



Negotiating Dangerous Fields: Pragmatic Strategies for Fieldwork amid Violence and Terror

ABSTRACT As anthropology turns toward the cultural issues of the 21st century, more and more ethnographic fieldwork is and will continue to be conducted in regions fraught with conflict, instability, and terror. Despite a growing literature that seeks to develop new theories and perspectives for the study of violence, little mention is made of the practical matters of survival in perilous field sites and how the anthropologist's experience of violence in the field should be considered. What is needed is a pragmatic strategy for dealing with threats to the safety, security, and well-being of anthropologists and informants who work amid the menace of violence. Drawing on my own fieldwork in Haiti, I suggest the adoption of new tactics for ethnographic research and survival in *dangerous fields*—strategies that challenge the conventional ethics of the discipline, reconfigure the relationship between anthropologist and informant, and compel innovation in negotiating the exchange of data under hazardous circumstances. [Key words: fieldwork, violence, methodology, ethics, Haiti]

IN 1990, THE AMERICAN ANTHROPOLOGICAL Association (AAA) published a much needed text on the practical hazards of ethnographic fieldwork. Nancy Howell's *Surviving Fieldwork: A Report of the Advisory Panel on Health and Safety in Fieldwork* (1990) represents the first serious endeavor to appraise the multitude of hazards and threats of the field in a systematic and objective fashion. The study evaluates over 80 separate variables, from sunburn experience, robbery, venereal disease, and stinging ants to frostbite, arrest, malaria, and military attack. It is a thoroughgoing endeavor that introduces compelling data on human hazards that are very difficult to quantify—suspicion of espionage, acute conflict, political turmoil, and other like risks.

Eight years later, a volume was published that deals extensively and authoritatively with issues of relevance to methodology in ethnographic fieldwork. *Handbook of Methods in Cultural Anthropology* (1998), edited by H. Russell Bernard, combines chapters by authors from both the scientific and the humanistic traditions in the most recent comprehensive definition and description of the foundational methods of anthropological field practice. Although the text represents a nearly thorough explanation of current techniques for approaching, acquiring, interpreting, applying, and presenting both quantitative and qualitative cultural data, the rather practical methodological issue of coping with crisis, violence, or terror in the field is

addressed at best in passing (Bernard 1998:215–217, 278, 283–284).

Somewhere between these two texts is a gap in the literature that fairly begs for a consideration of how the stress of field hazards affects the ethnographer's observations and data analysis. Though some anthropologists have written thoughtfully about their experiences with conflict in the field, little mention is made of how the reality of lived violence affects or is edited out of anthropological theory, method, ethics, and text. As we continue to conduct research in increasingly hostile and dangerous regions, the very real possibility of our victimization in the field presents a challenge not just to the practicalities of personal safety but also to the ethnographic methods and ethics that we are retrofitting for use in cultures in which ordinary interrelations and social institutions are overshadowed by unrest, instability, and fear. Even exposure to low-intensity repression or harassment over the course of research threatens to adversely affect the ways in which we approach the field and interpret social phenomena within it. What are needed are updated field strategies that address the unique considerations and concerns of the anthropologist conducting research in *dangerous fields*—those sites where social relationships and cultural realities are critically modified by the pervasion of fear, threat of force, or (ir)regular application of violence and where the customary approaches, methods, and ethics of anthropological fieldwork are at times insufficient, irrelevant, inapplicable

cable, imprudent, or simply naive. This article is a contribution to that end.

WORKING IN THE DANGEROUS FIELD

Given the relative frequency with which instability, violence, and crisis emerge in the field, it is not surprising that in recent years a considerable body of anthropological literature on the lived experience of human violence has emerged, while the ethnographic study of terror and resistance has become established as a subfield of its own (Sluka 2000:11). In the past decade, a number of anthropologists who have worked in states of unrest have contributed to the development of new theories and perspectives demanded by the emergent anthropology of violence and terror (Aretxaga 2000; Besteman 1999; Bourgois 1990, 1995a, 1995b; Daniel 1996; Dentan 2000; Falla 1994; Feldman 1991, 1995; Ferguson 1995; Ferguson and Whitehead 1992; Jenkins 1984; Mahmood 1996; Malkki 1995; Nagen-gast 1994; Nordstrom 1997; Nordstrom and Martin 1992a, 1992b; Nordstrom and Robben 1995; Olujic 1995, 1998; Riches 1986; Robben 1995, 1996; Robben and Nordstrom 1995; Sluka 1990, 1995, 2000; Suárez-Orozco 1992; Tausig 1987; Warren 1993, 2000). Increasing interest in cultures of violence, coupled with the rising number of anthropologists who are conducting research in volatile states, has created a sense of urgency for the formulation of strategies and approaches that are reliable, effective, and responsive to the unique methodological and ethical needs of ethnographers who work in dangerous fields. Sluka has recently suggested that "most, if not all, of those who do research on state terror run the risk of suffering re-tributions, even to the extent of themselves becoming targets" (2000:23); anthropological ethics and methodology need to be responsive to this reality.

Since 1994, I have worked with street children in Port-au-Prince, Haiti, where I have been studying the impact of political violence and poverty on the formation of the cultural identities of street children. Specifically, I have been concerned with the material conditions of the street that form the social world of these children; the physical, cultural and social hazards to their welfare; their tactics of survival amid violence and poverty; and how the relationship between street children and the greater society corresponds to new, emergent relations between the civil society and the state. On a much broader level, I have sought to better understand the role that political violence plays in everyday Haitian cultural relations. My choice of Port-au-Prince as a field site was appropriate for studying these issues, given that the instrumentality of terror there for the achievement of political ends has at times been obscured by its apparently indiscriminate and unpredictable application within the ordinary realms of the civil society.¹ The streets of Port-au-Prince have been the explosive terrain upon which violent conflicts over law and democracy have been fought since the bloody coup d'état that overthrew then-president Jean-Bertrand Aristide in 1991.

A fragile constitutional order was reestablished with a U.S.-led U.N. invasion that restored Aristide to the presidency in 1994. But the persistence of gangsterism, drug terrorism, police brutality and corruption, and the activities of paramilitaries and extrajudicial death squads have extended street violence into the present and have prompted aggressive state crackdowns and civil riots. Gunfire, both political and criminal in nature, barks out in sputters at unpredictable times and, recently, at unprecedented targets.²

I have at various times been present at street shootings, threatened, searched, suspected of subversion, and in the midst of crossfire; none of which is an experience unknown to anthropologists who have worked in dangerous fields in North America (Adler 1993; Bourgois 1995a, 1995b; Keiser 1979), Central and South America (Bourgois 1990; Chagnon 1997; Hale 1996; Hecht 1998; Stoll 1993), Europe (Aretxaga 2000; Sluka 1995, 2000), Africa (Besteman 1999; Malkki 1995; Nordstrom 1997; Robarchek and Robarchek 1998), and elsewhere (Daniel 1996; Keiser 1991; Mahmood 1996; Tambiah 1992). Howell's (1990) research indicates that while in the field, at least 42 percent of anthropologists reported experiencing "criminal interpersonal hazards" (robbery, assault, rape, murder), 9 percent reported "arrests in the field," 22 percent reported "living through political turmoil" (revolution, war, rioting), 15 percent reported that they were under "suspicion of spying," 12 percent reported experiencing "factional conflict" (acute hostilities within the group under study), and 2 percent reported "hostage-taking incidents." Indeed, researchers in dangerous fields are extremely vulnerable to these hazards and in many cases cope with them on an everyday basis. The problem, then, is not ascertaining *if* or *how* anthropologists end up in jeopardy in the field but, rather, what can be done to minimize the risks involved in working in the dangerous field. And perhaps just as important, how should the anthropologist's own experiences of, and reactions to, violence be integrated into the presentation of data, if at all?

Allen Feldman points out that "ethnographers, working in diverse zones of political emergency, have been tackling violence, terror, and death through methods of somatic, sensory, affective, semiotic, symbolic, phenomenological, linguistic, performative, and social historical construction" (1995:226). In other words, dangerous fields are customarily approached and engaged through a broad but interrelated range of improvised field strategies. But strategies of improvisation for survival in the field are not commonly discussed in graduate anthropology fieldwork courses, leaving many researchers to hash out crucial matters of personal safety after already finding themselves embroiled in crisis.

Against a backdrop of intimated and actual violence, and until I was forced to evacuate the field because of it, I conducted my research by applying traditional as well as unorthodox field methods, negotiating ethical dilemmas unique to my particular perilous circumstances. In January

2000, amid rising civil violence and political gunplay in the streets, and under the surveillance of the Anti-Gang Unit, a paramilitary arm of the Haitian National Police,³ I left Haiti—frightened, frustrated, uncertain as to exactly how I ended up in jeopardy, and unsure as to how my experiences of violence should be written into my work, if at all. I found out over time that I was not alone in my uncertainties or my frustrations. Carolyn Nordstrom describes the difficulties that she had in rectifying what she was taught about violence in the field and what she was actually seeing in Sri Lanka in 1983, and she suggests that this dilemma compromised her safety:

I racked my memory for everything I had heard or read on mobs and riots to help me. I realized that nothing that I had read or seen in the literature or media presentations on mobs and political violence resembled what I was observing. I felt momentarily angry that people produced “truths” about subjects from the safety of their offices, and that I might get into serious trouble because these “truths” did not match the realities at hand and thus produced no workable blueprint for analysis and action. . . .

Because there was little information available on how to conduct ethnography in violent areas, I did not have preconceptions of what was or was not possible. . . . I encountered several situations where luck, quick reflexes, and the foresight of those around me protected me from physical violence I had not anticipated. While the data I collected were invaluable, hindsight has led me to conclude that our discipline would be well advised to provide its researchers with a more realistic and critical methodology than I first took with me to the field. [1997:xvi–xvii]

RETHINKING METHODOLOGY IN THE DANGEROUS FIELD

Traditionally, the methodologies of cultural anthropology that we have been expected to use in dangerous fields are based on rigid, positivist frameworks and fixed assumptions about the social relations that govern the formalized means of acquiring data. Scientifically weighted and rationally ordered, traditional research strategies have sought to gather and arrange social facts in a purposeful and orderly manner, toward an approximated emic understanding about the field site and its cultural agents. The problem with such an approach is that it assumes ideal field circumstances for interacting with informants (i.e., stability, trust, quietude, security, freedom from fear) and presupposes the ethnographer’s position of control. But what one discovers when working in dangerous fields is that these conditions rarely exist, forcing anthropologists to innovate new tactics and techniques for getting needed data while at the same time minimizing attendant risks to life and limb. While special ethnographic, theoretical, and ethical sensitivities are required when working in dangerous areas, the hazards faced by anthropologists engaged in this kind of fieldwork are substantially mediated through the skillful negotiation of potentially hazardous circumstances (Sluka 1995:277) that are virtually unanticipated by most ethnographic methods. Informal strategies, tactics, and techniques that are sensitive to the emergence of

danger can facilitate an adaptive approach to data collection and ethnographer survival in hostile fields. Sharing the responsibilities of security with informants, selective deception, and a variety of techniques for low-profile data collection can effectively empower the ethnographer coping with danger in the field; but the adoption of such tactics as regular research protocol mandates a radical transformation of both ethnographic method and anthropological ethics.

This subject has been considered before. The problems that dangerous fields pose to ethnographic ethics have been addressed by June Nash (1976) and Richard Jenkins (1984) and more recently and most eloquently by Philippe Bourgois (1990). Patrick Peritore (1990) has discussed the problem of how to adapt field entry and research methods to the security threats present in some sensitive research areas, and Allen Feldman (1991) has discussed the treatment of taped interviews, informant anonymity, and access to the field in the culture of political violence. Although anthropologists continue to remark on the unique circumstances of fieldwork in hazardous areas (Daniel 1996; Feldman 1995; Lee 1995; Mahmood 1996; Nordstrom 1997; Nordstrom and Robben 1995; Sluka 1990, 1995, 2000), since the mid-1990s there appears to be limited discussion of how the dangers of fieldwork might limit the ethnographer’s control over ethical decisions or stimulate methodological innovation in studying violence and terror.

If we are to work in dangerous fields, we must begin with a fundamental shift in how *methodology* is defined—not as a rigid or fixed framework for the research but, rather, as an elastic, incorporative, integrative, and malleable practice. It should be informed by the shifting social complexities unique to unstable field sites and should depend on a level of investigative flexibility on the part of the ethnographer, who cannot always be expected to work in safety and security. The adoption of strategies for research that are responsive to the spectrum of risks existent in dangerous fields facilitates the engagement of data that simply cannot be accessed without an immeasurable degree of risk. On a practical level, these strategies of study should involve a careful determination of how best to approach a research field fraught with peril and tactics for reducing the likelihood that the anthropologist (or informants) will be shot or arrested while doing so.

CALCULATION, COLLECTION, SURVIVAL: CONTEMPORARY PROBLEMS OF COLONIAL LEGACY

The hazards associated with data collection in dangerous fields tempt the ethnographer on the ground to employ a computation of risk versus desired data. In such cases, ethnography is reduced to a sort of calculus involving three dependent variables: the type of information that is being sought, how it will be acquired, and what the risks to the anthropologist are. From this perspective, field methodology begins with a commodification of the data and the

treatment of the dangerous field as the market where those data are exchanged between anthropologist and informant—a determination is made as to how much must be laid down as collateral (in the form of risks to the personal safety and well-being of self and informant) in order to acquire certain information. This formula suggests that the amount of data that can be safely collected derives from the balance of what information is important to the study weighed carefully against how much that information is “worth” in terms of the anthropologist’s and informants’ personal attitudes concerning the relative possibility of dying or being arrested to exchange it. The tendency toward data commodification and risk calculation is familiar to those who work in fields ridden with danger. Peritore points out, for example, that “the researcher must be certain that the research has enough scientific seriousness that potential risks are repaid with significant knowledge” (1990:362). It is a common and understandable tactic in dangerous fields. In the midst of violence, it becomes surprisingly easy to weigh one’s life against the raw material of one’s livelihood. However common or seductive this calculative strategy may be, it is limited insofar as it originates from two problematic assumptions: (1) that the formula for assessing risk in acquiring desired data is a neutral and objective research tool, unencumbered by its roots in colonial endeavors, and (2) that it is possible to extract untainted data from a social milieu contaminated with violence.

The fundamental reality that made anthropology possible from the colonial period through the 1960s was the power relationship that existed between hegemonic European states and their non-European subject cultures. Stability in the ethnographic field was guaranteed through Western suppression of the colonial subjects (who were to become anthropological “subjects”) who occupied it. Talal Asad writes that

the colonial power structure made the object of anthropological study accessible and safe—because of it sustained physical proximity between the observing European and the living non-European became a practical possibility. It made possible the kind of human intimacy on which anthropological fieldwork is based, but ensured that that intimacy should be one-sided and provisional. [1973:17]

The feasibility of anthropological practice was predicated on the pacification of study populations, and so its historical roots lie in a tradition of subjugation that worked to further (whether passively or actively) imperial agendas. The failure of European states (and later North America) to complete the colonial project in some regions yielded unstable states where power fluctuated between centralized organs of administration and decentralized military might “wielded by local warlords or various sorts of insurrectionary leaders” (Giddens 1987:57) beyond the scope of state control. Out of this postcolonial instability have emerged the contemporary states of distress that yield the dangerous fields in which we now conduct research. Seen as such, field violence that comes with the instability of

these states should not be regarded by the ethnographer as anomalous or aberrant given the historical context—it is a social byproduct of colonial hegemony.

The states within which many dangerous fields are contained are intrinsically *weak*, in that they lack the sophisticated infrastructural surveillance tactics that would allow for more subtle, manipulative practices of power (Gledhill 1994). As such, the agents and proxies of such states *must* resort to kicking in doors, torture, and the institution of death squad activities to achieve desired ends because internal social control has become dependent on a state’s ability to terrorize its citizenry into conformity. For the anthropologist who works therein, the tendency toward a calculation of risk in “extracting” data derives from a misguided professional nostalgia for the stability once guaranteed by colonial force. When considered historically, the calculative strategy for data extraction that is so seductive to the ethnographer in the dangerous field can never be value free.

Haiti provides a case in point. Over the course of a particularly repressive postcolonial occupation of the country (1915–34), the United States guaranteed a political stability wholly predicated on the involuntary conscription of labor crews and the violent suppression of civil resistance. With the pacification came the anthropologists, many of whom forwarded the cause of the occupation with skewed ethnographies that falsely suggest savagery, cannibalism, sexual depravity, and human sacrifice on the part of Haitians (Craig 1933; Kuser 1921; Seabrook 1929; Williams 1933; Wirkus 1931). When the occupation ended in 1934, the U.S. Marines left behind the *gendarmierie*, the predecessor to the Haitian Army that Michel-Rolph Trouillot indicates would “kill as many Haitians during the second half of its 122-year-long history as it had Frenchmen during the war for independence” (1990:105). Because the violence perpetrated on the streets of Haiti today is the descendant of postcolonial occupation that was both supported by and supportive of suppressive anthropology there, it becomes impossible to evaluate it from a perspective of historical neutrality. Because anthropology played such a fundamental role in the historical subordination of Haiti, the U.S. ethnographer who works there carries the symbolic mark of the colonizer, a stigma perhaps imperceptible even to locals in the field. But the mark is nonetheless evident, as in the pervasive use of the Creole term *blan* to signify “foreigner,” regardless of racial phenotype.

The calculation of risk versus data extraction in dangerous fields is further hobbled by the assumption that the only relevant data in dangerous fields exist *embedded within* the violence, rather than *embodied by* the violence. Viewed from this perspective, cultural data are juxtaposed against the chaos of the field, the violence conceived as separate from the data that are to be culled from it. The ethnographer’s assessment of the risks involved in seeking out the data is a freighted construction, predicated not on the reality of violence as data but, rather, on a fiction of

violence as symptomatic of some social pathogen that is to be circumvented while maneuvering about the field. Here the violence is “treated as a . . . surface effect of [its] origin” rather than seen as a “condition of its own reproduction” (Feldman 1991:20).

It is admittedly difficult to conceive of violence in this way, as a reflexive reality, especially when one is embroiled in the peril. The very immediacy of violence in dangerous fields—concretized by gunfire, intimidation, corpses out of place, threat of arrest, blood in the street—distorts reality and can misinform, confuse, or paralyze ethnographic analysis through the creation of “feeble fictions in the guise of realism . . . flattening contradiction and systematizing chaos” (Taussig 1987:132). The result is an “epistemic murk” that extends the problem of ethnographic observation and representation beyond the merely philosophical—obscurity becomes a “high-powered medium of domination” (Taussig 1987:121). It is fairly easy here to be led astray—to be “seduced” by the actors in the violence. We may “trade our critical stance as observers for an illusion of congeniality” with the victims or even perpetrators of violence, our understanding of social and cultural phenomena subverted by a dissuasion of ethnographic inquiry beyond appearances (Robben 1995:85*). Here, rumor may be substituted for knowledge, and suspicion, for certainty. Now we are (mis)led into “stitch[ing] together what may well be correct facts but in so doing omit gaps, as if correlations can always eventually be linked by causal arrows, with the strength of detail then proving causality” (Simons 1995:50). By succumbing to the fictions constituted by the ambiguities of conflict, we fail to see the forest of cultural violence for the trees of its interpersonal consequences.

Amid the murk of the dangerous field, it becomes difficult for the ethnographer to locate the violence beyond the weapons and bodies used to accomplish the violent act itself. Nordstrom writes of this difficulty in a Mozambican context when she speaks of the concerns of a man whose testicles had been cut off by soldiers of Resistência Nacional Moçambicana.⁴ Because his concern was not with the overt violence perpetrated against him, or even with the wound itself, but, rather, with what the wound meant for his sexuality, we are left to wonder if the site of violence is his testicles, his sexual identity, his lineage, or somewhere else entirely (Nordstrom 1997:129–130). In the midst of violence, the very instruments of terror (rifle, machine gun, machete, rock, baton) and their targets (bodies) become distractions, diverting ethnographic attention away from the subtext of the violent act that *is* by focusing it on what the act *ought to be*, given the anthropologist’s construction of expected hazards that should correlate to the presence of these instruments. Anne Simons, speaking of her work in Mogadishu, illustrates just this point:

My own (ir)rationality at the time was that if this was really a significant confrontation, there would have to be a cataclysmic, massive shootout to prove it. Consequently, I

spent the whole day and that first night anticipating a sudden crescendo of gunfire. Instead, I heard only small-arms fire. Still, I was probably more anxious waiting for what did not occur than I was about the bullets that did occasionally zing over our roof. [1995:45]

Given the difficulties in ascertaining risk and the equally problematic task of deciding what should form the point of focus for the anthropologist in the dangerous field, it is not surprising that many of us find ourselves desperately searching for strategies that will acculturate our selves into a social milieu rife with thugs, informers, collaborators, spooks, jackals, death squads, traitors, and their anguished targets, to say nothing of the distractions of lies, rumors, silences, and seductions that surround everything we are investigating as ethnographers in dangerous fields. Feldman argues that a crucial component of this acculturative process is the anthropologist’s relinquishment of ownership over the “personal organs of perception that must be reinhabited, expanded, and intermingled to accommodate the material metaphors of a new sensorium” (1995:248) constituted by crisis and violence in the field. Raymond Lee argues for a reconfiguration of the senses as well, suggesting that anthropologists who work amid violent conflict “cope with ambient danger by developing a sensitivity to potentially hazardous situations” (1995:28), informed by an acquired knowledge and awareness of what constitutes danger in the context of a specific field. Frank Burton relates how his fieldwork in Northern Ireland necessitated a heightened awareness of his surroundings, with an acquired sense of wariness that even the mundane can be explosively lethal: “In Belfast one generally walks around parked uninhabited cars with suspicion, casts unnerving glances at unattended parcels, scrambles to get home before it is too dark, maps out safe and dangerous routes for journeys, all in an effort to evaluate risks which previously could be ignored” (1978:20). Acculturation to the dangerous field modifies other methodological approaches as well. A basic reality of Feldman’s research in Northern Ireland—pervasive surveillance—led him to abandon classical notions of participant-observation altogether, avoiding residence in the neighborhoods of his informants because to live among them might have been suggestive of complicity with the agents of surveillance, the police and the army. He took other methodological precautions as well—utilizing neutral spaces for interviews, avoiding long-term visual appropriation of any social milieu, restricting his mobility between adversarial spaces (only the police and army moved in such a manner), and demonstrating that there were things, places, and people that he did not want to know (1990*:12). Howell (1990) suggests that these kinds of awareness and sensitivity to field realities ought to be adopted as normative practice in dangerous fieldwork, and I agree. These adaptive strategies alter the way that the ethnographic environment is regarded: not as a static backdrop for data collection but as data themselves—a

brokering element of the type of information that is being sought and the tactics needed to get at it.

It is only through the lived witnessing that comes from submersion in the violence—whether it remains threatened or emerges in punctuated bursts—that we are able to experience the dangerous field in a meaningful way and write anthropology from and of it. But on a very practical level, this observational perspective can only be accomplished and maintained if the fieldworker is able to survive the violence. Work in dangerous fields implies an ability to negotiate daily a spectrum of social encounters with a diverse host of individuals, some of whom may be helpful, some of whom may be dangerous, and some of whom may be simply indifferent. The ability to “tell” who is who by reading the “frequently imaginary, microscopic signs” (Feldman 1995) is certainly an aspect of risky fieldwork that can only be hashed out in situ. But there are pragmatic tactics available to the ethnographer—methods that reflect the crucial insight that instability and violence are dimensions of life in dangerous fields and thus must be *negotiated* (not avoided, filtered out, or sifted through) with innovation and improvisation. Such insight is only gained over time, largely “by direct and intensive contact with complex field situations” that create “a profound understanding that transcends what can be gained at a distance or through formal methodologies” (Peritore 1990: 367).

NEGOTIATING DANGER IN PÓRTAIL LEOGÂNE

Since summer 1999, I had been working in a section of Port-au-Prince called Pórtail Leogâne. Haitians have taken to uneasily referring to the place, plagued by gangsterism, political gunplay, and drug terrorism, as “Kosovo.” Most of the street children in Pórtail Leogâne sniff a vaporous cobbler’s glue that can bring on a grievous zombification that inhibits better judgment and indulges desperate aggression.⁵ Some kids in Pórtail Leogâne tote razors that they have scavenged from trash piles, and many have vicious scars from fights with them. Though they keep the things primarily for grooming (shaving their heads, trimming their fingernails), the children who sniffed glue had a tendency to brandish them, often at me or my research assistant. These children are difficult and sometimes dangerous to approach, but the data that they provide to the anthropologist are invaluable insofar as these kids are clearly comported to the street in a manner profoundly different than that of other displaced children. My strategy for working with these kids was to enter Pórtail Leogâne with my research assistant *touni* (naked)—no camera, no recorder, no bags, often no notebook or pen. We were careful to scan the field discreetly for poorly concealed pistols, unsheathed machetes, paramilitary agents, and other potential dangers. We wore our shirts untucked at all times to give the impression that we could be armed ourselves. Because the older street boys in Pórtail Leogâne had a tendency to tightly (and at times menacingly) sur-

round us, we made every attempt to keep our backs to the wall, with the boys and the street in front of us. When not near a wall, we would stand facing one another, my research assistant watching over my shoulder, and I, over his. We would occasionally have to abort an interview with a street kid in Pórtail Leogâne he or others would grow impatient and threaten us. Other times, if the interview environment was growing palpably dangerous with the threat of gunplay or rock throwing in the surrounding neighborhood, we would retreat with a few children by taxi to Champ de Mars, a large, open, and usually safe plaza outside of Pórtail Leogâne near the National Palace. There we would complete the interviews.

These tactics for self-protection became inseparable aspects of my methodology and were incorporative of the field circumstances amid which the exchange of information was taking place. Rather than presupposing a safe location for an orderly, guided interview, the techniques used were informed by a minute-to-minute reassessment of what was going on around the neighborhood, and I adjusted my methods accordingly. At one moment, I could speak freely with informants and might even be able to take notes. But the next moment, in the midst of abrupt danger, we would have to *pa dan nou*—“shut our mouths,” hide the notes, and let the anxious, sweaty silence that now prevailed bespeak the volumes of data that hours of testimony could never provide. Because “silence can operate as a survival strategy” (Green 1995:118), it is not simply a symptom of fear but, rather, an aspect of cultural reality.

CHALLENGING THE ETHICAL CODES

Fieldwork that entails a recognizable degree of posturing in order to safeguard the anthropologist’s well-being may at times challenge the formal ethical guidelines laid down by our discipline. The current AAA “Code of Ethics” acknowledges that “the generation of anthropological knowledge is a dynamic process using many different and ever evolving approaches; and that for moral and practical reasons, the generation and utilization of knowledge should be achieved in an ethical manner” (AAA 2000a:1). The “Code of Ethics” also recognizes the relative autonomy of anthropologists in the field, who are pressed with making the necessary decisions for the preservation of the ethical integrity of their studies. But despite the fair amount of latitude for decision making implied by the code as a whole, its most basic assumptions about fieldwork ethics are challenged by the circumstances of the dangerous field, contributing to the moral confusion of ethnographers who engage daily in situations that the code is hopelessly unprepared to mediate. And whereas the “Code of Ethics” might be considered somewhat elastic, the “Statements on Ethics: Principles of Professional Responsibility” (adopted by the council of the AAA in 1971 and amended through 1986 [see 2000b]) offers the ethnographer in the field far less latitude. The principles

lay out bulleted points of ethics for anthropologists to consult in arbitrating their relations with informants and determining their responsibilities to the discipline and the public. Throughout my experiences in the field, I have found both the “Code of Ethics” and the “Principles of Professional Responsibility” to be at various times irrelevant, naive, or insufficient to guide my actions through the field conditions within which I was working.

The traditional methodologies of cultural anthropology are constructed under a rubric of overriding ethical concerns that generally situate the safety of the informant, and then the ethnographer-self, above any inquisitive imperative. The “Code of Ethics” clearly states that anthropologists have their primary obligations to the people they study and to the people with whom they work; the code also suggests that anthropologists ensure that their research does not harm the safety, dignity, or privacy of the people with whom they work or conduct research (AAA 2000a). The “Principles of Professional Responsibility” states the matter more forcefully: “Anthropologists must do everything in their power to protect the physical, social, and psychological welfare and to honor the dignity and privacy of those studied. . . . It is axiomatic that the rights, interests, and sensitivities of those studied must be safeguarded” (AAA 2000b:1). Herein is implied an intrinsic power relationship that conjures the colonial legacy of anthropology—one in which the anthropologist is assumed to be able to control or at least mediate or negotiate danger away from those with whom she or he is working. This is rarely, if ever, the case when working in dangerous fields. More often than not, the circumstances of such fields force a dramatic shift in power—one in which the anthropologist is more likely to rely on local knowledge and the protection extended by interlocutors or other locals in order to safeguard her or his welfare.

Nordstrom (1997) indicates in her narrative account of violence in Mozambique that her well-being was routinely safeguarded by her local associates. Peritore also realized the value of local protection in 1982, after finding himself in the midst of a violent riot in Managua:

I ended up lying behind a low garden fence while a mob chased by police jeeps stampeded down the narrow streets. The police began arresting journalists or persons with cameras, and I was saved from this only by a man who gave me a shopping bag and some bananas to conceal the camera. At the rendezvous point with my Nicaraguan journalist friend, the police were stripping his car and he motioned me away with his eyes. [1990:367]

Tobias Hecht, who conducted his research with street children in the northeast of Brazil in the early 1990s, puts the matter more bluntly: “I had the upper hand in terms of the creature comforts, but the research relationship was guided by a different dynamic. The problem of studying street children is that if you do it long enough, you come to realize that you depend on them, not they on you” (1998:8). In short, the ability to protect against harm or to offer aegis is not the exclusive domain of the anthropolo-

gist but, rather, must be regarded as power shared among the actors in the field toward the well-being of everyone concerned.

INFORMANT RESPONSIBILITY AND THE LOCALIZED ETHIC

By the very fact that we are participating in research that investigates, considers, or at least is engaged amid violence or terror or the threat thereof, we are inviting the possibility of victimization on ourselves and on our informants. If we have decided that such study is worthwhile and that we (the anthropologist and informants) are willing to accept a certain level of danger to participate in it, then we must also be prepared to accept a more humbly pragmatic role in our field relations. I suggest a reconfiguration of how we perceive our relationship with our informants. This relationship should be one of *mutual responsibility*—and not just for the validity of the data reported; all participants in the research must also willingly accept the possibility that *any* involvement in the study could result in intimidation, arrest, torture, disappearance, assassination, or a range of other, utterly unforeseeable dangers. The idea that the anthropologist is capable of anticipating the full array of possible repercussions of participation in the research, as suggested by the “Principles of Professional Responsibility,” is not only a colonial assumption but also revelatory of the lack of understanding of the circumstances involved in data collection in hostile environments. In the case of my own research, I quickly got the impression that my informants were better equipped than I to foresee the deadly consequences of participation in my study; and as such I relied on street children, my Haitian research assistant, and a host of other local associates in negotiating my own safety and the ethical issues of my study.

Rather than guide my fieldwork with hegemonic assumptions about uneven power relationships between ethnographer and informants, I applied a *localized ethic*—I took stock of the good advice and recommendations of the local population in deciding what conversations (and silences) were important, what information was too costly to life and limb to get to, the amount of exposure to violence considered acceptable, the questions that were dangerous to ask, and the patterns of behavior that were important to follow for the safety and security of myself and those around me. I preferenced the will and wishes of my informants (who were certainly better at anticipating danger than I was) over any arrogant presumptions as to what was supposed to be best for them. This was crucial to my negotiations on the streets of Port-au-Prince—it enabled me to set reasonable limits on my own inquiries but also facilitated the acquisition of otherwise inaccessible data.

The co-option of a localized ethic resurrects Bourgois’s (1990) concern that as we develop trusting relationships with our informants, gaining acceptance in their communities and thereby dissolving the barriers between insider

and outsider, we seek ultimately to encourage informants to forget who we are and what we do so that we may observe them engaged in undistorted social interactions. So, to secure truly informed consent, we would be obliged to interrupt controversial conversations and activities in order to remind our informants that they are under our observation. In most circumstances, I have found the anthropological instruments of consent themselves to be poorly adapted to fieldwork in states of conflict. Because their language and format generally derive from consent forms used in psychological and medical research, these documents could not possibly cover the full range of possible dangers implied by complicity in hazardous anthropological research. Fortunately, the "Code of Ethics" recognizes that "informed consent . . . does not necessarily imply or require a particular written or signed form" and that "it is the quality of consent, not the format, that is relevant" to ethical research design (AAA 2000a:3). Most university institutional review boards respect this digression and allow researchers to secure consent orally in fields where literacy is low or where legalist forms are unfamiliar. While working in Port-au-Prince with informants who could neither read nor sign their own names (often street children whose consent needed to be secured in loco parentis),⁶ I would spend a considerable amount of time talking with them about the range of foreseeable risks involved in participation, as well as the possible perils that could result from any association with me or my study. This strategy was also adopted by Tobias Hecht in his work with Brazilian street kids. He has indicated that the children "decided what to do [i.e., whether to participate in the research] in large part on the basis of past experience as well as on how they perceived me and what they made of my explanations. Some children chose not to participate" (personal communication, December 1, 2000).

Given the possibility of grave consequences for involvement by participants and our true inability to guarantee their safety or anticipate threats to them, there is at present some question as to whether research in dangerous fields should be engaged in at all. The *Handbook on Ethical Issues in Anthropology*, a special AAA online publication edited by Joan Cassell and Sue-Ellen Jacobs, highlights the ethical dilemma of anthropologist Daniel Peters, whose choice of research participants took on life and death significance entailing such responsibility that he "continues to wonder whether research interests warrant risks to local people" (2000). The handbook offers little discussion or guidance on how Peters's case (and those like it) should be handled, which is probably only appropriate, given the fact that any recommendation beyond those of the principals involved would be based on the arrogant assumption that anthropology has sole responsibility (read: capability) to determine what is best for the welfare of consenting informants.⁷ Those with whom we work in dangerous fields are entitled to negotiate equally and *exclusively* with ethnographers concerning matters of exposure to risk and research priorities. The acknow-

ledgment of this entitlement by anthropology is integral to the continuing project of decolonizing the discipline.

TRANSPARENCY AND DECEPTION

The "Code of Ethics" and "Principles of Professional Responsibility" coincide in their recommendation that the ethnographer maintain complete transparency in field relations, avoiding deception and thus suspicion of subversion through complete disclosure of the terms of the study. I have always abided by the spirit of this recommendation to the best of my abilities, but I take to task the common definitions of what it means to "deceive" in dangerous fields. There are instances when I found it necessary to misrepresent myself, not to conduct the research clandestinely but, rather, to protect or safeguard my own well-being. I do not believe that it is always necessary, practical, or even prudent to accurately represent oneself as an anthropologist to absolutely all people encountered in the field. I lied about who I am a number of times to various individuals over the course of my fieldwork in Haiti, usually when a complicated or confusing description of what an anthropologist does could have resulted in me getting arrested, shot, or worse.⁸ At other times I found it necessary to give the impression that I might be concealing a firearm (an instrument that some ethnographers have found a need for, though I have not), especially in situations in which not having a weapon not only put me in the minority but also made me very vulnerable. Though it may rub with aggravation against accepted ethical norms, if giving the *impression* that I was armed facilitated a greater chance of surviving the consequences of exchanging sensitive information being willingly supplied by consenting informants in dangerous situations, then I see little concern in doing so. Such misrepresentation not only permitted me the necessary posture for protecting myself in the field but also allowed me to engage informants who would otherwise have been neglected and to move through social milieus that beg for anthropological attention but were otherwise far too threatening to enter.

DATA COLLECTION, DATA PROTECTION

In addition to the challenges it poses to the standing ethical guidelines of our discipline, the dangerous field might also inform the techniques used in recording data. I found that certain traditional data-collection techniques were not feasible on the streets of Port-au-Prince. The old adage that "if its not written down, it didn't happen" has held little relevance for my research, when keeping detailed notes of my interviews could often have jeopardized the lives of myself and my informants. Field notes, however codified or locked in electronic documentation, contain sensitive information that could be used against the anthropologist (they could, for example, be suggestive of espionage or indicate association with state or social "enemies") or informants (they might contain evidence of illegal/prosecutable/sanctionable acts by the informants).

Most anthropologists who work in dangerous fields are well aware of the sensitivity, vulnerability, and potentially malign uses of their field notes. Christine Obbo writes, "During my fieldwork in Uganda, my field notes on the illicit income-generating activities in Wabigalo-Namuwongo would have been a useful tool in the hands of many government agents and bureaucrats to justify their harassment of informal-sector operatives" (1990:301).

As important as it is to have notes, even the best efforts to protect the identities of the informants who relate sensitive data therein are fallible. Margery Wolf's experiences in the People's Republic of China illustrates just this point: "The research in the PRC made me painfully aware of my own mortality and of the sensitive nature of field notes. . . . [I]n the original field notes for [one] village, even though we assigned everyone an identification number, the names of the individuals we knew best had a way of slipping in" (1990:352). If we are to take Peritore's (1990) advice and assume the worst when considering the broader implications of ethnographic intervention, we must always assume that the notes taken in the dangerous field will be confiscated and deciphered and that even our best intentions to encrypt the names of our informants might be compromised by the unnoticed lapses in attention to such matters that regularly emerge over the course of the daily grind of fieldwork. In other words, the encryption of notes taken in the dangerous field should always be assumed imperfect. We must remind ourselves daily that some of the things that we jot down can mean harassment, imprisonment, exile, torture, or death for our informants or for ourselves and take our notes accordingly.

It is for these reasons that while working in Port-au-Prince in the 1990s, I relied at times on meticulous memorization of the details of entire conversations in order to defer the risk of having written evidence that the dialogue had taken place at all. At other times I would retreat to alleys, toilet stalls, or dark areas of cafes to surreptitiously scribble jottings onto scraps of notepaper that I kept in my boot—a method dictated by what I considered pragmatic for my own safety and that of my informants.⁹ It is always desirable to have written notes of interviews or testimonials, but where these documents could bring down the force of violence against ourselves or our research participants, we need to defer entirely or modify our methods of taking them in favor of safeguarding against violent reprisals. Richard Jenkins (1984) suggests that the recording of sensitive information in the field should be a selective process, forgoing the documentation of some information altogether in favor of keeping such data only in one's memory. He also recommends that one only carry around the current day's notes and keep in the field no more than a few weeks' worth of notes, kept under lock and key until they can be sent home.

The assumption that audio or video recording is the ideal medium for data inscription in all ethnographic field research is also problematic. Often the very instrumental nature of the medium discounts it as a safe tool to use in

dangerous fields. The text inscribed upon audio- or videocassettes differs from written documentation in a fundamental way. Sensitive though they may be, written field notes are once removed from the informants who provided the information on which they are based—that is, the informants' testimonies are filtered into field notes by the ethnographer who transforms subject statements into interpreted jottings. But whereas written data may be at least partially codified or modified by the anthropologist's interpretation (and thus somewhat obscured from prying eyes), voice or image recording is difficult if not impossible to obscure in the field. And in reality it represents testimonial fact that can be replayed without a need to negotiate mediated meanings, for the data are not secondhand (that is, passed through the anthropologist's rendering, as in the case of written notes) but, rather, are testimonies recorded in the first person. Written notes isolate an informant's testimony within a specific space and time in the past—a written note always follows an informant's utterance and cannot therefore precede it; as such, the verbal statement exists in the perpetual past. But, conversely, a recorded conversation may be replayed again and again, in different times and spaces for different audiences in the future. The dialogue exists now in a perpetual present and can be damaging, incriminating, or fatal to the informant and the anthropologist, whose very voices or images are implicated in the recording.

None of this is to say that I abandoned altogether the use of formalized written field notes or recorded interviews in the course of my fieldwork. Nor do I advocate their abandonment by others. But I did regularly refrain from jotting down noteworthy social descriptions that would surely have been routinely documented under "normal" circumstances. I took pains to ensure that the notes and recordings that I did take were hidden in a secure place while they were in my field residence and that they were meticulously codified, protected, and irregularly and unpredictably sent out of Haiti. The prudence of this tactic was evident when it became clear that I was under the surveillance of state authorities, who expressed a threatening interest in the notes that I was taking.¹⁰ The questions that I was asking street children—where they slept, with whom they associated, what weapons they carried, and so on—were questions that the Anti-Gang Unit would have liked to know answers to as well. The notes and cassettes that I held in my possession at that time would certainly have been evidence enough for it to arrest most of my child informants. It also could have implicated me (falsely) as a spy, insofar as the data contained therein could easily have been construed as (or revealed to be) damaging to the Haitian state. I was sufficiently unnerved by all of this the night before I left Haiti that I considered destroying the tapes and the notes that I still had before departing, though I did not—but only because my professional despair over burning them overcame my better judgment to actually do so.

VIOLENCE, TERROR, AND FEAR IN THE FIRST PERSON

Robert M. Emerson, Rachel I. Fretz, and Linda L. Shaw (1995) have argued that there is a fundamental inseparability of research methods from research findings. They point out that “*what* the ethnographer finds out is inherently connected with *how* she finds it out,” and as such it is “critical for the ethnographer to document her own activities, circumstances, and emotional responses as these factors shape the process of observing and recording others’ lives” (1995:11). With some eloquent exceptions (Bourgois 1990; Daniel 1996; Nordstrom 1997; Sluka 1990, 1995), a recurrent problem with ethnographies of dangerous fields is the failure of anthropologists to consider their own role as actors in the drama of violence playing out before them.

The likelihood of being shot, either as a target or as a collateral casualty, is significantly escalated in certain dangerous fields and surely was an everyday concern during my work in Haiti. My physical proximity to gunplay in the streets, combined with the persistent anxiety that I felt as a result of those occasions when I was almost hit myself, prompted me to take careful stock of how my fear of dying violently in the field was impacting my work. The bullet fired, the rock thrown, and the machete swung are all data insofar as they play a role in the construction of a culture of violence; likewise, the target for which any of the above is intended is also relevant to the data, whether that target is a local or the anthropologist. The researcher’s reactions to, fears of, and anxieties toward in-field violence have a place in ethnography that needs to be clearly defined, especially when critical incidents in the field severely obscure what the role of the ethnographer should be.

Just as the sexual involvements of anthropologists in the field are increasingly regarded as relevant to the interpretation of some field data (Altork 1995; Bolton 1995, 1996; Gearing 1995; Rabinow 1977), so too should the ethnographer’s intimate feelings and responses to violence and terror be included in the data set when such information might shed light on the circumstances under which that information was acquired and how those circumstances modified the social relations amid which it was obtained. As ethnographers we must divorce ourselves from our historical assumption of objective immunity from the interpersonal implications of field relations and in turn embrace a mutual responsibility between anthropologist and informant. In this way, ethnography becomes a reflexive dialogue of the kind described by Cynthia Keppley Mahmood when she writes,

I see the reflexivity of contemporary ethnography as an honesty about the limitations of our vision that has too long been suppressed in the interests of creating a false aura of authority about our work. Relinquishing that authority is not easy, and it is particularly not easy to share authority with one’s interlocutors [i.e., informants] without denying one’s own ultimate responsibility for a text. [1996:235]

The dangerous field imposes an even greater limitation on our vision as it reminds us that as we observe we participate; and when we do so in the midst of violence, we become part and parcel of it. It is a humbling professional experience, one that obfuscates our critical lens long after we have left the field. That obfuscation leaves us in the dilemma best expressed by Ruth Behar, who writes that

in the midst of a massacre, in the face of torture, in the eye of a hurricane, in the aftermath of an earthquake, or even, say, when horror looms apparently more gently in memories that won’t recede and so come pouring forth in the late night quiet of a kitchen. . . . [D]o you the observer, stay behind the lens of the camera, switch on the tape recorder, keep pen in hand? Are there limits—of respect, piety, pathos—that should not be crossed, even to leave a record? But if you can’t stop the horror, shouldn’t you at least document it? [1996:2]

The value of documenting horrors moves beyond journalistic and humanitarian motivations. Ethnographic fieldwork is the primary means for the gathering of raw data for cultural anthropologists grappling with issues of violence and terror. But the significance of a pragmatic methodology for negotiating dangerous fields is far greater than its mere value as a facilitator of data collection. Among the most pressing problems for the ethnographer of violence is the predominance of anthropological theories that continue to hold to the presumption that cultural facts can be culled and separated from social context, and this is simply counterproductive to the task of ethnographic fieldwork. If we accept that an archaeologist’s knowledge of an artifact’s significance is dependent in large part on where that artifact was in relation to other pieces in the ground, why should we not recognize context dependence when theorizing the nature of the data we seek to “extract” from the social field? Anthropological theory needs to explicitly acknowledge the truism (well known to seasoned ethnographers of dangerous fields) that cultural traits do not exist in compartmentalized units that are separable from one another, and so where violence *contaminates* social relations, there is no possibility of collecting data that can be considered “pure.” Violence *is* culture, and we need to continue the ethnographic task of demystifying its nature if we are to theorize it alongside other social phenomena. Herein lies an opportunity for the ethnography of violence to significantly influence the course of anthropological theory—by repeatedly demonstrating that violence is not separable from kinship, or market activities, or language, or any other social relations that from a distance may not appear to be modified by it. Here we see the importance of considering the fears and anxieties of the anthropologist on the ground; as a functioning agent in the local culture of violence (that is, as a *subjective* rather than an *objective* agent), the ethnographer is obligated to demonstrate how the pervasion of violence modifies her or his own field relations and how similar modifications extend to those ordinary relations of the local community as well.

THE COMPULSION TO ACT IN THE DANGEROUS FIELD

Amid the horrors of the dangerous field, some anthropologists have carried the idea of methodological and ethical reorientation to a level of radical transformation. Nancy Scheper-Hughes exemplifies this position in an article she published in *Current Anthropology* in 1995, calling for the formation of a “militant anthropology”:

The new cadre of “barefoot anthropologists” that I envision must become alarmists and shock-troopers—the producers of politically complicated and morally demanding texts and images capable of sinking through the layers of acceptance, complicity, and bad faith that allow the suffering and death to continue without even [a] pained cry of recognition. [1995:417]

John Gledhill (1999) has contextualized Scheper-Hughes’s much maligned statement by clarifying that her fundamental point is that anthropologists in morally challenging field circumstances need to stand prepared to act and speak on behalf of *something* if they are to act ethically at all. In other words, the ethnographer’s obligation to engage the field as a moral subject is not suspended simply because one’s role has traditionally been defined as objective. This much is indisputable. As a moral agent working with destitute street children, I often acted outside the customary bounds of the objective anthropologist. I treated and dressed their wounds; I took razor blades from the hands of the aggressive and angry among them; I gave them clothes, money, and food when I could. I spoke on their behalf at the juvenile prison, helped to extricate them from hostile territories, and associated with members of political organizations that advocate for street children. The decision to do these things had nothing to do with being a militant anthropologist. It was simply an extension of a localized ethic that demanded it.

Scheper-Hughes’s pleas for a “barefoot cadre” are something else entirely, for she does not seem to cry out for individual moral accountability as much as she calls out for political activism as an *integral part of doing anthropology* amid horror, and that is the crucial distinction. In her *Death without Weeping: The Violence of Everyday Life in Brazil*, Scheper-Hughes argues that “what may never be compromised are our personal accountability and answerability to the other”; but she elevates this idea of individual moral accountability to the disciplinary level when she describes anthropology both as “a field of knowledge (a *disciplinary* field) and as a field of action (a *force* field). Anthropology can be a site of resistance” (1992:24–25). Although I agree wholeheartedly with the truism that anthropologists do not lose their moral accountability or responsibility while on the job, the assumption that we should use the discipline specifically as a platform for political partisanship is troubling to me. There must be a clear line of distinction that separates anthropology from activism if anthropology is going to continue to be taken seriously as a social science and thus a source of cultural data usable in other sciences and disciplines.

Although my work with democratization and political violence in Haiti clearly placed me in situations in which I had opportunities to use my dual identity as an American and an academic to advance social justice initiatives, I opted not to. My reasons were really threefold. First, to act on behalf of some social issue is not why I was in Haiti in the first place, though Scheper-Hughes might argue that it should have been. I was not there as an activist struggling to change the political system, I was there as an ethnographer to document the impact of violence on street children, and that is all. Second, to act as an anthropologist means to act with responsibility not only toward my associates and interlocutors in the field but to the discipline itself as well. I see nothing wrong with anthropologists acting on behalf of political agendas (indeed, I do myself), so long as they are acting as individual activists and not representing that activism as anthropological doctrine.

My final reason is one that emerged over time from my experiences working in Haiti. When I first arrived in 1994, I was a supporter of the restored president, Jean-Bertrand Aristide. He was democratically elected, he was from the slums, he ran on a platform of transparency and social justice, he was an advocate for the poor, and he was overthrown in a bloody coup d’état in 1991 and restored by U.S. invasion the year I arrived. Moreover, the first four years of my fieldwork were focused on the Lafanmi Selavi orphanage that Aristide initiated when he was a Catholic priest in 1986. I became personally acquainted with Aristide and his family over the years of my fieldwork and was firmly convinced that he was a good leader and worthy of my support. But that was before he began to amass fabulous wealth in a country where over 75 percent live in abject poverty. And that was before he ordered tear gas into the orphanage he started, in order to suppress an uprising of youths who were protesting what I witnessed myself to be living conditions of filth and scarcity. And that was before he began to assemble a foreboding power structure around his presidency, not unlike that of his junta predecessors. So where should the discipline have stood on the issue of Aristide in 1994? Where should it stand on the issue of Aristide in 2002? The cadre of barefoot anthropologists might suggest that I stand on the side of the down-trodden, but experience has taught me that the people are fickle—they, like me, supported him in 1994; and they, like me, are not sure what to do now.

I did not use anthropology to advocate social justice issues in Haiti because I humbly accepted that I am in no position to speak with certainty on behalf of the discipline on such unpredictable matters of politics. Where should the discipline stand on this issue? It should stand out of the way so that we can get back to doing ethnography. There is an inherent danger in intentionally (and with claims of disciplinary certainty) advancing *any* political agenda on behalf of anthropology. To expand on the sentiments expressed by Rorty (1982), the conscious advocacy of a moral position in a particular field instance is praiseworthy, but it is unlikely that there is something universal

and timeless about that given particular moral position that makes its advocacy worthy of a universal and timeless ethical position for the discipline in toto.

I do not want to be an “alarmist” or a “shock trooper” because they are never taken very seriously, because I do not see the adoption of such a stance as necessary in order to act as a moral individual while doing anthropology, because I am distrustful of those who are so certain of their answers that they have few questions, and because I firmly believe that anthropology can contribute to social reform without being “militant.” If ethnographic data are used to effect social change, that is one thing. But if ethnography itself is nothing more than a methodology for political advocacy, then why should we trust the data it puts forth any more than we trust the heavily biased data produced by partisan policy research institutes (like those affiliated with the tobacco industry) that seek to advance platforms instead of build knowledge?

CONCLUSION

As the body of research on the anthropology of violence and terror grows, so too grows the frequency with which anthropologists are thrust or are thrusting themselves into dangerous fields. Although the tried and true methods of cultural fieldwork must continue to be used, they must be responsive to the dynamic and oftentimes hazardous conditions amid which the data must be gathered. Ethnographers working in dangerous fields are pressed with the challenge of innovating new strategies for the preservation of their well-being while at the same time continuing to identify and explain the unique social interrelations that arise amid crisis and strife. The pressures of peril placed on anthropologists in dangerous fields demand that we reassess our relationship to the field and to our informants. We need not abandon wholesale the discipline’s ethical codes, but we might well consider the adoption of a more localized ethic if it can decrease our exposure to danger while on the job, even if such an adoption at times runs counter to the established ethics of traditional anthropology. As long as we continue to value new data from new social fields, and as long as we are willing to chance a certain degree of risk to acquire them, anthropology will need to adapt and accommodate as it always has.

J. CHRISTOPHER KOVATS-BERNAT *Department of Sociology and Anthropology, Muhlenberg College, 2400 Chew Street, Allentown, PA 18104*

NOTES

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power relationships among anthropologist, colleague, and informant but also guaranteed that I would live to write this. I thank Jeff Sluka first for his research and second for encouraging me to write about the practical matters of negotiating safety in dangerous fieldwork. I also thank Tobias Hecht, Peter Peritore, and Cynthia Keppley Mahmood for their thoughts, advice, and suggestions. I sincerely appreciate the thoughtful and challenging work of the anonymous reviewers of this article, whose impact on the final text has been more than substantial. I owe a genuine debt of gratitude to F. Niyi Akinnaso at Temple University and Tom Patterson at the University of California (Riverside) for teaching me the most valuable lesson of research in dangerous fields—when to terminate the research and evacuate for my own safety. Finally, I thank Dina, who steadfastly dealt with the terror of the dangerous field from a perspective that I will never know.

1. For a thorough analysis of my research with street children and political violence in Haiti, I refer the reader to my doctoral dissertation (Kovats-Bernat 2001). I have published elsewhere (Bernat 1999) some of my research findings concerning the specific cultural uses of state terror in Haiti.

2. On the night of January 11, 2000, a French businessman, his 18-year-old daughter and, a Haitian companion were killed in Jacmel, a quiet resort area in the south of Haiti. Four days later, an American tourist was assassinated by a gunshot to the face when she refused to surrender her car to bandits on the outskirts of Port-au-Prince. These casualties were in addition to several Haitians shot to death in the capital in the same month, some in broad daylight. One Haitian newspaper has referred to the increase in violence against foreigners as “a new precedent”; foreigners, especially Americans, have been historically excepted from targeting for violence in Haiti by virtue of their citizenship—until now.

3. The Anti-Gang Unit is chiefly responsible for intelligence and interdiction in combating street violence in Haiti. Port-au-Prince’s street-child population is especially targeted for investigation (and often beatings and arrest) by agents of Anti-Gang, who regard them as largely responsible for the escalation in disorder on the streets of the capital. For a detailed discussion of the state’s association of street children with urban mayhem in Haiti, see my recent treatment of these matters in *Peace Review’s* theme issue on children and war (Kovats-Bernat 2000).

4. Resistência Nacional Moçambican (RENAMO) is a rebel group that was instigated by Rhodesia and later South Africa throughout the 1980s Mozambican civil war. Destabilization of the existing regime was a primary motivation for the formation of the group, and its dirty war tactics gained RENAMO infamy in international human rights circles (Nordstrom 1993).

5. I use the term *zombification* intentionally because a child who sniffs glue becomes by definition “zombie”—an individual whose flesh is animated but who lacks that aspect of the soul (the *ti bon anj* or “little good angel”) that determines her or his individuality, character, will power, and self-control. Even a cursory encounter with a child high on glue is enough to convince one that zombification has taken place.

6. I paid the kids with whom I worked for their participation in interviews, which raises another whole spectrum of ethical concerns. Sometimes the raw economic exchange of the transaction evoked a kind of prostitution. I often wanted to give some children more than others, figuring after a while that a particularly painful testimony merited a measure more than a less moving one. At times, in the midst of poverty, enmeshed in violence, among dying children, frightened and saddened, I would define Port-au-Prince for what it seemed to be at those moments—brutal, despairing, surreal. To situate myself, to give myself significance in that world, I sometimes indulged that fiction that the dangerous field evokes—that I am separate from all of this, maybe even in a position of power over it. I never really got over the compulsion to tell the children whom I had just paid to spend the money on food, for example. Eventually, though, I localized even my paternalism—I would just caution them to stick the money down their pants, so that the older boys would not beat them up for it. I think it is only by witnessing and immersing oneself in these kinds of circumstances that one can realize that no two field approaches can ever be so similar as to be adjudicated by a single, rigid ethical code. It is to realize what Hecht (2000) means when he writes of the “violent

indifference" of observing the daily existence of children who are dying right before one's eyes.

7. Cynthia Keppley Mahmood (1996) cites the theologian Martin Buber (1965) in explaining the deficiencies of current anthropological conceptions of ethnographer-informant relations by eloquently illustrating the inadequacies of our terminology in defining the uniquely problematic moral relationships among actors in the field. Existentialist philosophy has given us some very relevant discussions concerning the nature of ethical negotiations that could be put to good use in mediating our relations with informants in the field. Among the most significant in defining the social nature of ethical relations are those contributions by Emmanuel Levinas (1969, 1993, 1998), who envisions an ethics that derives from the social level of person-to-person contact. From this perspective, each moral "ought" derives from the particulars of moral conflict unique to a given social scenario. As such, he concludes that there can be no universal standard of moral judgment that is applicable in all social cases. Although we need not hold to such pure relativism as dogma, we would do well to recognize the arrogance implied by the current assumption that anthropology has hegemonic moral authority to decide what the nature of moral relations will be in the field. While not advocating a social prerequisite for ethics, Richard Rorty (1982) argues for a similar context dependence for ethics when discussing the nature of pragmatism. He writes that pragmatists

see certain acts as good ones to perform, under the circumstances, but doubt that there is anything general and useful to say about what makes them all good. The assertion of a given sentence—or the adoption of a disposition to assert the sentence, the conscious acquisition of a belief—is a justifiable, praiseworthy act in certain circumstances. But, *a fortiori*, it is not likely that there is something general and useful to be said about what makes all such actions good—about the common feature of all the sentences which one should acquire a disposition to assert. [1982:xiii]

8. I have on separate occasions identified myself as a journalist (when pressed against a wall by a youth wielding a machete during a street demonstration) and as a mission worker (when stopped at a remote roadblock outside of Port-au-Prince by un-uniformed armed men). I have at other times allowed certain individuals to assume (without encouraging them) that I worked for the U.S. embassy, a development agency, or a nongovernmental organization in some capacity. These individuals had nothing to do with my research, and I found it best to allow such people to believe me to be whatever it was they wanted. I never lied or misrepresented myself to any of my informants or confidantes and would often spend hours trying to articulate the concept of "anthropologist" to street children who did not initially grasp the idea.

9. Despite these unorthodox techniques for data recording, I never took such notes without the knowledge of the informants. I secreted myself away from public view to scribble the jottings so as to avoid attracting too much attention to the fact that my informants were providing me with information valuable enough to write down. It was as much for the security of the others involved in the data transaction as it was for my own protection.

10. In a conversation that I had with an agent of the Anti-Gang Unit, probing questions were posed to me regarding the notes that I was taking while interviewing street children under suspicion for illegal activity. I flatly refused to divulge any information to this agent concerning my field notes. Later that afternoon, after having been followed and observed by them for the better part of a day, I confronted two men who were eavesdropping on my interviews with street kids. They were Anti-Gang. They questioned me about my notes, suggested that my work was subversive and that they knew where I was staying and with whom I associated, and made veiled threats to my security. After deliberating the field situation for several days, and after talking at length with my field assistant, street children, and colleagues back at my department at Temple University, I decided to assume the worst and leave Haiti. My Haitian research assistant left for Florida a few weeks later.

REFERENCES CITED

- Adler, Patricia A.
1993 *Wheeling and Dealing: An Ethnography of an Upper-Level Drug Dealing and Smuggling Community*. New York: Columbia University Press.
- Altork, Kate
1995 *Walking the Fire Line: The Erotic Dimension of the Fieldwork Experience*. In *Taboo: Sex, Identity, and Erotic Subjectivity in Anthropological Fieldwork*. Don Kulick and Margaret Willson, eds. Pp. 107–139. London: Routledge.
- American Anthropological Association
2000a Code of Ethics of the American Anthropological Association. Electronic document, <http://www.ameranthassn.org/committees/ethics/ethcode.htm>.*
2000b Statements on Ethics: Principles of Professional Responsibility. Electronic document, <http://www.ameranthassn.org/stmts/ethstmnt.htm>.
- Aretxaga, Begona
2000 *A Fictional Reality: Paramilitary Death Squads and the Construction of State Terror in Spain*. In *Death Squad: The Anthropology of State Terror*. Jeffrey A. Sluka, ed. Pp. 46–69. Philadelphia: University of Pennsylvania Press.
- Asad, Talal
1973 Introduction. In *Anthropology and the Colonial Encounter*. Talal Asad, ed. Pp. 9–19. Atlantic Heights, NJ: Humanities Press.
- Behar, Ruth
1996 *The Vulnerable Observer: Anthropology That Breaks Your Heart*. Boston: Beacon Press.
- Bernard, H. Russell, ed.
1998 *Handbook of Methods in Cultural Anthropology*. Walnut Creek, CA: AltaMira Press.
- Bernat, J. Christopher
1999 *Children and the Politics of Violence in Haitian Context: Statist Violence, Scarcity, and Street Child Agency in Port-au-Prince*. *Critique of Anthropology* 19(2):121–138.
- Besteman, Catherine
1999 *Unraveling Somalia: Race, Violence, and the Legacy of Slavery*. Philadelphia: University of Pennsylvania Press.
- Bolton, Ralph
1995 *Tricks, Friends and Lovers: Erotic Encounters in the Field*. In *Taboo: Sex, Identity, and Erotic Subjectivity in Anthropological Fieldwork*. Don Kulick and Margaret Willson, eds. Pp. 107–139. London: Routledge.
1996 *Coming Home: Confessions of a Gay Ethnographer in the Years of the Plague*. In *Out in the Field: Reflections of Lesbian and Gay Anthropologists*. Ellen Lewin and William Leap, eds. Pp. 147–170. Urbana: University of Illinois Press.
- Bourgois, Phillipe
1990 *Confronting Anthropological Ethics: Ethnographic Lessons from Central America*. *Journal of Peace Research* 27(1):43–54.
1995a *Families and Children in Pain in the U.S. Inner City*. In *Small Wars: The Cultural Politics of Childhood*. Nancy Scheper-Hughes and Carolyn Sargent, eds. Pp. 331–351. Berkeley: University of California Press.
1995b *In Search of Respect: Selling Crack in El Barrio*. New York: Cambridge University Press.
- Buber, Martin
1965 *Between Man and Man*. New York: Macmillan.
- Burton, Frank
1978 *The Politics of Legitimacy: Struggle in a Belfast Community*. London: Routledge and Kegan Paul.
- Cassell, Joan, and Sue-Ellen Jacobs, eds.
2000 *Handbook on Ethical Issues in Anthropology*. Electronic document, <http://www.ameranthassn.org/sp23.htm>.
- Chagnon, Napoleon
1997 *Yanomamö*. Fort Worth, TX: Harcourt Brace College Publishers.
- Craige, John H.
1933 *Black Bagdad*. New York: Minton, Balch.
- Daniel, E. Valentine
1996 *Charred Lullabies: Chapters in an Anthropology of Violence*. Princeton, NJ: Princeton University Press.
- Dentan, Robert Knox
2000 *Trying to Tell the Truth about Violence: Some Difficulties*. *The HFG Review* 4(1). Electronic document, <http://www.hfg.org/html/pages/mag4/toc.htm>.

- Emerson, Robert M., Rachel I. Fretz, and Linda L. Shaw
1995 *Writing Ethnographic Fieldnotes*. Chicago: University of Chicago Press.
- Falla, Ricardo
1994 *Massacres in the Jungle: Ixcán, Guatemala, 1975–1982*. Boulder, CO: Westview Press.
- Feldman, Allen
1991 *Formations of Violence*. Chicago: University of Chicago Press.
1995 *Ethnographic States of Emergency*. In *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 224–252. Berkeley: University of California Press.
- Ferguson, R. Brian
1995 *Yanomami Warfare: A Political History*. Santa Fe, NM: SAR Press.
- Ferguson, R. Brian, and Neil L. Whitehead, eds.
1992 *War in the Tribal Zone*. Santa Fe, NM: SAR Press.
- Gearing, Jean
1995 *Fear and Loving in the West Indies: Research from the Heart*. In *Taboo: Sex, Identity, and Erotic Subjectivity in Anthropological Fieldwork*. Don Kulick and Margaret Willson, eds. Pp. 107–139. London: Routledge.
- Giddens, Anthony
1987 *A Contemporary Critique of Historical Marxism, vol. 2: The Nation-State and Violence*. Berkeley: University of California Press.
- Gledhill, John
1994 *Power and Its Disguises: Anthropological Perspectives on Politics*. London: Pluto Press.
1999 *Moral Ambiguities and Competing Claims to Justice*. Electronic document, <http://les1.man.ac.uk/sa/jg/Moral%20Ambiguities.pdf>.
- Green, Linda
1995 *Living in a State of Fear*. In *Fieldwork under Fire: Contemporary Studies in Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 105–127. Berkeley: University of California Press.
- Hale, Charles
1996 *Resistance and Contradiction: Miskitu Indians and the Nicaraguan State, 1894–1987*. Stanford, CA: Stanford University Press.
- Hecht, Tobias
1998 *At Home in the Street: Street Children of Northeast Brazil*. Cambridge: Cambridge University Press.
2000 *Street Ethnography: Some Notes on Studying and Being Studied*. *Focaal: Tijdschrift Voor Antropologie* 36.*
- Howell, Nancy
1990 *Surviving Fieldwork: A Report of the Advisory Panel on Health and Safety in Fieldwork*. Special Publication, 26. Washington, DC: American Anthropological Association.
- Jenkins, Richard
1984 *Bringing It All Back Home: An Anthropologist in Belfast*. In *Social Researching: Politics, Problems, Practice*. Colin Bell and Helen Roberts, eds. Pp. 147–163. London: Routledge and Kegan Paul.
- Keiser, R. Lincoln
1979 *The Vice Lords: Warriors of the Street*. New York: Holt, Rinehart and Winston.
1991 *Friend by Day, Enemy by Night*. New York: Holt, Rinehart and Winston.
- Kovats-Bernat, J. Christopher
2000 *Anti-Gang, Arimaj, and the War on Street Children*. *Peace Review* 12(3):415–421.
2001 *The Impact of Poverty, Violence and State Repression on the Cultural Identity and Social Agency of Street Children in Port-au-Prince, Haiti*. Ph.D. dissertation, Temple University.
- Kuser, J. Dryden
1921 *Haiti, Its Dawn of Progress after Years in a Night of Revolution*. Boston: R. G. Badger.
- Lee, Raymond M.
1995 *Dangerous Fieldwork*. Thousand Oaks, CA: Sage Publications.
- Levinas, Emmanuel
1969 *Totality and Infinity*. Pittsburgh: Duquesne University Press.
1993 *Outside the Subject*. Stanford, CA: Stanford University Press.
- 1998 *Otherwise Than Being: Or Beyond Essence*. Pittsburgh: Duquesne University Press.
- Mahmood, Cynthia Keppley
1996 *Fighting for Faith and Nation*. Philadelphia: University of Pennsylvania Press.
- Malkki, Lisa* H.
1995 *Purity and Exile*. Chicago: University of Chicago Press.
- Nagengast, Carole
1994 *Violence, Terror, and the Crisis of the State*. *Annual Review of Anthropology* 23:109–136.
- Nash, June
1976 *Ethnology in a Revolutionary Setting*. In *Ethics and Anthropology: Dilemmas in Fieldwork*. Michael Rynkiewicz and James Spradley, eds. Pp. 148–166. New York: Wiley and Sons.
- Nordstrom, Carolyn
1993 *Treating the Wounds of War*. *Cultural Survival Quarterly (Summer)*:28–30.
1997 *A Different Kind of War Story*. Philadelphia: University of Pennsylvania Press.
- Nordstrom, Carolyn, and JoAnn Martin
1992a *The Culture of Conflict: Field Reality and Theory*. In *The Paths to Domination, Resistance, and Terror*. Carolyn Nordstrom and JoAnn Martin, eds. Pp. 3–17. Berkeley: University of California Press.
1992b *The Paths to Domination, Resistance, and Terror*. Berkeley: University of California Press.
- Nordstrom, Carolyn, and Antonius C. G. M. Robben, eds.
1995 *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Berkeley: University of California Press.
- Obbo, Christine
1990 *Adventures with Fieldnotes*. In *Fieldnotes: The Makings of Anthropology*. Roger Sanjek, ed. Pp. 290–302. Ithaca, NY: Cornell University Press.
- Olujić, Maria B.
1995 *The Croatian War Experience*. In *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 186–204. Berkeley: University of California Press.
1998 *Children in Extremely Difficult Circumstances: War and Its Aftermath in Croatia*. In *Small Wars: The Cultural Politics of Childhood*. Nancy Scheper-Hughes and Carolyn Sargent, eds. Pp. 318–330. Berkeley: University of California Press.
- Peritore, Patrick N.
1990 *Reflections on Dangerous Fieldwork*. *American Sociologist* 21(4):359–373.
- Rabinow, Paul
1977 *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.
- Riches, David, ed.
1986 *The Anthropology of Violence*. Oxford: Basil Blackwell.
- Robarchek, Clayton, and Carole Robarchek
1998 *Worani: The Contexts of Violence and War*. Fort Worth, TX: Harcourt Brace.
- Robben, Antonius C. G. M.
1995 *The Politics of Truth and Emotion among Victims and Perpetrators of Violence*. In *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 186–204. Berkeley: University of California Press.
1996 *Ethnographic Seduction, Transference, and Resistance in Dialogues about Terror and Violence in Argentina*. *Ethos* 24(1):71–106.
- Robben, Antonius C. G. M., and Carolyn Nordstrom
1995 *The Anthropology and Ethnography of Violence and Sociopolitical Conflict*. In *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 1–23. Berkeley: University of California Press.
- Rorty, Richard
1982 *Consequences of Pragmatism*. Minneapolis: University of Minnesota Press.
- Scheper-Hughes, Nancy
1992 *Death without Weeping: The Violence of Everyday Life in Brazil*. Berkeley: University of California Press.

- 1995 The Primacy of the Ethical: Propositions for a Militant Anthropology. *Current Anthropology* 36(3):409–440.
- Seabrook, William B.
1929 *The Magic Island*. New York: Harcourt, Brace and Co.
- Simons, Anne
1995 The Beginning of the End. *In* *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 42–60. Berkeley: University of California Press.
- Sluka, Jeffrey A.
1990 Participant Observation in Violent Social Contexts. *Human Organization* 49(2):114–126.
1995 Reflections on Managing Danger in Fieldwork: Dangerous Anthropology in Belfast. *In* *Fieldwork under Fire: Contemporary Studies of Violence and Survival*. Carolyn Nordstrom and Antonius C. G. M. Robben, eds. Pp. 276–294. Berkeley: University of California Press.
- Sluka, Jeffrey A., ed.
2000 *Death Squad: The Anthropology of State Terror*. Philadelphia: University of Pennsylvania Press.
- Stoll, David
1993 *Between Two Armies in the Ixil Towns of Guatemala*. New York: Columbia University Press.
- Suárez-Orozco, Marcelo
1992 A Grammar of Terror: Psychological Responses to State Terrorism in Dirty War and Post–Dirty War Argentina. *In* *The Paths to Domination, Resistance, and Terror*. Carolyn Nordstrom and JoAnn Martin, eds. Pp. 219–259. Berkeley: University of California Press.
- Tambiah, Stanley Jeyaraja
1992 *Buddhism Betrayed? Religion, Politics, and Violence in Sri Lanka*. Chicago: University of Chicago Press.
- Taussig, Michael
1987 *Shamanism, Colonialism, and the Wild Man: A Study in Terror and Healing*. Chicago: University of Chicago Press.
- Trouillot, Michel-Rolph
1990 *Haiti: State against Nation*. New York: Monthly Review Press.
- Warren, Kay B.*
2000 Conclusion: Death Squads and Wider Complicities: Dilemmas for the Anthropology of Violence. *In* *Death Squad: The Anthropology of State Terror*. Jeffrey A. Sluka, ed. Pp. 226–247. Philadelphia: University of Pennsylvania Press.
- Warren, Kay B., ed.
1993 *The Violence Within: Cultural and Political Opposition in Divided Nations*. Boulder, CO: Westview Press.
- Williams, Joseph
1933 *Voodoo and Obeays: Phases of West African Witchcraft*. New York: AMS Press.
- Wirkus, Faustin
1931 *The White King of La Gonave*. Garden City, NY: Garden City Publishing Co.
- Wolf, Margery
1990 Chinanotes: Engendering Anthropology. *In* *Fieldnotes: The Makings of Anthropology*. Roger Sanjek, ed. Pp. 343–355. Ithaca, NY: Cornell University Press.