge*. The value I place on the former does not make my claims 'true,' but it does strengthen my ability to argue for their validity.

This leads to my second point: that the validity of one's interpretation can be measured by its logical coherence does not imply that there is an objective measure of logical coherence (in contrast to a rational choice approach, for instance). Put simply, I am interested in whether or not my conclusions make sense to me, and if they are convincing to others. Do they provide a reasonable answer for the questions I was trying to answer? If not, then I try again. Does such a position lead to relativism? Absolutely. My goal as a researcher is to provide an argument about why my interpretation is valid, so that I can convince others that mine is one of the best interpretations out there. In a very real sense, I am constructing my own representation of the representations I am studying – I am very much part of the process of knowledge construction that I am investigating. Being self-reflexive and honest, I admit that I, like all other researchers, am motivated by an array of personal, political, and intellectual agendas. With my work, I am constructing my own discourses. And because I want them to gain social dominance, I am concerned that my conclusions convince other people.

7
Ethnographic Research

Hugh Gusterson

> The anthropologist is always inclined to turn toward the concrete, the particular, the microscopic. We are the miniaturists of the social sciences, painting on Lilliputian canvases with what we take to be delicate strokes. We hope to find in the little what eludes us in the large, to stumble upon general truths while sorting through special cases.
>
> Clifford Geertz (1968: 4)

James Clifford (1997: 56) has, in a much cited locution borrowed from Renato Rosaldo, theorized the methodology of ethnographic research – my craft – as 'deep hanging out.' This perverse phrase captures nicely the improvisational quality of fieldwork, the confusing overlap between informal streetcorner conversation and the serious inquiry embodied in ethnographic fieldwork, and the profound level of understanding of the other for which ethnography aims through apparently casual methods.

This phrase 'deep hanging out' also hints at a contrast between the methodologies of cultural anthropology (which inclines toward the informal) and political science (which is more tightly buttoned). It is my impression, based on limited observation of the training of graduate students in international relations, that political scientists are expected to go into their dissertation research with well-honed hypotheses that aim to prise open crevices in the existing literature based on a careful parsing of independent and dependent variables and a shrewd selection of case studies that might illuminate the relationships between those variables. Political Science graduate students often seem to know what their dissertation will argue, and what the chapter outline will look like, before they have got deeply into the research. While anthropology

graduate students spend years acquiring language skills, working on pre-dissertation literature reviews and writing dissertation proposals, these proposals often focus more on broad questions suggested by the existing literature than on hypotheses to be tested. Meanwhile dissertation committees in anthropology departments tend to expect student research plans to shift as they encounter the vicissitudes of the fieldwork environment: bureaucratic difficulties in accessing a particular site, research subjects disinclined to discuss the topic that seemed so crucial in the student's literature review, research subjects passionately interested in discussing issues the student had not thought to inquire about, and unpredicted events (riots, protests, scandals, conflicts, funerals, celebrations, and so on) that provide unforeseen but compelling windows onto an unfamiliar cultural world.

Moreover, although there are stories of anthropologists such as Melville Herskovits insisting that his students mail their fieldnotes to him from the field for review, most anthropologists report that they received minimal guidance about fieldwork from advisers and dissertation committees either before they went to the field or while they were there. I myself, for example, have never seen another anthropologist's notes, and I am far from unique in that regard (see Sanjek 1990). Anthropologists often assume that each fieldwork situation is different, and that researchers will have to improvise accordingly. Furthermore, first fieldwork is a 'rite of passage' (turning graduate students into mature anthropologists), and it is part of the ritual testing to throw students on their own resources.

In this chapter, stressing the simultaneous informality and rigor of ethnographic fieldwork, I shall take the reader through the key components of 'the ethnographic method.' Although anthropologists often use methods that overlap with those of other disciplines – archival research, written questionnaires, and formal interviews, for example – I focus here on methodological concerns more unique to the ethnographic encounter: gaining access to the field; doing semi-structured interviews and what anthropologists oxymoronically refer to as 'participant observation'; navigating the ethical obligations of fieldwork; and writing up research first through fieldnotes and, later, in ethnographies. (The word 'ethnography,' confusingly, refers both to a method of research and to the finished literary product.) Until the upheavals in anthropology of the late 1980s and 1990s, anthropologists were most likely to study non-Western cultures rather than Western, metropolitan cultures; to study a single localized site; and to focus their studies on those subordinate in status. Recent years,

by contrast, have seen the increasing legitimacy in the anthropology of 'repatriated anthropology,' 'multi-sited ethnography,' and 'studying up' (Nader 1974; Clifford and Marcus 1986; Marcus and Fischer 1986; Marcus 1995).

I shall draw opportunistically on the relatively small methods literature in anthropology and on what I know of others' fieldwork, but I shall also draw considerably on my own experience doing ethnographic research among American nuclear weapons scientists and, to a lesser extent, antinuclear activists. My original dissertation fieldwork in the San Francisco Bay Area in the late 1980s, part of the disciplinary transformation, was on the Lawrence Livermore National Laboratory, the nuclear weapons laboratory that designed the warheads for missiles (Gusterson 1996). I was trying to understand how scientists came to feel that they had a vocation to design nuclear weapons; I also wanted to describe the phenomenology of weapons work, the effect of weapons work on marital and family relationships, the relationship between the weapons laboratory and local institutions ranging from churches to the town council, and the impact upon the laboratory of the sizeable antinuclear protests of the early 1980s. I interviewed many of the protestors as well, and at one point accompanied a group of anarchists from the Bay Area on a weeklong protest trip to the Nevada Nuclear Test Site.

More recently, for a follow-up book, I have been doing multi-sited fieldwork among weapons scientists at both the Livermore and the Los Alamos nuclear weapons laboratories; among antinuclear activists in California, New Mexico, and Washington DC; and sporadic interviews with senior bureaucrats from the nuclear weapons complex wherever I can find them. If my earlier fieldwork focused largely on rank-and-file weapons scientists, this research has been more centered on senior managers of the weapons laboratories and on major players in the Washington defense bureaucracy – busy decision-makers who are not easily accessed. The purpose of this research is to trace the process by which the national security bureaucracy (especially the nuclear weapons complex) came to acquiesce in the suspension of nuclear testing and the negotiation of the Comprehensive Test Ban Treaty in the early 1990s (Gusterson 2004). If the first research project was anchored to a single, localized site – the Livermore Laboratory – the second project has, in keeping with a more general anthropological evolution away from a preoccupation with the local, focused much more on diffuse networks, structures, and processes that are both national and international in scale.

Accessing the field

Like the space shuttle entering the earth's atmosphere, the ethnographer entering the field must get the angle of approach just right, or the resultant friction may burn up the mission. Unlike shuttle astronauts, ethnographers have widely varying missions, each with different optimal angles of approach. Sometimes what opens the village doors can be quite unpredictable, especially to an outsider. Paul Stoller (1989: 40–1) reports that he made little headway in penetrating the world of sorkos – magician-healers in Niger – until the day a bird defecated on his head. This was taken by a sorko who witnessed it as sign that Stoller was chosen for apprenticeship.

What works for one ethnographer seeking entrée to the field may prove disastrous for another. Margaret Harrell (2003), for example, is an anthropologist who studied US military families. She reports that a letter from a commanding officer directing military personnel to cooperate with her was indispensable to her fieldwork. By contrast, the anthropologist Philippe Bourgois (1995), who did fieldwork with crack dealers in New York's Spanish Harlem, would have been crippled by the endorsement of uniformed authorities and, in his case, being mistreated by the police on one occasion helped his fieldwork considerably. In general, ethnographers entering the field seek to ally with gatekeepers who will vouch for them and to avoid falling in with the wrong crowd – the only problem being that, as you enter an unfamiliar cultural situation, it can be quite hard to tell which is which.

Ethnographers are inevitably marked in the field by their race, class, gender, education level, nationality, and other characteristics. In some contexts, aspects of the researcher's own identity may play a facilitating role: in others they may be crippling. It is hard, for example, to imagine a woman doing Loïc Wacquant's (2003) research with boxers in Chicago, or a man doing Elizabeth Fernea's (1969) research among the wives of a sheikh in Iraq or Stephanie Kane's work with female prostitutes (1998). Ethnographers inevitably have to decide which aspects of a field environment are more or less accessible or closed off by virtue of their own identity.

In my own case, when I decided to do an ethnography of the Lawrence Livermore National Laboratory, my problem was that I was a foreign (British) citizen attempting to study a top secret military facility where I knew nobody and to which access was largely forbidden for those without clearances. I thought of making a formal approach to the Laboratory's management for permission to study the facility, but

decided the likelihood was low that such permission would be granted and, once denied, it was not inconceivable that Lab management would actively obstruct more informal approaches to their weapons scientists. In the end, I tried a scattershot approach of three simultaneous entry strategies, only one of which was truly fruitful and one of which was nearly quite damaging.

The first strategy, joining my practical need for accommodation with my interest in meeting laboratory employees, was to look for a room in a house occupied by lab employees. Over the course of 2 years of field-work, I lived in three different houses with different kinds of laboratory employees – a technician, a computer programmer, and an engineer. Over time, I heard a lot of gossip about the Lab from these employees, who I got to know well as individual friends. However, they did not introduce me to many other lab employees, and it is dangerous to rely on single sources to understand a complex institution employing over 8000 people. I felt as if I were slowly developing a deep understanding of very tiny and isolated pockets of laboratory life from my roommates.

Roommates were, however, a particularly good source of basic orientation information. Disorientation is one of the strongest sensations of the ethnographer newly arrived in the field. Consequently, the beginning of field research is often dominated by an attempt to simply get one's bearings by asking lots of very basic questions. In my case, these questions included the following: Why do some people have red and others green badges? How many directorates are there at the Lab, and what do they all do? What is that tall building in the middle of the Lab I can see from the perimeter? What kinds of clothes do people wear to work at the Lab and how should I dress when meeting them? Is it alright to talk about 'bombs' or should I call them 'devices'? What is a CAIN booth? (It regulates access to restricted areas of the Lab for those with clearances. An employee stands in the booth and swipes a card, as if at an ATM, entering a secret code, and is then granted admission.)

My second strategy was to make use of one chance contact I had made at a party a few weeks before coming to Livermore. At this party I met a woman and her husband, who worked as a scientist at the Lab. They both lived in Livermore, and the wife was especially interested in my research. She invited me to lunch with a promise that she would provide me entrée to a wider network in Livermore. I noted that she brought her teenage daughter to lunch and seemed uncomfortable. I was fortunate to discover from a friend of the couple that her husband (who was not keen on talking to me) was concerned that my interest in his wife was not purely academic, so I moved on. As my research unfolded, I

observed that scientists were happy to talk to me when I was introduced through networks of scientists at the Lab, but often resisted talking to me if the introduction came through their spouses.

The approach that worked, my third strategy, was the result of extraordinary serendipity. My graduate student advisor mentioned to me that he was supervising an undergraduate thesis on the town of Livermore by a student who grew up there. I contacted the student and found that his father worked at the Lab. The son arranged for me to go and visit his father. I anticipated discussing with the father the feasibility of my study and getting his advice on how to approach people. Instead, when I arrived at his home at seven o'clock one evening, he said, 'Take out your notebook. I will tell you my life story.' I said very little for the next 2 hours, at the end of which I had pages of fascinating material about a man who had fled North Korea as a teenager, come to the United States with nothing, trained as a physicist, and sought work as a weapons scientist because of what he referred to throughout the interview as his 'monolithic anticommunism.' He demonstrated for me that evening that the way to understand lab employees was not to ask a series of abstract questions about their ideological beliefs but to elicit life histories that crystallized their commitments in narratives of the events through which they were enacted – a technique whose power has been beautifully demonstrated in Faye Ginsburg's (1989) ethnography of pro-life and pro-choice activists in the Mid-West, published just 2 years after my conversation with the Korean scientist in Livermore. At the end of our encounter, the scientist offered to put me in touch with five more lab scientists if they agreed. They did. Each of them referred me to still more colleagues, and the rest was history.

This technique of building an exponentially increasing network of research subjects from an original subject zero is referred to in the methodological literature, for obvious reasons, as the 'snowball technique.' Its strength is that people who trust one another trust those referred to them through the network. Its weakness is that it does not operate through random sampling, and there is an obvious danger that the ethnographer will get trapped inside the network's echo chamber and will be confused by what he or she hears there for the wider discourse of an entire institutional setting (see also Ackerly in this book). In my own case, I was confident that I was reaching a wider sample partly because my collection of interviewees was so large, and partly because I deliberately pushed interviewees to refer me to others chosen to diversify my sample.

I also, over time, further diversified my pool of subjects by searching for interlocutors in other settings too. In a context where about three quarters of weapons scientists identified themselves as active Christians, church attendance proved an important way of getting to meet them as well as building relationships with their pastors, who also became interview subjects. I joined a softball and a basketball team at the Lab; I joined the Lab singles group (more of a Friday evening and weekend outings club than a dating arrangement); I hung around bars in town, and I sometimes went for lunch to the Lab cafeteria, which was open to the public and proved a good place to cajole scientists I already knew into introducing me to others.

The pool of interlocutors I developed through these techniques has been important also for my newer research on the weapons laboratories' adaptation to the end of nuclear testing. I have gone back to some of these interlocutors to explore their reaction to life in a weapons laboratory without nuclear testing. However, my new research has focused much more on very busy senior managers than my earlier research did. In securing interview access, I have been fortunate to be able to build on the success of the first research project: that research secured me a professorship at MIT, which is a highly respected institution at Livermore and Los Alamos. Senior managers there will usually make time to talk to an MIT professor. Beyond that, my original research has now been widely profiled in local newspapers, it produced a book that many lab employees have read, and I have written a number of articles for local newspapers. This has given me a measure of legitimacy around town, and it gives potential interlocutors a sense that I am a known quantity who can be trusted as much as any outsider can. One lesson to draw is that when anthropologists' relationships to research sites carry on across a decade or more, as they often do, they deepen over time, opening up new vistas of understanding.

Participant observation

If you asked an older generation of anthropologists to define 'the ethnographic method,' they would put 'participant observation' at the center of it. Participant observation, the essence of the 'deep hanging out,' denotes a method of research in which ethnographers join in the flow of daily life while also taking notes on it (either in real time or shortly afterwards). If the locals went hunting, harvesting, drinking, feasting, or pilgrimaging, the anthropologist tried to go with

them, often to do it with them, and to record as accurately as possible what was said and done.

There are many obvious benefits to participant observation. First, this level of sustained contact with research subjects helps to build relationships of trust and intimacy with them. Second, seeing for oneself what people do and choosing what to record of it is surely far better than learning about it after the fact in a fragmentary fashion from documents or informant interviews. It is the difference between sitting in someone's living room with them and peeking in through a keyhole. Finally, participant observation is a particularly effective way of exploring the difference between the 'frontstage' and 'backstage' – between formal, idealized accounts of a culture and the messy divergences of actual practice. Imagine what a Martian ethnographer would believe about the way an American university works if they relied on formal interviews with faculty and staff, and then imagine what they would learn instead if they went to faculty meetings and gossipy lunches with the staff while living in a student dorm in the evenings, and you will get my point.

Some of my favorite ethnographies use participant observation for particularly good effect. In *Peyote Hunt*, Barbara Myerhoff (1976) accompanies a group of Huichol Indians led by Ramon, a shamanic figure, on a long pilgrimage into the Mexico desert to the Huichols' original mythic home and home still to their gods. Their pilgrimage culminates with the sacred ingestion of peyote and with the harvesting of the hallucinogenic buttons for rituals for the coming year. Her participation in the pilgrimage and its visionary culmination enables her to get inside Huichol cosmology and mystical religious experience as much as any outsider can. Myerhoff's narrative has a cinematic quality. As she relates, with a novelist's eye for detail and drama, the pilgrims' jokes, the reader feels that he or she is alongside the Huichols in their journey. *Of Two Minds*, by Tanya Luhrmann (2001), looks at the socialization of American psychotherapists and psychiatrists. Her description of the way medical residents learn their trade and internalize diagnostic categories of mental illness is particularly enlivened by the fact that she put herself through the same apprenticeship in order better to understand it.

Participant observation has been especially important in ethnographic investigations of American poverty. This is because there is often a sharp divergence between, on the one hand, judgmental assumptions about the poor that circulate in the media and among policy makers and, on the other hand, the lived experience of poverty. In books such as Carol Stack's *All Our Kin* (1997) and Philippe Bourgois' *In Search Of Respect* (1995), privileged white ethnographers reposition themselves by living

in the midst of poor black and Hispanic communities. More effectively than any dry, statistics-laden policy study, these ethnographies build a picture of the exhausting daily grind of lives lived in poverty, of creative adaptations to poverty that are also entrapping (such as crack dealing), and of the barriers to escaping the ghetto that are so much more clearly visible from within than outside. But the ultimate exercise in participant observation in poverty was conducted not by a professional ethnographer but by the journalistic public intellectual Barbara Ehrenreich. In her justly celebrated book, *Nickel and Dimed* (2002), she goes undercover, working as a low-end waitress, a hotel maid, and a Walmart worker. Ehrenreich records not only the mass of petty brutalities against the poor in the workplace but also keeps an exact ledger of the financial costs faced by low-income workers versus the income they can secure. By the end of the book, one thinks it a miracle anyone moves up from this life at all.

Given the insights participant observation facilitates, I regret the limited role it played in my own fieldwork among weapons scientists. Although I spent as much time as possible simply 'hanging out' with Lab employees in church, in their homes, and on hikes, I sometimes wonder what I might have seen had I been allowed to come into the Lab day after day with my notebook and fade into the background. Anthropologists of science who have been given full access to scientific laboratories have often written ethnographies that focus on the microprocesses through which scientific facts are constructed (Latour and Woolgar 1986; Fujimura 1996; Knorr Cetina 1999). I suspect that, had I engaged in participant observation within the Lab itself, I would have written an ethnography more focused on disputes over weapons design details, the bureaucratic relationships between different ranks and categories of employees, and the phenomenological disconnect between small daily tasks within the laboratory and the laboratory's larger project of developing a massive arsenal of weapons of mass destruction capable of liquidating hundreds of millions of people. As it was, my enforced positioning on the margins of laboratory life produced an ethnography that foregrounded secrecy practices within the Laboratory and the Laboratory's relationships with other institutions, and my residence outside the Laboratory fence but within the homes of weapons scientists made me particularly sensitive to the role and experience of laboratory spouses.

Despite the circumscribed role participant observation played in my field research, there are still things I would not know without having engaged in it. For example, I recall being in the cafeteria of the Livermore

Laboratory when CNN started to broadcast the story of the Oklahoma City bombing. As I watched weapons scientists around me turn up the volume on the cafeteria TV and, using CNN's details about blast damage, rush to calculate the power of Timothy McVeigh's bomb on the backs of their white paper table napkins, I viscerally understood something about the phenomenology of their craft. Other informal interactions have also been instructive. By befriending a new Lab employee and watching her mounting anxiety as her investigation for a security clearance dragged on for months, I came to understand, better than I could through interviews, the indispensability of a clearance, the petty humiliations of life without a clearance, and the terror an employee feels at the prospect of denial. Taking a long and beautiful dog-walk with another employee, I was stunned by a torrent of criticism of the Director of Los Alamos that he (and his colleagues) had held back in other interactions. I have also found that rank-and-file weapons scientists talking over a beer joke to the detriment of their managers and evince much more skepticism about the new simulation technologies being developed at the weapons labs than similar scientists do in tape recorded interviews or than managers in any context I can access. Rank-and-file weapons designers' informal narratives of the origin of these simulation technologies are more likely to stress pork barrel deals in Washington, whereas more formal interviews with managers accent the scientific and technical logic of the technologies and the overall rationality of the program of stockpile stewardship. In other words, my ability to 'hang out' with ordinary weapons scientists gave me special insight into the gulf between 'frontstage' and 'backstage' narratives of the stockpile stewardship program, between what is said in public and what is whispered or said jokingly in private.

A second example comes from my parallel fieldwork among antinuclear activists. As these activists prepared to go on a week-long protest to the Nevada Nuclear Test Site, I attended their preparatory workshops where I heard first-hand about people using sick days and vacation time to keep their jobs while they went on the protest. I joined with them as they role-played being arrested and subjected to police brutality, and having their planning meetings infiltrated by undercover police officers. Then I traveled with them to the Nevada Desert, where I lived in a tent for a week with no running water and was taught by those around me how to deal with the extremes of heat and cold in the desert in spring. Finally, I shared their experience of civil disobedience. Without having gone through all this myself, I do not think I could so easily grasp the extraordinary sense of community among the activists, the sacrifices many of the protestors made to be there, or

the relationship between the privations of protest and the strange rush of euphoria from civil disobedience. Nor, without my time amongst the protestors, would I have recognized with such clarity the mistaken nature of comments made by members of the Livermore community characterizing the protestors as communists and unemployed folks who had nothing better to do.

A final contribution made by participant observation is more amorphous and mysterious, but no less important for that. It concerns the reformation of my own emotional relationship to nuclear weapons. When I arrived in Livermore in the mid-1980s, I did so as someone who had been deeply concerned about the possibility of superpower nuclear war to the point of even having occasional nightmares about it. By the time I left Livermore 2 years later, I had lost my subjective fear of nuclear weapons and have never been able to recover it. It just disappeared! I am unable to give a precise account of the processes involved here but it is clear that, in some way, living amongst people who joked about nuclear weapons and took for granted the human ability to control these weapons, I absorbed their sense of ease – or, if you prefer, their ability to live in denial.

Semi-structured interviews

The core of my research consisted of semi-structured interviews organized around the elicitation of life histories. I collected well over a hundred of these interviews, which were almost always tape-recorded. This was important because I was interested in the exact language scientists used to describe their beliefs and experiences, and because my interlocutors attached great importance to precise quotation of their remarks. In my original research it was through such semi-structured interviews that I came to understand how weapons scientists understood the ethics and politics of their work, how they reconciled their weapons work with their religious commitments, how they experienced the weapons design process emotionally, and how weapons work affected family life. In my more recent research, I have used such interviews to reconstruct negotiations about the end of nuclear testing at the higher levels of the weapons bureaucracy, to understand the purpose of new simulation technologies being built at the weapons labs, and to elicit the response of rank-and-file weapons scientists to the end of nuclear testing and the emergence of virtual nuclear weapons science.

Many social scientists, less interpretively focused than I, are deeply concerned about the exact comparability of their subjects under the

research microscope. Sociologists devising questionnaires, for example, seek to ensure that, however diverse their pool of research subjects, they are responding to the same questions. Here it is the consistency of the questions posed to different individuals or populations that enables the sociologist to make differentiating generalizations: everything comes back to the way different people respond to the same questions. If each interview or questionnaire is different, then comparison is clouded.

While the benefits of such a research protocol are obvious, it also acts as a straitjacket. If, as Sharon Hutchinson (1996) says, ethnography is 'the fine art of conversation,' individuals like to talk about different things and, by insisting on precise comparability, this research methodology prevents the detailed exploration of individuality. It also tends to bore research subjects, forcing them into a kind of mass-produced superficiality. In my interviews there was a core set of questions I asked everyone: where were you educated? To what level and in what subject? What are your religious commitments? What is your work at the Lab or in the antinuclear movement? How did you come to decide to do weapons work? Has anyone in your family or beyond given you a hard time for working on weapons? Such questions, as well as producing a matrix for comparison, served as icebreakers and orienting probes for deeper conversations that followed. But beyond this elementary set of common questions, my interviews with different research subjects diverged quite substantially as I followed strategies I call 'branching' and 'building.'

My interviews followed a 'branching' pattern as I tailored them to individual interests and identities. Interviews followed different trajectories for physicists and engineers, for the elite weapons designers and the scientists who worked under them, for Christians, Jews, and atheists. Interviews also branched in different directions as my line of questioning responded to what individual scientists showed particular interest in discussing.

As for 'building,' each interview built upon earlier ones as my understanding of the Lab deepened and expanded over time, and interviews I did at the end of the research project were quite different from those conducted at the outset. I came to think of myself as having conversations not just with unique individuals, each fascinating in his or her own right, but also with a single entity: a discourse community. As these unfolding conversations suggested recurrent discursive themes, new avenues of inquiry, or newly evident lacunae in my own understanding, so the questioning shifted, each conversation establishing

a new beachhead as I probed more deeply into the culture of the Lab or, sometimes, circled back recursively to check anomalies and uncertainties.

Researchers who subscribe to more positivist understandings of the world than I do assume that research subjects have stable 'values,' 'preferences,' 'beliefs,' 'ideologies,' or 'cultures' and that it is the researcher's job to find out what they are as cleanly as possible (to some extent, Checkel and Hermann, in this book). But I soon noticed that subjects I interviewed more than once might contradict themselves in interesting ways, or that some interviewees presented themselves quite differently to journalists and to me. Positivists would see such fluctuations as 'noise' to be eliminated in order to ascertain what the informant 'really' thinks. I came, instead, to see these instabilities of discourse as themselves part of informants' cultural identities. And if, for example, a scientist's statements about the Russians showed little fluctuation while his or her comments about the ethics of weapons work were variable, this variability was itself an important ethnographic datum.

Just as Lao Tzu said that no two stones can be thrown in the same river, so I would say that it is not possible to interview the same subject twice. Thus, rather than thinking that I was sampling or eliciting a stable, pre-existing reality as objectively as possible, I began to think of interviews as dynamic events through which the identity of the subject was performed and even co-constructed by the interviewer and interviewee. In these conversations, interviewees did not so much manifest an unchanging essence there, like some geological pattern, plain for any researcher to see if they knew how to scrape away the surface. Instead they drew on the complex repertoires of their speech community to perform themselves in response to particular lines of questioning (How is your work ethical? Do you think nuclear war will happen? How do you deal with antinuclear activists?) that often reflected my own past in the antinuclear movement. A different interlocutor with different preoccupations would have provoked different performances of self since, as Renato Rosaldo (1989: 19) observes, 'the ethnographer, as a positioned subject, grasps certain human phenomena better than others. He or she occupies a position or structural location and observes with a particular angle of vision.' And, of course, as my earlier discussion of the way my interviews built upon one another makes clear, I was changed by each interview too: no two interviews were done by the same interviewer.

At their worst, these interviews produced the ethnographic equivalent of American Presidential debates: stale performances using rehearsed lines and recycled snippets from the Laboratory's public relations

campaigns. At their best, the interviews produced performances of self in modest kitchens and living rooms around Livermore that were profound, touching, revelatory, funny, counter-intuitive, and educative. The role of interviewer affords a license to ask questions of a kind that would not normally be permitted for strangers – indeed, even for friends in most contexts – while the act of sustained, attentive, supportive listening can be powerfully enabling for the person being heard (and indeed, in a different way, for the listener as well). This kind of listening – accompanied by requests to clarify apparent contradictions, to tie emotions to recalled events, or to address narrative gaps – can induce a creatively reflective state of mind as interviewer and interviewee move into a zone of interaction that hybridizes therapeutic encounters and journalistic interrogations.

Some of my interviews lasted 4 hours. One lasted for 15 hours, spread over a series of sessions, which a retired scientist taped as a bequest for his daughter. (When I attended his funeral after he died of Alzheimers a few years later, I felt a secret and special bond to him.) I began to realize that, as scientists reflected on the ethics of their work, reconstructed their decisions to come to the laboratory, and recalled their emotional responses to nuclear tests they had experienced, they were sometimes opening spaces they shared with few others. One wife, eavesdropping on my interview with her husband, interrupted to say, 'How come you told him that? You've never told me that!' Many scientists told me that they thought about the ethics of their work but none of their colleagues did – a clear indication that everyone was thinking about nuclear ethics, but quietly and in private. The interviews, then, generated articulations not only of fiercely public ideologies, but also of the private, the whispered, the half crystallized on the edge of consciousness. And once these articulations became public, as they were pushed back into the community through my writing, then in a modest way they changed the field of discourse I had come to study.

Inscriptions

In earlier generations, anthropologists passed many of their evening hours typing up index cards. These cards enabled them to store and sort information they had gathered on, say, patrilateral cross cousins, funeral rituals, or witchcraft beliefs. Doing fieldwork in the computer age, I use the cut-and-paste function of Word to do some of the work for which those anthropologists used index cards. However, I mainly organize my notes around interviews and interactions with individuals, recording

their exact words whenever possible. The fundamental organizing principle of my notes, then, is the individual biography, though I do also sort information on my hard drive and in manila folders around themes. Sometimes I take a pair of scissors to printed transcripts of interviews, scattering textual shards to differently themed manila folders. Clearly there is a relationship between the organization of my notes around individuals and the fact that my writing often makes use of long quotes from individual informants and, on occasion, features extensive profiles of individual research subjects (Gusterson 1995a,b).

How do ethnographers know when it is time to leave the field and start writing? Often, they have no choice: their research funds dry up or their sabbaticals end, and they go home with whatever notes they have. In my own case, I felt that fieldwork was getting stale when I found myself often able to predict how research subjects would answer my questions. While I was still learning new things, this meant that my understanding of the culture was achieving a certain depth and stability and was, to some degree, plateauing. It was time to stop talking and start writing.

In preparing to write, I read my notes on interviews with individual interlocutors, as well as transcriptions of them, flagging recurrent patterns, variations on themes, and quotable passages. The recurrent patterns have ranged from noting that Livermore scientists are more optimistic about simulation technologies than Los Alamos scientists to observing the use of similar metaphors by different people, often people who do not know one another. Examples include the use of birth metaphors to describe the process of designing and testing a nuclear weapon, the use of machine metaphors to describe the human body, and the use of anthropomorphic metaphors to describe machines.

Ethnographers of my generation, often influenced directly or indirectly by the writings of Michel Foucault, tend to see human cultural worlds as constructed by the intersecting power of ingrained cultural practices and the discourses through which people speak about their world. When we do fieldwork we note these practices and we record as much of the discourse as we can, looking for recurrent patterns. Just as psychotherapists have to talk to people at the conscious level in order to deduce what is happening in their unconscious worlds, so anthropologists have to observe and talk to individuals (or groups of individuals), but are really interested in the practices and discourses that transcend the level of the individual and, to put it in Foucauldian terms, provide the social material from which their individuality is constructed.

(See Neumann and Dunn, in this book, for examples of finding such discursive patterns at other levels of analysis.)

Writing this up as ethnography poses its own set of challenges since, compared to political science monographs, the criteria for writing and judging ethnography are much looser and more heterogeneous. Ward Goodenough (1981), likening culture to language, said that an ethnography was a sort of cultural grammar book, and that, just as a grammar book would teach you how to speak a language, so an ethnography should teach you how to behave appropriately in a particular culture. Clifford Geertz (1973), seeing culture as a text to be interpreted rather than a set of rules to be followed, thought good ethnography gave deep insight into the nuances of an alien lifeworld and into the meanings by which its adherents struggled to live. Carolyn Nordstrom (2004: 14) meant something similar when she said 'ethnography must be able to bring a people and a place to life in the eyes and hearts of those who have not been there.'

Such descriptions imply that the goal of ethnography is just particularistic description. However, as the quote by Clifford Geertz at the front of this chapter makes clear, the point of ethnography is to describe the particular in order to illuminate 'general truths' – the functioning of capitalism, the nature of ritual, the experience of oppression, say. In general, anthropologists would agree that good ethnography gives a rich evocation of the cultural world it describes while also contributing something to theory and being of interest to those who are not specialists on the culture area described. They would also say that it should 'feel right' to those other area specialists – though not being without surprises – and that it should give a thick enough description that readers could draw their own inferences about the culture being described.

The index card generation of anthropologists often said in their writing that 'the data suggest' and 'it was observed that...,' but who collected these data and by whom was it observed? References to 'data' and the use of the passive voice – the hallmarks of what Donna Haraway (1988) calls the 'God's eye view from nowhere' – are the familiar tropes of phony objectivism. Of course, we check our facts, quote people accurately, and do our best to make sure we know what people mean by what they say, but in the end 'data' are collected and written up by individual researchers who have their own concerns, insights, and blind spots. For this reason, as I have done throughout this chapter, I use the first person in my writing, in order to remind the reader that the 'data' have been collected, sifted, organized, and represented by a particular

individual who readers have to decide whether or not they trust. As a way of helping readers to make up their minds, at the end of my first book, *Nuclear Rites*, I also gave a page each to a handful of key informants to comment on the book.

Human subjects and ethics

In the United States, government agencies such as the National Science Foundation (NSF) and the National Institutes of Health (NIH) require that research they fund be approved by university panels for the protection of human subjects and refuse to disburse money until these review boards have approved it. In the wake of scandals such as the death of Jessie Gelsinger, a healthy 18-year-old killed in 1999 by poorly conceived gene therapy research at the University of Pennsylvania, universities are also increasingly concerned to review the safety of human subjects in research conducted by their students or faculty (Stolberg 1999). (For an example of a human subjects tutorial and exam, see http://web.mit.edu/committees/couhes/.) While the process of human subjects review gives universities more control over research for which they may be legally liable, it can also benefit researchers, since the university effectively legitimates the research it has approved and indemnifies researchers in the event of legal action.

Many anthropologists see human subjects review boards as, at best, institutions that slow research with unnecessary red tape and, at worst, the preserve of curmudgeonly bureaucrats from other disciplines who do not understand the unique exigencies of ethnographic fieldwork. In the past, conflicts have focused in particular on consent forms. Human subjects bureaucracies like consent forms because they clarify the contract between researchers and subjects while providing tangible evidence that subjects agreed to be studied. Anthropologists often dislike consent forms, first, because their subjects may not be able to read and are often suspicious of people bearing bureaucratic paperwork and, second, because in many Third World countries (especially those with overly energetic police forces) the quickest way to lose a subject's friendship and cooperation is to ask them to sign a form saying they agree to inform on their country to a foreigner. Consequently, anthropologists are sometimes tempted to engage in research under the human subjects bureaucracy radar or to diverge from written protocols in research practice.

Readers should not infer from this that anthropologists are indifferent to the well-being of their subjects. In my experience, the opposite is true.

But, in keeping with the informality of anthropology, it is often assumed that human subjects are best protected not by inflexible bureaucratic codes but by ethnographers who think situationally about an internalized mandate to 'do no harm.' Such a perspective is affirmed by the current language in the American Anthropological Association (AAA) ethics code (http://www.aaanet.org/committees/ethics/ethcode.htm (See also Fluehr-Lobban 1998, 2003)), which states,

[I]t is understood that the informed consent process is dynamic and continuous; the process should be initiated in the project design and continue through implementation by way of dialogue and negotiation with those studied. Researchers are responsible for identifying and complying with the various informed consent codes, laws and regulations affecting their projects. Informed consent, for the purposes of this code, does not necessarily imply or require a particular written or signed form. It is the quality of the consent, not the format, that is relevant.

The 1971 version of the AAA ethics code took a particularly strong stance against secret consulting by ethnographers. Reflecting general disapproval of anthropologists who secretly consulted for the American national security state during the Vietnam War, it said,

'In accordance with the Association's general position on clandestine and secret research, no reports should be provided to sponsors that are not also available to the general public and, where practicable, to the population studied... Anthropologists should not communicate findings secretly to some and withhold them from others.'

In response to lobbying from anthropologists who consult for the private sector and are concerned about proprietary data, that language has now been watered down. The current AAA ethics code merely says that anthropologists 'must be open about the purpose(s), potential impacts, and source(s) of support for research projects with funders, colleagues, persons studied or providing information, and with relevant parties affected by the research.'

Still, even in its contemporary weakened version, the ethics code stresses the importance of obtaining the informed consent of those being studied:

'Anthropological researchers should obtain in advance the informed consent of persons being studied, providing information, owning or

controlling access to material being studied, or otherwise identified as having interests which might be impacted by the research.'

This is quite different from the ethics code of, say, the American Psychological Association, which allows for the routine deception of subjects in psychological experiments, provided this deception has been approved by human subjects review boards and as long as it is explained to research subjects after the completion of the experiment.

Two famous scandals in anthropology underline the ethical dangers of the ethnographic method. In 1983, Stanford University (the department in which I was trained) denied a PhD to Steven Mosher on ethical grounds. Among the concerns, he was accused of taking photographs without their consent of women undergoing abortions and of endangering research subjects who criticized China's birth control policies by not concealing their identities (Sun 1983; Turner 1983; Lee 1986). And the journalist Patrick Tierney (2000) unleashed the biggest controversy in 30 years by claiming that, in the 1960s, James Neel had exacerbated a deadly measles epidemic among the Yanomami of Venezuela through his inappropriate use of a flawed vaccine and that Napoleon Chagnon, complicit with Neel, staged fights among the Yanomami to make his documentary films more interesting, among other charges. In the confusing debate that followed, Tierney softened some of his allegations, and over time the charges against Neel began to look much weaker than those against Chagnon (Sahlins 2000; Borofsky 2005).

Such scandals aside, most anthropologists do show concern for the well-being of the human subjects with whom they work. If one listens to corridor talk among anthropologists, they tend to be concerned about protecting the confidentiality of their interlocutors and about advocating for underprivileged communities they study. Many anthropologists donate book royalties or other income to communities with whom they may have a lifelong research relationship, and they often go to special lengths to secure medical or educational help for individual interlocutors with whom they have particularly close relationships. One of my colleagues at MIT recently paid for the medical care of an ailing informant, for example, and then for his funeral.

Anthropologists who work in war-torn parts of the world also fret that their work might inadvertently facilitate government repression, the maneuvers of death squads, and so on. It is said, for example, that some anthropological work on Mayan textile patterns may have helped Guatemalan death squads identify indigenous communities for liquidation. The French anthropologist Georges Condominas was horrified to learn that the US government had (illegally) translated and distributed

his ethnography of a Vietnamese people to Green Berets during the Vietnam War and that his research subjects were subsequently tortured (see Berreman 1980). There are even instances of anthropologists who have left book manuscripts unpublished out of such concerns. Ever since the AAA was torn apart in 1968 by revelations that some anthropologists were secretly consulting on counter-insurgency in Southeast Asia for the US national security state, most anthropologists have kept their distance from such agencies as the CIA, the Department of Defense, and even USAID that might be interested in their knowledge of populations around the world (Berreman 1974; Wakin 1992; Price 2000, 2004). After 9/11, some suggested that anthropologists should contribute their expertise to the war on terror by working more closely with US national security agencies, but this suggestion has been more condemned than approved within anthropology (Gusterson 2003, 2005; Wax 2003; McFate 2005; Moos 2005a,b; Price and Gusterson 2005).

As for my own research, I have had to make sure that my interlocutors understood why I was interested in talking to them. Most of them had PhDs and worked in bureaucratic contexts; they were reassured by a consent form stating that a university Institutional Review Board (IRB) had approved my research, that I was funded by a well-known foundation, and that set forth the contractual terms of our conversations. The most reassuring of these contractual terms was that I promised not to quote them by name – an easy commitment for me to make since it is conventional for anthropologists to invent pseudonyms for those they portray in their writing. The only exception I have made to this rule has been for very senior officials in the weapons bureaucracy who are often quoted in the newspaper and who give explicit permission to be quoted by name.

There were three respects in which my fieldwork relationship with human subjects was unusual for an anthropologist. First, most of the people I interviewed had top-secret clearances and I had to take special care not to jeopardize those clearances. In some cases that has meant not using information people have shared in indiscreet moments; in others it has meant taking particular care to obscure the source of information that, whether or not it is officially secret, does not usually circulate in the public sphere. Second, the antinuclear activists I studied are subjects not only of my inquiring gaze but also, often, of government surveillance. I have been acutely aware that it is difficult to draw a clear line between writing that explains the cultural logic of the antinuclear movement in ways activists themselves might appreciate and writing that might feed into the intelligence-gathering of government agencies that do not wish these activists well. I have tried to write about the symbolic and ideological systems of activists rather than about their operational

procedures, though this skews my writing on this subject. Third, my commitment to fieldwork among both weapons scientists and activists – two communities deeply antagonistic to one another – poses a special burden. I have had to make sure that each community understood that I was also talking to their antagonists, but also to take care not to let either community use me as an intelligence agent against the other.

Conclusion

At the outset, I emphasized that ethnographic methods are simultaneously rigorous, informal, and improvisational. There is, obviously, a tension between these three descriptors, but I believe it is a creative one. While I have benefited enormously from reading the work on my research specialty, nuclear politics, and culture, by scholars from other disciplines, I am struck that no other research methodology enables the investigator to grapple with the lived experience of people in the way that ethnography does. Historians are confined by the documents they can find or by the decades-old memories of interviewees; psychologists only access the minds of their subjects through questionnaires or highly staged interviews; while political scientists often reify their material through the deployment, unpersuasive and metaphysical to this analyst, of assumptions about the rational calculations of human actors or the methodological separability of so-called 'dependent' and 'independent' variables. Ethnography is always in danger of lapsing into memoir or journalism at one extreme or obscuring the human beings it studies with relentless theorization at the other, but its creative stew of investigative techniques also holds the promise of a human(e) science that seeks objectivity without objectifying its subjects, that balances rigor with reflexivity, and understands that human action cannot be investigated apart from the local meanings attached to it.

Acknowledgment