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## *Spatial Practices: Fieldwork, Travel, and the Disciplining of Anthropology*

The day after the Los Angeles earthquake of 1994, I watched a TV interview with an earth scientist. He said he had been “in the field” that morning looking for new fault lines. It was only after a minute or so of talk that I realized he had been flying around in a helicopter the whole time. Could this be fieldwork? I was intrigued by his invocation of the field, and somehow unsatisfied.

My dictionary begins its long list of definitions for “field” with one about open spaces and another that specifies cleared space. The eye is unimpeded, free to roam. In anthropology Marcel Griaule pioneered the use of aerial photography, a method continued, now and again, by others. But if overview, real or imagined, has long been part of fieldwork, there was still an oxymoronic bump in the earth scientist’s airborne “field.” Particularly in geology, indeed in all the sciences which value fieldwork, the practice of research “on the ground,” observing minute particulars, has been a *sine qua non*. The French equivalent, *terrain*, is unequivocal. Gentlemen naturalists were supposed to have muddy boots. Fieldwork is earthbound—intimately involved in the natural and social landscape.

It was not always so. Henrika Kuklick (1997) reminds us that the move toward professional field research in a range of disciplines, including anthropology, took place at a particular historical moment: in the late nineteenth century. A presumption in favor of professional work that was

down-close, empirical, and interactive was quickly naturalized. Fieldwork would put theory to the test; it would *ground* interpretation.

In this context, flying around in a helicopter seemed a bit abstract. Yet, on reflection, I had to allow the earth scientist his practice of going "into the field" while never setting foot there. In some crucial way, his use of the term qualified. What mattered was not simply the acquisition of fresh empirical data. A satellite photo could provide that. What made this fieldwork was the act of physically *going out* into a *cleared place of work*. "Going out" presupposes a spatial distinction between a home base and an exterior place of discovery. A cleared space of work assumes that one can keep out distracting influences. A field, by definition, is not overgrown. The earth scientist could not have done his helicopter "fieldwork" on a foggy day. An archeologist cannot excavate a site properly if it is inhabited or built over. An anthropologist may feel it necessary to clear his or her field, at least conceptually, of tourists, missionaries, or government troops. Going out into a cleared place of work presupposes specific practices of displacement and focused, disciplined attention.

In this essay I hope to clarify a crucial and ambivalent anthropological legacy: the role of travel, physical displacement, and temporary dwelling away from home in the constitution of fieldwork. I will discuss fieldwork and travel in three sections. The first sketches some recent developments in sociocultural anthropology, showing where classic research practices are under pressure. I suggest why fieldwork remains a central feature of disciplinary self-definition. The second section focuses on fieldwork as an embodied spatial practice, showing how, since the turn of the century, a disciplined professional body has been articulated along a changing border with literary and journalistic travel practices. In opposition to these purportedly superficial, subjective, and biased forms of knowledge, anthropological research was oriented toward the production of deep, *cultural* knowledge. I argue that the border is unstable, constantly renegotiated. The third section surveys current contestations of normative Euro-American travel histories that have long structured anthropology's research practices. Notions of community insides and outsides, homes and abroads, fields and metropolises, are increasingly challenged by postexotic, decolonizing trends. It is much less clear what counts, today, as acceptable fieldwork, the range of spatial practices "cleared" by the discipline.

I borrow the phrase "spatial practice" from Michel de Certeau's book

*The Practice of Everyday Life* (1984). For de Certeau, "space" is never ontologically given. It is discursively mapped and corporeally practiced. An urban neighborhood, for example, may be laid out physically according to a street plan. But it is not a space until it is practiced by people's active occupation, their movements through and around it. In this perspective, there is nothing given about a "field." It must be worked, turned into a discrete social space, by embodied practices of interactive travel. I will have more to say, en route, about the expanded sense, and limitations, of the term "travel" as I use it. And I will be concerned, primarily, with norms and ideal types. In the introduction to an important collection of essays on the "field" in anthropology, Gupta and Ferguson (1996) argue that current practice potentially draws on a broad range of ethnographic activities, some of them unorthodox by modern standards. But they confirm that, since the 1920s, a recognizable norm has held sway in the academic centers of Euro-America.<sup>1</sup> Anthropological fieldwork has represented something specific among overlapping sociological and ethnographic methods: an especially deep, extended, and interactive research encounter. That, of course, is the ideal. In practice, criteria of "depth" in fieldwork (length of stay, mode of interaction, repeated visits, grasp of languages) have varied widely, as have actual research experiences.

This multiplicity of practices blurs any sharp, referential meaning for "fieldwork." What are we talking about when we invoke anthropological fieldwork? Before proceeding, I must linger a moment on this problem of definition. Elementary semantics distinguishes several ways meanings are sustained: roughly, by reference, concept, and use. I will draw primarily on the latter two, commonly qualified as "mentalist" (Akmajian et al., 1993: 198–201). Conceptual definitions use a prototype, often a visual image, to define a core against which variants are evaluated. A famous photograph of Malinowski's tent pitched in the midst of a Trobriand village has long served as a potent mental *image* of anthropological fieldwork. (Everyone "knows" it, but how many could describe the actual scene?) There have been other images: visions of personal interaction—for example, photos of Margaret Mead leaning intently toward a Balinese mother and baby. Moreover, as I have already suggested, the word "field" itself conjures up mental images of cleared space, cultivation, work, ground. When one speaks of working in the field, or going into the field, one draws on mental images of a distinct place with an inside and outside, reached by practices of physical movement.

These mental images focus and constrain definitions. For example, they make it strange to say that an anthropologist in his or her office talking on the phone is doing fieldwork—even if what is actually happening is the disciplined, interactive collection of ethnographic data. Images materialize concepts, producing a semantic field that seems sharp at the “center” and blurred at the “edges.” The same function is served by more abstract concepts. A range of phenomena are gathered around prototypes; I will, in deference to Kuhn (1970: 187), speak of *exemplars*. Just as a robin is taken to be a more typical bird than a penguin, thus helping to define the concept “bird,” so certain exemplary cases of fieldwork anchor heterogeneous experiences. “Exotic” fieldwork pursued over a continuous period of at least a year has, for some time now, set the norm against which other practices are judged. Given this exemplar, different practices of cross-cultural research seem less like “real” fieldwork (Weston, 1997).

Real for whom? The meaning of an expression is ultimately determined by a language community. This use criterion opens space for a history and sociology of meanings. But it is complicated, in the present case, by the fact that those people recognized as anthropologists (the relevant community) are critically defined by having accepted and done something close (or close enough) to “real fieldwork.” The boundaries of the relevant community have been (and are, increasingly) constituted by struggles over the term’s proper range of meanings. This complication is present, to some extent, in all community-use criteria for meaning, especially when “essentially contested concepts” (Gallie, 1964) are at stake. But in the case of anthropologists and “fieldwork,” the loop of mutual constitution is unusually tight. The community does not simply use (define) the term “fieldwork”; it is materially used (defined) by it. A different range of meanings would make a different community of anthropologists, and vice versa. The sociopolitical stakes in these definitions—issues of inclusion and exclusion, center and periphery—need to be kept explicit.

### Disciplinary Borderlands

Consider the project of Karen McCarthy Brown, who studied a vodou priestess in Brooklyn (and accompanied her on a visit to Haiti). Brown traveled into the field by car, or on the New York subway, from her home in Manhattan. Her ethnography was less a practice of intensive dwelling (the “tent in the village”) and more a matter of repeated visiting, collabo-

rative work. Or perhaps her work involved what Renato Rosaldo once called, in a discussion of what makes anthropological ethnography distinctive, "deep hanging out."<sup>2</sup> Before working with Alourdes, the subject of her study, Brown had made research trips to Haiti. But when she visited Alourdes for the first time, she felt a new kind of displacement:

Our nostrils filled with the smells of charcoal and roasting meat and our ears with overlapping episodes of salsa, reggae, and the bouncy monotony of what Haitians call jazz. Animated conversations could be heard in Haitian French Creole, Spanish, and more than one lyrical dialect of English. The street was a crazy quilt of shops: Chicka-Licka, the Ashanti Bazaar, a storefront Christian church with an improbably long and specific name, a Haitian restaurant, and Botanica Shango—one of the apothecaries of New World African religions offering fast-luck and get-rich-quick powders, High John the Conqueror root, and votive candles marked for the Seven African Powers. I was no more than a few miles from my home in lower Manhattan, but I felt as if I had taken a wrong turn, slipped through a crack between worlds, and emerged on the main street of a tropical city. (Brown, 1991: 1)

Compare this "arrival scene" (Pratt, 1986) with Malinowski's famous "Imagine yourself set down [on a Trobriand Island beach]" (Malinowski, 1961). Both rhetorically construct a sharply different, tropical "place," a topos and topic for the work to follow. But Brown's contemporary version is presented with a degree of irony: her tropical city in Brooklyn is sensuously real *and* imaginary—an "illusion," she goes on to call it, projected by an ethnographic traveler in a complexly hybrid world-city. Hers is not a neighborhood (urban village) study. If it has a microcosmic locus, it is Alourdes' three-story row house in the shadow of the Brooklyn-Queens Expressway—home of the only Haitian family in a black North American neighborhood. Diasporic "Haiti," in this ethnography, is multiply located. Brown's ethnography is situated less by a discrete place, a field she enters and inhabits for a time, than by an interpersonal relationship—a mixture of observation, dialogue, apprenticeship, and friendship—with Alourdes. With this relationship as its center, a cultural world of individuals, places, memories, and practices is evoked. Brown visits, frequents, this world both in Alourdes' house, where ceremonies and socializing take place, and elsewhere. Brown's "field" is wherever she is with Alourdes. She returns, typically, to sleep, reflect, write up her notes, lead her life at home in lower Manhattan.

Following established fieldwork practice, Brown's ethnography contains little detail about the everyday life in Manhattan interspersed with the visits to Brooklyn. Her field remains discrete, "out there." And while the relationship/culture under study cannot be neatly spatialized, a different place is visited intensively. There is a physical, interpersonal interaction with a distinct, often exotic, world, leading to an experience of initiation. While the spatial practice of dwelling, taking up residence in a community, is not observed, the ethnographer's movement "in" and "out," her coming and going, is systematic. One wonders what effects these proximities and distances have on the way Brown's research is conceived and represented. How, for example, does she pull back from her research relationship in order to write about it? This taking of distance has typically been conceived as a "departure" from the field, a place clearly removed from home (Crapanzano, 1977). What difference does it make when one's "informant" routinely calls one at home to demand help with a ceremony, support in a crisis, a favor? Spatial practices of travel and temporal practices of writing have been crucial to the definition and representation of a *topic*—the translation of ongoing experience and entangled relationship into something distanced and representable (Clifford, 1990). How did Brown negotiate this translation in a field whose boundaries were so fluid?

A similar but more extreme challenge for the definition of "real" fieldwork is raised by David Edwards in his article "Afghanistan, Ethnography, and the New World Order." Entering anthropology with hopes of returning to Afghanistan to conduct "a traditional sort of village study in some mountain community," Edwards confronted a war-torn, dispersed "field": "Since 1982, I have carried out fieldwork in a variety of places, including the city of Peshawar, Pakistan, and various refugee camps scattered around the Northwest Frontier Province. One summer, I also traveled inside Afghanistan to observe the operations of a group of *mujahadin*, and I have spent quite a bit of time among Afghan refugees in the Washington, D.C., area. Finally, I have been monitoring the activities of an Afghan computer newsgroup" (Edwards, 1994: 343).

Multi-locale ethnography (Marcus and Fischer, 1986) is increasingly familiar; multi-locale *fieldwork* is an oxymoron. How many sites can be studied intensively before criteria of "depth" are compromised?<sup>3</sup> Roger Rouse's fieldwork in two linked sites retains the notion of a single, albeit mobile, community (Rouse, 1991). Karen McCarthy Brown stays within the "world" of an individual. But David Edwards' practice is more scat-

tered. Indeed, when he begins to link his dispersed instances of "Afghan culture," he must rely on fairly weak thematic resonances and the common feeling of "ambiguity" they produce—at least for him. Whatever the borders of Edwards' "multiply inflected" cultural object (Harding, 1994), the range of spatial practices he adopts to encounter it is exemplary. He writes that he has "carried out fieldwork" in a city and refugee camps; he has "traveled" to observe the *mujahadin*; he has "spent quite a lot of time" (hanging out, deeply?) with Afghans in Washington, D.C.; and he has been "monitoring" the exile computer newsgroup. This last ethnographic activity is the least comfortable for Edwards (349). At the time of writing, he has only been "lurking" not posting his own messages. His research on the Internet is not yet interactive. But it is very informative. Edwards intensively listens in on a group of exiled Afghans—male, relatively affluent—worrying together about politics, religious practices, the nature and boundaries of their community.

The experiences of Karen McCarthy Brown and David Edwards suggest some of the current pressures on anthropological fieldwork seen as a spatial practice of intensive dwelling. The "field" in sociocultural anthropology has been constituted by a "historically specific range of distances, boundaries, and modes of travel" (Clifford, 1990: 64). These are changing, as the geography of distance and difference alters in postcolonial/neocolonial situations, as power relations of research are reconfigured, as new technologies of transport and communication are deployed, and as "natives" are recognized for their specific worldly experiences and histories of dwelling and traveling (Appadurai, 1988a; Clifford, 1992; Teaiwa, 1993; Narayan, 1993). What remains of classic anthropological practices in these new situations? How are the notions of travel, boundary, co-residence, interaction, inside and outside that have defined the field and proper fieldwork being challenged and reworked in contemporary anthropology?



Before taking up these questions, we need a clear sense of what dominant practices of the "field" are at issue, what issues of disciplinary definition constrain current arguments. Fieldwork normally involves physically leaving "home" (however that is defined) to travel in and out of some distinctly *different* setting. Today, the setting can be Highland New Guinea; or it can be a neighborhood, house, office, hospital, church, or lab. It can be

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defined as a mobile society, that of long-distance truckers, for example—providing one spends long hours in the cab, talking (Agar, 1985). Intensive, “deep” interaction is required, something canonically guaranteed by the spatial practice of extended, if temporary, dwelling in a community. Fieldwork can also involve repeated short visits, as in the American tradition of reservation ethnology. Teamwork and long-term research (Foster et al., 1979) have been variously practiced in different local and national traditions. But common to these practices, anthropological fieldwork requires that one do something more than pass through. One must do more than conduct interviews, make surveys, or compose journalistic reports. This requirement continues today, embodied in a flexible range of activities, from co-residence to various forms of collaboration and advocacy. The legacy of intensive fieldwork defines *anthropological* styles of research, styles critically important for disciplinary (self-)recognition.<sup>4</sup>

There are no natural or intrinsic disciplines. All knowledge is interdisciplinary. Thus, disciplines define and redefine themselves interactively and competitively. They do this by inventing traditions and canons, by consecrating methodological norms and research practices, by appropriating, translating, silencing, and holding at bay adjacent perspectives. Active processes of *disciplining* operate at various levels, defining “hot” and “cold” domains of the disciplinary culture, certain areas that change rapidly and others that are relatively invariant. They articulate, in tactically shifting ways, the solid core and the negotiable edge of a recognizable domain of knowledge and research practice. Institutionalization channels and slows but cannot stop these processes of redefinition, except at peril of sclerosis.

Consider the choices faced today by someone planning the syllabus for an introductory graduate proseminar in sociocultural anthropology.<sup>5</sup> Given a limited number of weeks, how important is it that novice anthropologists read Radcliffe-Brown? Robert Lowie? Would it be better to include Meyer Fortes or Kenneth Burke? Lévi-Strauss, surely . . . but why not also Simone de Beauvoir? Franz Boas, of course . . . and Frantz Fanon? Margaret Mead or Marx . . . or E. P. Thompson, or Zora Neale Hurston, or Michel Foucault? Melville Herskovitz perhaps . . . and W. E. B. DuBois? St. Clair Drake? Work on photography and media? Kinship, once a disciplinary core, is now actively forgotten in some departments. Anthropological linguistics, still invoked as one of the canonical “four fields,” is very unevenly covered. In some programs, one is more likely to read literary



theory, colonial history, or cognitive science . . . Synthetic notions of *man*, the "culture-bearing animal," that once stitched together a discipline now seem antiquated, or perverse. Can the disciplinary center hold? In the introductory syllabus, a hybrid selection will eventually be made, tuned to local traditions and current demands, with recognizably "anthropological" authors at the center. (Sometimes the "pure" disciplinary lineage will be cordoned off in a History of Anthropology course, required or not.) Anthropology reproduces itself while selectively engaging with relevant interlocutors: from social history, from cultural studies, from biology, from cognitive science, from minority and feminist scholarship, from colonial discourse critique, from semiotics and media studies, from literary and discourse analysis, from sociology, from psychology, from linguistics, from ecology, from political economy, from . . .

Sociocultural anthropology has always been a fluid, relatively open discipline. It has prided itself on its ability to draw on, enrich, and synthesize other fields of study. Writing in 1964, Eric Wolf optimistically defined anthropology as a "discipline between disciplines" (Wolf, 1964: x). But this openness poses recurring problems of self-definition. And partly because its theoretical purview has remained so broad and interdisciplinary, despite recurring attempts to cut it down to size, the discipline has focused on research practices as defining, core elements. Fieldwork has played—and continues to play—a central disciplining function. In the current conjuncture, the range of topics anthropology can study and the array of theoretical perspectives it can deploy is immense. In these areas the discipline is "hot," constantly changing, hybridizing. In the "colder" domain of acceptable fieldwork, change is also occurring but more slowly. In most anthropological milieus, "real" fieldwork continues to be actively defended against other ethnographic styles.

The exotic exemplar—co-residence for extended periods away from home, the "tent in the village"—retains considerable authority. But it has, in practice, been decentered. The various spatial practices it authorized, as well as the relevant criteria for evaluating "depth" and "intensity," have changed and continue to change. Contemporary political, cultural, and economic conditions bring new pressures and opportunities to anthropology. The range of possible venues for ethnographic study has expanded dramatically, and the discipline's potential membership is more diverse. Its geopolitical location (no longer so securely in the Euro-American "center") is challenged. In this context of change and contestation, academic an-

thropology struggles to reinvent its traditions in new circumstances. Like the changing societies it studies, the discipline sustains itself in blurred and policed borderlands, using strategies of hybridization and reauthentication, assimilation and exclusion.

Suggestive boundary problems emerge from David Edwards' awkward time on the Afghan Internet. What if someone studied the culture of computer hackers (a perfectly acceptable anthropology project in many, if not all, departments) and in the process never "interfaced" in the flesh with a single hacker. Would the months, even years, spent on the Net be fieldwork? The research might well pass both the length-of-stay and the "depth"/interactivity tests. (We know that some strange and intense conversations can occur over the Net.) And electronic travel is, after all, a kind of *dépaysement*. It could add up to intensive participant observation in a different community without ever physically leaving home. When I've asked anthropologists whether this could be fieldwork, they have generally responded "maybe," even, in one case, "of course." But when I press the point, asking whether they would supervise a Ph.D. dissertation based primarily on this kind of disembodied research, they hesitate or say no: it would not be currently acceptable fieldwork. Given the traditions of the discipline, a graduate student would be ill-advised to follow such a course. We come up against the institutional-historical constraints that enforce the distinction between fieldwork and a broader range of ethnographic activities. Fieldwork in anthropology is sedimented with a disciplinary history, and it continues to function as a rite of passage and marker of professionalism.

A boundary that currently preoccupies sociocultural anthropology is that which separates it from a heterogeneous collection of academic practices often called "cultural studies."<sup>6</sup> This border renegotiates, in a new context, some of the long-established divisions and crossings of sociology and anthropology. Qualitative sociology, at least, has its own ethnographic traditions, increasingly relevant to a postexoticist anthropology.<sup>7</sup> But given fairly firm institutional identities, in the United States at least, the border with sociology seems less unruly than that with "cultural studies." This new site of border crossing and policing partly repeats an ongoing, fraught relationship with "textualism" or "lit. crit." The move to "recapture" anthropology—manifested in dismissals of the collection *Writing Culture* (Clifford and Marcus, 1986) and more recently, often incoherently, in sweeping rejections of "postmodern anthropology"—is by now routine in

some quarters. But the border with cultural studies may be less manageable; for it is easier to maintain a clear separation when the disciplinary other, literary-rhetorical theory or textualist semiotics, has no fieldwork component and at best an anecdotal "ethnographic" approach to cultural phenomena. "Cultural studies," in its Birmingham tradition as well as in some of its sociological veins, possesses a developed ethnographic tradition much closer to anthropological fieldwork. The distinction, "We do fieldwork, they do discourse analysis," is more difficult to sustain. Some anthropologists have turned to cultural studies ethnography for inspiration (Lave et al., 1992), and indeed there is much to learn from its increasingly complex articulations of class, gender, race, and sexuality. Moreover, what Paul Willis did with the working-class "lads" of *Learning to Labour* (1977)—hanging out with them at school, talking with parents, working alongside them on the shop floor—is comparable to good fieldwork. Its depth of social interaction was surely greater than, say, that achieved by Evans-Pritchard during his ten months with hostile and reluctant Nuer.

Many contemporary anthropological projects are difficult to distinguish from cultural studies work. For example, Susan Harding is writing an ethnography of Christian fundamentalism in the United States. She has done extensive participant observation in Lynchburg, Virginia, in and around Jerry Falwell's church. And of course the television ministry of Falwell and others like him is very much her concern—her "field." Indeed, she is interested not primarily in a spatially defined community but in what she calls the "discourse" of the new fundamentalisms.<sup>8</sup> She is concerned with TV programs, sermons, novels, media of all kinds, as well as with conversations and everyday behavior. Harding's mixture of participant-observation, cultural criticism, and media and discourse analysis is characteristic of work in the current ethnographic border zones. How "anthropological" is it? How different is Susan Harding's frequenting of evangelicals in Lynchburg from Willis' or Angela McRobbie's studies of youth cultures in Britain or the earlier work of the Chicago School sociologists? There are certainly differences, but they do not coalesce as a discrete method, and there is considerable overlap.

One important difference is Harding's insistence that a crucial portion of her ethnographic work must involve *living with* an evangelical Christian family. Indeed, she reports that this was when she felt she had really "entered the field." Previously she had stayed in a motel. One might think

of this as a classic articulation of fieldwork, deployed in a new setting. In a sense it is. But it is part of a potentially radical decentering. For there can be no question of calling the period of intensive co-residence in Lynchburg the essence or core of the project to which the TV viewing and reading were ancillary. In Harding's project, "fieldwork" was an important way of finding out how the new fundamentalism was lived in everyday terms. And while it certainly helped define her hybrid project as anthropological, it was not a privileged site of interactive depth or initiation.

Harding's work is an example of research which draws on cultural studies, discourse analysis, and gender and media studies while maintaining crucial anthropological features. It marks a current direction for the discipline, one in which fieldwork remains a necessary but no longer privileged method. Does this mean that the institutional border between anthropology, cultural studies, and allied traditions is open? Far from it. Precisely because the crossings are so promiscuous, the overlaps so frequent, actions to reassert identity are mounted at strategic sites and moments. These include the initiatory process of graduate certification, and moments when people need to be denied a job, funding, or authority. In the everyday disciplining that makes anthropologists and not cultural studies scholars, the boundary is reasserted, routinely. Most publicly perhaps, when graduate students' "field" projects are approved, the distinctive spatial practices that have defined anthropology tend to be reasserted—often in nonnegotiable ways.

The concept of the field and the disciplinary practices associated with it constitute a central, ambiguous legacy for anthropology. Fieldwork has become a "problem" because of its positivist and colonialist historical associations (the field as "laboratory," the field as place of "discovery" for privileged sojourners). It has also become more difficult to circumscribe, given the proliferation of ethnographic topics and the time-space compressions (Harvey, 1989) characteristic of postmodern, postcolonial/neocolonial situations. What will anthropology make of this problem? Time will tell. Fieldwork, a research practice predicated on interactive depth and spatialized difference, is being "reworked" (Gupta and Ferguson's term), for it is one of the few relatively clear marks of disciplinary distinction left. But how wide can the range of sanctioned practices be? And how "decentered" (Gupta and Ferguson) can fieldwork become before it is just one of a range of ethnographic and historical methods used by the discipline, in concert with other disciplines?

Anthropology has always been more than fieldwork, but fieldwork has been something an anthropologist *should* have done, more or less well, at least once.<sup>9</sup> Will this change? Perhaps it should. Perhaps fieldwork will become merely a research tool rather than an essential disposition or professional marker. Time will tell. At present, however, fieldwork remains critically important—a disciplining process and an ambiguous legacy.

### The Fieldwork Habitus

The institutionalization of fieldwork in the late nineteenth and early twentieth centuries can be understood within a larger history of "travel." (I use the term in an expanded sense, of which more in a moment.) Among Westerners traveling and dwelling abroad, the anthropological fieldworker was a latecomer. Explorers, missionaries, colonial officers, traders, colonists, and natural scientific researchers were well-established figures before the emergence of the on-the-ground anthropological professional. Prior to Boas, Malinowski, Mead, Firth, et al., the anthropological scholar usually remained at home, processing ethnographic information sent by "men on the spot" who were drawn from among the sojourners just mentioned. If metropolitan scholars ventured out, it was on survey and museum-collection expeditions. Whatever exceptions there may have been to this pattern, interactive depth and co-residence were not yet professional requirements.

When intensive fieldwork began to be championed by the Boasians and Malinowskians, an effort was required to distinguish the kind of knowledge produced by this method from that acquired by other long-term residents in the areas studied. At least three "disciplinary others" were held at arm's length: the missionary, the colonial officer, and the travel writer (journalist or literary exoticist). Much could be said about anthropology's fraught relations with these three professional alter egos whose purportedly amateur, interventionist, subjective accounts of indigenous life would be "killed by science," as Malinowski put it.<sup>10</sup> My focus here is limited to the border with literary and journalistic travel. As a methodological principle I do not presuppose the discipline's self-definitions, whether positive ("we have a special research practice and understanding of human culture") or negative ("we are *not* missionaries, colonial officers, or travel writers"). Rather, I assume that these definitions must be actively produced, negotiated, and renegotiated through changing historical relation-

ships. It is often easier to say clearly what one is not than what one is. In the early years of modern anthropology, while the discipline was still establishing its distinctive research tradition and authoritative exemplars, negative definitions were critical. And in times of uncertain identity (such as the present), definition may be achieved most effectively by naming clear *outsides* rather than by attempting to reduce always diverse and hybrid *insides* to a stable unity. A more or less permanent process of disciplining at the edges sustains recognizable borders in entangled borderlands.

Anthropological research travelers have, of course, regularly depended on missionaries (for grammars, transportation, introductions, and in certain cases for a deeper translation of language and custom than can be acquired in a one- or two-year visit). The fieldworker's professional difference from the missionary, based on real discrepancies of agenda and attitude, has had to be asserted against equally real areas of overlap and dependency. So, too, with colonial (and neocolonial) regimes: ethnographers typically have asserted their aim to understand not govern, to collaborate not exploit. But they have navigated in the dominant society, often enjoying white skin privilege and a physical safety in the field guaranteed by a history of prior punitive expeditions and policing (Schneider, 1995: 139). Scientific fieldwork separated itself from colonial regimes by claiming to be apolitical. This distinction is currently being questioned and renegotiated in the wake of anticolonial movements which have tended not to recognize the distance claimed by anthropologists from contexts of domination and privilege.

The travel writer's transient and literary approach, sharply rejected in the disciplining of fieldwork, has continued to tempt and contaminate the scientific practices of cultural description. Anthropologists are, typically, people who leave and write. Seen in a long historical perspective, fieldwork is a distinctive cluster of travel practices (largely but not exclusively Western). Travel and travel discourse should not be reduced to the relatively recent tradition of literary travel, a narrowed conception which emerged in the late nineteenth and early twentieth centuries. This notion of "travel" was articulated against an emerging ethnography (and other forms of "scientific" field research) on the one hand, and against tourism (a practice defined as incapable of producing serious knowledge) on the other. The spatial and textual practices of what might now be called "sophisticated travel"—a phrase taken from *New York Times* supplements

catering to the "independent" traveler<sup>11</sup>—function within an elite, and highly differentiated, tourist sector defined by the statement, "We are not tourists." (Jean-Didier Urbain, in *L'idiot du voyage* (1991), has thoroughly analyzed this discursive formation. See also Buzzard, 1993, and Chapter 8 below.) The literary tradition of "sophisticated travel," whose disappearance has been lamented by critics such as Daniel Boorstin and Paul Fussell, is reinvented by a long list of contemporary writers—Paul Theroux, Shirley Hazzard, Bruce Chatwin, Jan Morris, Ronald Wright, et al.<sup>12</sup>

"Travel," as I use it, is an inclusive term embracing a range of more or less voluntarist practices of leaving "home" to go to some "other" place. The displacement takes place for the purpose of gain—material, spiritual, scientific. It involves obtaining knowledge and/or having an "experience" (exciting, edifying, pleasurable, estranging, broadening). The long history of travel that includes the spatial practices of "fieldwork" is predominantly Western-dominated, strongly male, and upper-middle class. Good critical and historical work is now appearing in this comparative domain, paying attention to political, economic, and regional contexts, as well as to the determinations and subversions of gender, class, culture, race, and individual psychology (Hulme, 1986; Porter, 1991; Mills, 1991; Pratt, 1992).

Before the separation of genres associated with the emergence of modern fieldwork, travel and travel writing covered a broad spectrum. In eighteenth-century Europe, a *récit de voyage* or "travel book" might include exploration, adventure, natural science, espionage, commercial prospecting, evangelism, cosmology, philosophy, and ethnography. By the 1920s, however, the research practices and written reports of anthropologists had been much more clearly set apart. No longer scientific travelers or explorers, they were defined as fieldworkers, a change shared with other sciences (Kuklick, 1996). The field was a distinctive cluster of academic research practices, traditions, and representational rules. But while competing practices and rhetorics were actively held at bay in the process, the newly cleared disciplinary space could never be entirely free of contamination. Its borders would have to be rebuilt, shifted, and reworked. Indeed, one way to understand the current "experimentalism" of ethnographic writing is as a renegotiation of the boundary, agonistically defined in the late nineteenth century, with "travel writing."

"Literariness," held at a distance in the figure of the travel writer, has returned to ethnography in the form of strong claims about the prefigu-

ration and rhetorical communication of "data." The facts do not speak for themselves; they are emplotted rather than collected, produced in worldly relationships rather than observed in controlled environments.<sup>13</sup> This growing awareness of the poetical and political contingency of fieldwork—an awareness forced on anthropologists by postwar anticolonial challenges to Euro-American centrality—is reflected in a more concrete textual sense of the ethnographer's location. Elements of the "literary" travel narrative that were excluded from ethnographies (or marginalized in their prefaces) now appear more prominently. These include the researcher's routes into and through "the field"; time in the capital city, registering the surrounding national/transnational context; technologies of transport (getting there as well as being there); interactions with named, idiosyncratic individuals rather than anonymous, representative informants.

In Chapter 1, I worked to decenter the field as a naturalized practice of *dwelling* by proposing a cross-cutting metaphor: fieldwork as *travel encounters*. To decenter or interrupt fieldwork-as-dwelling is not to reject or refute it. Fieldwork has always been a mix of institutionalized practices of dwelling and traveling. But in the disciplinary idealization of the "field," spatial practices of moving to and from, in and out, passing through, have tended to be subsumed by those of dwelling (rapport, initiation, familiarity). This is changing. Ironically, now that much anthropological fieldwork is conducted (like Karen McCarthy Brown's) close to home, the materiality of travel, in and out of the field, becomes more apparent, indeed constitutive of the object/site of study. Fieldwork in cities must distinguish itself from other forms of interclass, interracial travel and appreciation, marking a difference from established traditions of urban social work and liberal "slumming." The home of the research traveler exists in a politicized prior relation to that of the people under study (or, in contemporary parlance, the people "worked with"). These latter may themselves travel regularly to and from the home base of the researcher, if only for employment. (The "ethnographic," cross-cultural knowledge of a maid or service worker is considerable.) These parallel, sometimes intersecting, spatiopolitical relations have also been present in "exotic" anthropological research, particularly when colonial or neocolonial flows of armies, commodities, labor, or education materially link the poles of fieldwork travel. But images of distance, rather than of interconnection and contact, have tended to



naturalize the field as an other *place*. The socially established routes constitutive of field *relations* are harder to ignore when the research is conducted nearby, or when airplanes and telephones compress space.

Fieldwork thus "takes place" in worldly, contingent relations of travel, not in controlled sites of research. Saying this does not simply dissolve the boundary between contemporary fieldwork and travel (or journalistic) work. There are important generic and institutional distinctions. The injunction to dwell intensively, to learn local languages, to produce a "deep" interpretation is a difference that makes a difference. But the border between the two relatively recent traditions of literary travel and academic fieldwork is being renegotiated. Indeed, the example offered above by David Edwards' multiple sites of encounter brings fieldwork (dangerously, some may feel) close to travel. This rapprochement takes a different form in Anna Tsing's innovative ethnography *In the Realm of the Diamond Queen* (1993). Tsing conducts fieldwork in a classic "exotic" site, the Meratus Mountains of South Kalimantan, Indonesia. While preserving disciplinary practices of intensive local interaction, her writing systematically crosses the border of ethnographic analysis and travel narration. Her account historicizes both her own and her subjects' practices of dwelling and traveling, deriving her knowledge from specific encounters between differently cosmopolitan, gendered individuals, not cultural types. (See, particularly, Part Two: "A Science of Travel.") Her field site in what she calls an "out of the way place" is never taken for granted as a natural or traditional environment. It is produced, a contact space, by local, national, and transnational forces of which her research travel is a part.

Edwards and Tsing exemplify exotic fieldwork at the edges of changing academic practice. In both, differently spatialized, we see the increased prominence of practices and tropes commonly associated with travel and travel writing.<sup>14</sup> These are currently visible in much anthropological ethnography, figuring different versions of the routed/rooted researcher, the "positioned subject" (Rosaldo, 1989: 7). Signs of the times include a trend toward use of the first-person singular pronoun in accounts of fieldwork, presented as stories rather than as observations and interpretations. Often the field journal (private, and closer to the "subjective" accounts of travel writing) leaks into the "objective" field data. I am not describing a linear movement from collection to narration, objective to subjective, impersonal to personal, co-residence to travel encounter. It is a question not of a progression from ethnography to travel writing but rather of a shifting

balance and a renegotiation of key *relations* that have constituted the two practices and discourses.



In tracking anthropology's changing relations with travel, we may find it useful to think of the "field" as a habitus rather than as a place, a cluster of *embodied* dispositions and practices. The work of feminist scholars has played a crucial role in specifying the social body of the ethnographer, while criticizing the limitations of androcentric "gender-neutral" work and opening up major new areas of understanding.<sup>15</sup> Similarly, anticolonial pressures, colonial discourse analysis, and critical race theory have decentered the predominantly Western, and white, traditional fieldworker. Seen in light of these interventions, the fieldwork habitus of the Malinowskian generation appears as the articulation of specific, disciplined practices.

This normative "body" was *not* that of a traveler. As it drew on older traditions of scientific travel, it did so in sharpened opposition to romantic, "literary," or subjective strands. The body legitimated by modern fieldwork was not a sensorium moving through extended space, across borders. It was not on an expedition or a survey. Rather, it was a body circulating and working (one might almost say "commuting") within a delimited space. The local map predominated over the tour or itinerary as a technology of physical location. Being there was more important than getting there (and leaving there). The fieldworker was a homebody abroad, not a cosmopolitan visitor. I am, of course, speaking broadly of disciplinary norms and textual figures, not of the actual historical experiences of field anthropologists. In varying degrees, these diverged from the norms while being constrained by them.

Emotions, a necessary part of the controlled empathy of participant-observation, were not accorded primary expression. They could not be the chief source of public judgments about the communities under study. This was particularly true of negative assessments. The moral judgments and curses of the travel writer, based on social frustrations, physical discomforts, and prejudices as well as on principled criticism, were excluded or downplayed. An understanding rapport and measured affection were favored. Expressions of overt enthusiasm and love were circumscribed. Anger, frustration, judgments on individuals, desire, and ambivalence went into private diaries. The scandal provoked, in some quarters, by the

publication of Malinowski's intimate diary (1967) was related to the glimpse it gave of a less temperate, race- and sex-conscious subject/body in the field. Early public transgressions of the professional habitus include works by Leiris (1934, written as a field journal), Bowen (1954, in novel form), and Jean Briggs (1970, in which personal emotions perhaps for the first time were central to an ethnographic monograph).

If emotions tended to be marginalized, so, for the most part, did the researcher's experiences of gender, race, and sex. Gender, while occasionally featured (particularly in the "marked," female case), was not publicly recognized as constituting the research process in a systematic way. Margaret Mead, for example, did at times conduct research and write "as a woman," crossing defined women's and men's spheres, but her disciplinary persona was that of a scientifically authoritative cultural observer, of unmarked gender and by default "male." Her more "subjective," "soft" stylistic experiments and popular writings did not bring her credit within the disciplinary fraternity, where she adopted a more "objective," "hard" voice. Lutkehaus (1995) provides a contextual account of these historically gendered locations and Mead's shifting persona. Male researchers of Mead's generation did not conduct research "as men" among locally defined women and men. Many purportedly holistic "cultural" accounts were, in fact, based on intensive work with men only. Overall, the constraints and possibilities attached to the researcher's gender were not salient features of the field habitus.

The same went for race. Here sociocultural anthropology's important theoretical and empirical critique of racialist essences doubtless influenced the professional habitus. "Race" was not the social/historical formation of contemporary critical race theorists (for example, Omi and Winant, 1986; Gilroy, 1987) but a biological essence whose "natural" determinations were contested by the contextual determinations of "culture." Anthropologists, the culture-bearing scholars, needed to decenter and cross over putatively essential racial lines. Their interactive and intensive understanding of cultural formations gave them a powerful tool against racial reductions. But in attacking a *natural* phenomenon, they did not confront race as a *historical* formation that located their subjects politically and that simultaneously constrained and empowered their own research (Harrison, 1991: 3).<sup>16</sup> Occasionally this positioning could be glimpsed—for example, in Evans-Pritchard's introduction to *The Nuer* (1940); but it was not part of the explicit body, the professional habitus, of the fieldworker.

By contrast, travel writers often noticed color and spoke from a racialized position. Of course, they were not necessarily critical of the relations involved—often quite the reverse! The point is not to celebrate a relatively greater awareness of race—and gender—in travel writing but to show how, by contrast, the habitus of the ethnographer downplayed these historical determinations. However marked it was by gender, race, caste, or class privilege, ethnography needed to transcend such locations in order to articulate a deeper, *cultural* understanding. This articulation was based on powerful techniques, including at least the following: extended co-residence; systematic observation and recording of data; effective interlocution in at least one local language; a specific mix of alliance, complicity, friendship, respect, coercion, and ironic toleration leading to “rapport”; a hermeneutic attention to deep or implicit structures and meanings. These techniques were designed to produce (and often did produce, within the horizons I am trying to delimit) more contextual, less reductive understandings of local lifeways than did the passing observations of the traveler.

Some writers who could be classified as travelers stayed for extended periods abroad, spoke local languages, and had complex views of indigenous (as well as of creole/colonial) life. Some classified as ethnographers stayed relatively short times, spoke languages badly, and did not interact intensively. The range of actual social relations, communicative techniques, and spatial practices deployed between the poles of fieldwork and travel is a continuum, not a sharp border. There has been considerable overlap.<sup>17</sup> But in spite of, or rather because of, this border complexity, the discursive/institutional lines had to be clearly drawn. This need sustained pressures which, over time, gathered empirical experiences closer to the two poles. In this process, the “superficiality” of the traveler and travel writer was opposed to the “depth” of the fieldworker. But one might also say, provocatively, that the former’s “promiscuity” was disciplined in favor of the “family values” often invoked in ethnographic prefaces: fieldwork as a process of getting along with others, of adoption, initiation, learning local norms—much as a child learns.

The habitus of modern fieldwork, defined against that of travel, has proscribed interactive modes long associated with travel experience. Perhaps the most absolute continuing taboo is on sexual liaisons. Fieldworkers could love but not desire the “objects” of their attention. On the continuum of possible relations, sexual entanglements were defined as

dangerous, too close. Participant-observation, a delicate management of distance and proximity, should not include entanglements in which the ability to maintain perspective might be lost. Sexual relations could not be avowed sources of research knowledge. Nor could going into trance or taking hallucinogens, though the taboo there has been somewhat less strict, a certain amount of "experimentation" sometimes being justifiable in the name of participant-observation. Sexual experimentation was, however, out of bounds. A disciplined, participant-observing body "went along" with indigenous life, selectively.

At its inception, though, the taboo on sex may have been less against "going native" or losing critical distance than against "going traveling," violating a professional habitus. In travel practices and texts, having sex, heterosexual and homosexual, with local people was common. Indeed in certain travel circuits, such as the nineteenth-century *voyage en Orient*, it was quasi-obligatory.<sup>18</sup> A popular writer such as Pierre Loti consecrated his authority, his access to the mysterious and feminized Other, through stories of sexual encounter. In fieldwork accounts, however, such stories have been virtually nonexistent. Only recently, and still rarely, has the taboo been broken (Rabinow, 1977; Cesara, 1982). Why should sharing beds be a less appropriate source of fieldwork knowledge than sharing food? There may, of course, be many practical reasons for sexual restraint in the field, just as certain places and activities may be off limits to the tactful (and locally dependent) sojourner. But they are not off limits in all places and at all times. Practical constraints, which vary widely, cannot account for the disciplinary taboo on sex in fieldwork.<sup>19</sup>

Enough has been said, perhaps, to make the central point: a disciplinary habitus has been sustained around the embodied activity of fieldwork: an ungendered, unraced, sexually inactive subject interacts intensively (on hermeneutic/scientific levels, at the very least) with its interlocutors. If actual experiences in the field have diverged from the norm, if the taboos have sometimes been broken, and if the disciplinary habitus is now publicly contested, its normative power remains.



Another common travel practice before 1900, cross-dressing, was suppressed or channeled in the disciplining of modern fieldwork's professional "body." This is a far-reaching topic, and I must limit myself to preliminary remarks. Daniel Defert (1984) has written suggestively on the

history of "clothing" in codes of European travel observation prior to the nineteenth century. A substantial, integral link was once assumed between the person and his/her outward appearance—*habitus*, in Defert's premodern usage.<sup>20</sup> In a deep sense it was understood that "clothes make the man" ("L'habit fait le moine"). Interpretations of *habitus*, not to be confused with *habits* ("clothes") or with the later concept of culture, were a necessary part of travel interactions. This included the communicative manipulation of appearances—what might be called, somewhat anachronistically, cultural cross-dressing. By the nineteenth century, in Defert's account, *habitus* had been reduced to *habits*, to surface coverings and adornments; *costume* had emerged as a deformation of the richer *coutume* (a term which combined the ideas of costume and custom).

Clothes would become just one of many elements in a taxonomy of observations made by scientific travelers, components of an emerging *cultural* explanation. Defert perceives this transition in Gérando's scientific advice to travelers and explorers, published in 1800. Explorers have often merely described the clothes of indigenous peoples, he wrote. You should go farther and inquire why they may or may not be willing to give up their traditional clothing for ours, and how they conceive of their origin (Defert, 1984: 39). Here the interpretive grid of *habitus* is replaced (and made to seem superficial) by a deeper conception of identity and difference. Travel relations had long been organized by complex and highly codified protocols, "surface" semiotics and transactions. The interpretation and manipulation of clothing, gesture, and appearance were integral to these practices. Seen as the outcome of this tradition, nineteenth-century cultural cross-dressing was more than just dress-up. A serious, communicative play with appearances and a site of crossover, it articulated a less absolute or essential notion of difference than that instituted by relativist notions of culture with their concepts of nativeness inscribed in language, tradition, place, ecology, and—more or less implicitly—race. The experiences of a Richard Burton or an Isabelle Eberhardt passing as "Orientals," and even the more blatantly theatrical costuming of Flaubert in Egypt or Loti on shore leave, partake of a complex tradition of travel practices held at arm's length by a modernizing ethnography.<sup>21</sup>

Seen from the perspective of fieldwork (intensive, interactive, based in language learning), cross-dressing could appear only as superficial dress-up, a kind of touristic slumming. In this optique, the practices of an ethnographer like Frank Hamilton Cushing, who adopted Zuni dress (and

even, it has been suggested, produced "authentic" indigenous artifacts), would be somewhat embarrassing. His intensive, interactive research was not quite "modern fieldwork." A similar sense of embarrassment is experienced today by many viewers of Timothy Asche's film *A Man Called Bee*, devoted to Napoleon Chagnon's research among the Yanomami. I am thinking particularly of the opening shot, which zooms in slowly on a painted, scantily clad figure in a fighting pose who turns out, finally, to be the anthropologist. Whatever the intent of this opening, satiric or otherwise (it's not entirely clear), the impression remains that this is not a "professional" way to appear. A certain excess is registered, perhaps too easily written off as egotism. Liza Dalby's book *Geisha* (1983), which includes photographs of the anthropologist being transformed through makeup and wearing full geisha attire, is more acceptable, since the adoption of a geisha "habitus" (in Defert's older sense—a mode of being, manifested through clothes, gesture, and appearance) is a central issue in her participant observation and written ethnography. Yet the photographs of Dalby looking almost exactly like a "real" geisha break with established ethnographic conventions.

At another pole are the photographs published by Malinowski (in *Coral Gardens and Their Magic*, 1935) of himself in the field. He is dressed entirely in white, surrounded by black bodies, sharply distinguished by posture and attitude. This is insistently not a man about to "go native." Such a self-presentation is akin to the gestures of colonial Europeans who dressed formally for dinner in sweltering climates so as not to feel they were slipping "over the edge." (The miraculous starched collars of Conrad's accountant in *Heart of Darkness* are a paradigm case in colonial literature.) But ethnographers have not, typically, been so formal, and I would suggest that their fieldwork habitus was more of an intermediate formation, predicated on not theatrically standing out from local life (not asserting their difference or authority by wearing military uniforms, pith helmets, and the like) while remaining clearly marked by white skin, proximity to cameras, note pads, and other nonnative accoutrements.<sup>22</sup> Most professional fieldworkers did not try to disappear into the field by indulging in "superficial" travel practices of masquerade. Their embodied distinction suggested connections at deeper, hermeneutic levels, understandings forged through language, co-residence, and cultural knowledge.

More than a few telling glimpses of the anthropologist's habitus, overlapping and distinct from that of the traveler, are provided by Lévi-Strauss

in *Tristes Tropiques* (1973). "In September 1950," he writes, "I happened to find myself in a Mogh village in the Chittagong hill tracts." After several days, he ascends to the local temple, whose gong has punctuated his days, along with the sound of "childish voices intoning the Burmese alphabet." All is innocence and order. "We had taken off our shoes to climb the hillock, and the fine, damp clay felt soft under our bare feet." At the entry to the simple, beautiful temple, built on stilts like the village houses, the visitors perform "prescribed ablutions," which after the climb through the mud seem "quite natural and devoid of any religious significance."

A peaceful, barn-like atmosphere pervaded the place and there was a smell of hay in the air. The simple and spacious room which was like a hollowed-out haystack, the courteous behaviour of the two priests standing next to their beds with straw mattresses, the touching care with which they had brought together or made the instruments of worship—all these things helped to bring me closer than I had ever been before to my idea of what a shrine should be like. "You need not do what I am doing," my companion said to me as he prostrated himself on the ground four times before the altar, and I followed his advice. However, I did so less through self-consciousness than discretion: he knew that I did not share his beliefs, and I would have been afraid of debasing the ritual gestures by letting him think I considered them as mere conventions: but for once, I would have felt no embarrassment in performing them. Between this form of religion and myself, there was no likelihood of misunderstanding. It was not a question of bowing down in front of idols or of adoring a supposed supernatural order, but only of paying homage to the decisive wisdom that a thinker, or the society that created his legend, had evolved twenty-five centuries before and to which my civilization could contribute only by confirming it. (410–411)

Going barefoot could hardly be a casual gesture for Lévi-Strauss; but here, along with ritual cleansing prior to entering the shrine, it seems simply natural. Everything draws him into sympathy and participation. But he marks a line at the physical act of prostration. The line expresses a specific *discretion*, that of a visitor who looks beyond "mere conventions" or going along with appearances to a deeper level of respect based on historical knowledge and cultural comprehension. The anthropologist's authentic bow to Buddhism is a mental one.

Lévi-Strauss is tempted, retrospectively at least, to prostrate himself in the hill temple. Another anthropologist might well have done so. My point



in noticing this line between physical and hermeneutic acts of connection is not to claim that Lévi-Strauss draws it in a place typical of anthropologists. I do want to suggest, however, that a similar line will be drawn somewhere, sometime, in the maintenance of a professional fieldwork habitus. Lévi-Strauss is clearly not one of those Western spiritual travelers who sojourn in Buddhist temples, shaving their heads and wearing saffron robes. And in this he represents the traditional ethnographic norm. One could, of course, imagine a Buddhist anthropologist becoming almost indistinguishable, in both practice and appearance, from other adepts during a period of fieldwork in a temple. And this would be a limit case for the discipline. It would be treated with suspicion, in the absence of other clearly visible signs of professional *discretion* (etymologically: a separation).<sup>23</sup>

Today, in many locations, indigenous people, ethnographers, and tourists all wear T-shirts and shorts. Elsewhere, distinctions of dress are more salient. In highland Guatemala it may be a necessity of decorum, a sign of respect or solidarity, to wear a long skirt or an embroidered shirt in public. But this is hardly cross-dressing. Can, should, an anthropologist wear a turban, yarmulke, *jallabeyya*, *huipil*, or veil? Local conventions vary. But whatever tactics are adopted, they are employed from a position of assumed *cultural discretion*. Moreover, as ethnographers work increasingly in their own societies, the issues I have been discussing in an exoticist frame become confused, the lines of separation less self-evident. Embodied professional practices of the "field"—gendered, raced, sexualized locations and crossovers, forms of self-presentation, and regulated patterns of access, departure, and return—are renegotiated.

### Rerouting the Field

I have tried to identify some of the sedimented practices through and against which newly diverse ethnographic projects struggle for recognition within anthropology. Established practices come under pressure as the range of sites that can be treated ethnographically multiplies (the academic border with "cultural studies") and as differently positioned, politically invested scholars enter the field (the challenge of a "postcolonial anthropology"). The latter development has far-reaching implications for disciplinary reinvention. Fieldwork defined through spatial practices of travel and dwelling, through the disciplined, embodied interactions of partici-

pant-observation, is being rerouted by "indigenous," "postcolonial," "diasporic," "border," "minority," "activist," and "community-based" scholars. The terms overlap, designating complex sites of identification, not discrete identities.

Kirin Narayan (1993) questions the opposition of native and nonnative, insider and outsider anthropologists. This binary, she argues, stems from a discredited, hierarchical colonial structure. Drawing on her own ethnography in different parts of India, where she feels different degrees of affiliation and distance, Narayan shows how "native" researchers are complexly and multiply located vis-à-vis their worksites and interlocutors. Identifications cross-cut, complement, and trouble one another. "Native" anthropologists—like all anthropologists, Narayan argues—"belong to several communities simultaneously (not least of all the community we were born into and the community of professional academics)" (Narayan 1993: 24). Once the structuring opposition between "native" and "outside" anthropologist is displaced, the relations of cultural inside and outside, home and away, same and different that have organized the spatial practices of fieldwork must be rethought. How does the disciplinary injunction that fieldwork involve some sort of "travel"—a practice of physical displacement that defines a site or object of intensive research—constrain the range of practices opened up by Narayan and others?

In Narayan's analysis, fieldwork begins and ends in displacement, enacted across constitutive borders—fraught, amorous edges. There is no simple, undivided, "native" position. Once this is recognized, however, the hybridity she embraces needs specification: What are its limits and conditions of movement? One can be more or less hybrid, native, or "diasporic" (a term that perhaps best captures Narayan's own complex locations) for determinate historical reasons. Indeed, the title of "native" or "indigenous" anthropologist might be retained to designate a person whose research travel leads out and back from a home base, "travel" understood as a detour through a university or other site which provides analytic or comparative perspective on the place of dwelling/research. Here the usual spatialization of home and abroad would be reversed. Moreover, for many fieldworkers neither the university nor the field provides a stable base; rather, both serve as juxtaposed sites in a mobile comparative project. A continuum, not an opposition, separates the explorations, detours, and returns of the indigenous or native scholar from those of the diasporic or postcolonial.<sup>24</sup> Thus, the requirement that an-

thropological fieldwork involve *some* kind of travel need not marginalize those formerly called "natives." The roots and routes, the varieties of "travel," need to be more broadly understood.

Recent work by Mary Helms (1988), David Scott (1989), Amitav Ghosh (1992), Epeli Hau'ofa et al. (1993), Teresia Teaiwa (1993), Ben Finney (1994), and Aihwa Ong (1995), among others, has reinforced a growing awareness of discrepant travel routes—traditions of movement and interconnection not definitively oriented by the "West" and an expanding cultural-economic world system. These routes follow "traditional" and "modern" paths, within and across contemporary transnational and inter-regional circuits. A recognition of these paths makes space for travel (and fieldwork) that does not originate in the metropolises of Euro-America or their outposts. If, as is likely, some form of travel or displacement remains a constituting element in professional fieldwork, reworking the "field" must mean multiplying the range of acceptable routes and practices.

An attention to the varieties of "travel" also helps clarify how, in the past, cleared spaces of scientific work have been constituted through the suppression of cosmopolitan experiences, especially those of the people under study. Generally speaking, the localization of "natives" meant that intensive interactive research was done in spatially delimited fields and not, for example, in hotels or capital cities, on ships, in mission schools or universities, in kitchens and factories, in refugee camps, in diasporic neighborhoods, on pilgrimage buses, or at a variety of cross-cultural sites of encounter.<sup>25</sup> As a Western travel practice, fieldwork was grounded by a historical vision, what Gayatri Spivak calls a "worlding," in which one section of humanity was restless and expansive, the rest rooted and immobile. Indigenous authorities were reduced to native informants. The marginalization of travel practices, those of researchers and hosts, contributed to a *domestication* of fieldwork, an ideal of interactive dwelling which, however temporary, could not be seen as merely passing through. That anthropology's interlocutors often saw things differently did not, until recently, disturb the discipline's self-image.<sup>26</sup>

Alternate forms of travel/fieldwork, whether indigenous or diasporic, grapple with many problems similar to those of conventional research: problems of strangeness, privilege, miscomprehension, stereotyping, and political negotiation of the encounter. Ghosh is especially trenchant on the potentially violent miscomprehensions and stereotypes integral to his re-

search as a *doktor al Hindi* among Muslims. Epeli Hau'ofa speaks for an interconnected "Oceania," but he does so as a Tongan living in Fiji, a location not forgotten by his diverse Islander audiences. At the same time, the routes and encounters of ethnographers such as Ghosh or Hau'ofa are different from those of traditional fieldwork sojourners. Their cultural comparisons need not presuppose a Western/university home, a "central" site of theoretical accumulation. And while their research encounters may involve hierarchical relations, they need not presuppose "white" privilege. Their work may or may not crucially depend on colonial and neocolonial circuits of information, access, and power. For example, Hau'ofa publishes in Tonga and Fiji and wants to articulate an old/new "Oceania." In this he differs from Ghosh, who publishes, crucially though not exclusively, in the West. The language(s) the ethnography uses, the audiences it addresses, the circuits of academic/media prestige it appeals to, may be discrepant from, though seldom unconnected with, the communicative structures of global political economy. A case in point: *A New Oceania*, by Hau'ofa et al., was delivered to me by hand.<sup>27</sup> Published in Suva, the book would not have reached me through my normal reading networks. Can a work centered and routed like this one intervene in Euro-American anthropological contexts? What are the institutional barriers? The power to determine audiences, publications, and translations is very unevenly distributed, as Talal Asad has often reminded us (Asad, 1986).

The oxymoronic term "indigenous anthropologist," coined at the beginning of the ongoing postcolonial/neocolonial recentering of the discipline, is no longer adequate to characterize a wide range of scholars studying in their home societies. Difficult issues arise. How exactly will "home" be defined? If, as I assume, no *inherent* authority can be accorded to "native" ethnographies and histories, what constitutes their *differential* authority? How do they supplement and criticize long-established perspectives? And under what conditions will local knowledge enunciated by locals be recognized as "anthropological knowledge"? What kinds of displacement, comparison, or taking of "distance" are required for family knowledge and folk history to be recognized as serious ethnography or cultural theory by the disciplinary center?

Anthropology potentially includes a cast of diverse dwellers and travelers whose displacement or travel in "fieldwork" differs from the traditional spatial practice of the field. The West itself becomes an object of study

from variously distanced and entangled locations. Going "out" to the field now sometimes means going "back," the ethnography becoming a "note-book of a return to the native land." In the case of a diasporic scholar, the "return" may be to a place never known personally but to which she or he ambivalently, powerfully "belongs." *Returning* to a field will not be the same as *going out* to a field. Different subjective distances and affiliations are at stake.

A growing awareness of these differences has emerged within Euro-American anthropology during recent decades. In an important discussion, David Scott named some of the historical locations constraining an emergent "postcoloniality" in anthropology.

By raising in different ways the problem of "place" and the non-Western anthropologist, both Talal Asad [1982] and Arjun Appadurai [1988b] have suggested that to undermine the asymmetry in anthropological practice many more such anthropologists should study Western societies. This, to be sure, is a step in the right direction inasmuch as it subverts the pervasive notion that the non-Western subject can speak only within the terms of his/her own culture. Moreover, it privileges in some degree the possibility of tacking back and forth between cultural spaces. At the same time, it would seem to fix and repeat the colonially established territorial boundaries within which the postcolonial is encouraged to move: center/periphery—and typically, the center of neocolonial governance and the periphery of origin. European and American anthropologists continue to go where they please, while the postcolonial stays home or else goes West. One wonders whether there might not be a more engaging problematic to be encountered where the postcolonial intellectual from Papua New Guinea goes not to Philadelphia but to Bombay or Kingston or Accra. (Scott, 1989: 80)

Escape from the polarizing historical force field of the "West" is no easy matter, as Scott's subsequent discussion of Ghosh makes clear. But Scott also argues that the cross-cultural "tacking" of anthropologists should not be reduced to movements between centers and peripheries in a world system. Contemporary ethnography, including Scott's own from Jamaica via New York to Sri Lanka, is necessarily "traveling in the West" (Ghosh, quoted by Scott, 82). It is also traveling in and against, through the West.

Ethnography is no longer a normative practice of outsiders visiting/studying insiders but, in Narayan's words, a practice of attending to

“shifting identities in relationship with the people and issues an anthropologist seeks to represent” (Narayan, 1993: 30). How identities are negotiated relationally, in determined historical contexts, is thus a process constituting both the subjects and objects of ethnography. Much emerging work now makes these complex relational processes explicit. Paula Ebron (1994, 1996), for example, conducts research on Mandinka praise-singers both in West Africa and in the United States, where they find appreciative audiences. Her ethnography is multiply located and—as she clearly shows—entangled in the traveling culture circuits of world music and tourism. It also works in tension with a history of dominant Western inventions of Africa—she cites Mudimbe (1988)—and more or less romanticized African American projections formed in reaction to histories of racism. Ebron moves among these intersecting contexts. “Africa” cannot be held “out there.” It is an empowering and problematic part of her own African American tradition as well as a relay—not an origin—in a continuing diasporic history of transits and returns (see Chapter 10). This history implicates her academic ethnography, whose site is the relational negotiation of “subjects in difference,” a space where praise-singers, tourists, and anthropologists claim and negotiate cultural meanings. Her field includes the airports where these travelers cross.

“Indigenous,” “postcolonial,” “diasporic,” or “minority” attachments are frequently at issue in the way anthropological “fields” are negotiated. Scholars such as Rosaldo (1989), Kondo (1990), Behar (1993), and Limón (1994), to cite only a few, define the spatial practices of their fieldwork in terms of a politics of locations, of tactically shifting insides and outsides, affiliations and distances. Their anthropological “distance” is challenged, blurred, relationally reconstructed. Often they express their complex situated knowledges by textual strategies in which the embodied, narrating, traveling scholar/theorist is prominent. But this choice should be seen as a critical intervention against disembodied, neutral authority, not as an emerging norm. There is no narrative form or way of writing inherently suited to a politics of location. Others working within and against a still predominantly Western anthropology may choose to adopt a more impersonal, demystifying, indeed objective rhetoric. David Scott and Talal Asad are strong examples. Their discourses are, nevertheless, openly that of politically committed, situated scholars, not neutral observers. A very wide range of rhetorics and narratives—personal and impersonal, objec-

tive and subjective, embodied and disembodied—are available to the located scholar-traveler. The only tactic excluded, as Donna Haraway has said, is the “God Trick” (Haraway, 1988).



Most of the anthropologists cited in the previous section have done something *like* traditional fieldwork: studying “out” or “down.” This has contributed to their survival, indeed success, within the academy, even as they work to criticize and open it up. The licensing function of having done “real” fieldwork—intensive and displaced from the university—remains strong. Indeed, ethnography that takes place within *diasporic* affiliations may be more easily accepted than research whose attachments, however ambivalent, are indigenous or *native*. (Recall that these locations fall on an overlapping continuum, not on either side of a binary opposition.) Diasporic (dis)locations have travel and distance built into them, usually including metropolitan spaces. Native (re)locations, while they include travel, are centered in a way that makes the metropole and the university peripheral. I have suggested that displacement, Scott’s “tacking” between cultural spaces, remains a constitutive feature of anthropological fieldwork. Can this displacement be extended to include travel to and through the university? Can the university itself be seen as a kind of fieldsite—a place of cultural juxtaposition, estrangement, rite of passage, a place of transit and learning? Mary John (1989) opens such a possibility in her prescient discussion of a compromised, emergent “anthropology in reverse” for postcolonial feminists: a coerced and desired travel “West,” and an unstable coexistence of roles, anthropologist and native informant. How does travel through the university reposition the “native” place, where the anthropologist maintains connections of residence, kinship, or political affiliation that go beyond visiting, however intensive? Angie Chabram explores this repositioning in her provocative sketch of a Chicana/o “oppositional ethnography” (Chabram, 1990). Here, “minority” and “native” trajectories may overlap: rooted in the “community” (however defined) and routed through academia.

When ethnography has primarily served the interests of community memory and mobilization and only secondarily the needs of comparative knowledge or science, it has tended to be relegated to the less prestigious categories of “applied anthropology,” “oral history,” “folklore,” “political journalism,” or “local history.” But as fieldwork becomes differently rooted

and routed in some of the ways I have been tracking, many scholars may take a renewed interest in applied research, oral history, and folklore, stripped now of their sometimes paternalistic traditions. The oral history/community mobilization work of the El Barrio Project at the New York Centro de Estudios Puertorriqueños is a frequently cited example (see Benmayor, 1991; Gordon, 1993). Dara Culhane Speck's *An Error in Judgment* (1987) carefully fuses community memory, historical scholarship, and current political advocacy. Esther Newton's subtle articulation of margins, as loyal lesbian participant-observer, outsider/insider in a predominantly gay male community, produces an exemplary fusion of local history and cultural criticism (Newton, 1993a). Epeli Hau'ofa's research in Tonga is another case in point (as distinct from his exoticist work in Trinidad or his studies in Papua New Guinea, where he was a different kind of "Pacific" outsider). Returning to do research in his native Tonga, Hau'ofa writes in more than one language and style to both analyze and influence local responses to Westernization. He maintains a stylistic distinction between writing for the discipline and writing as political intervention and as satiric fiction (Hau'ofa, 1982). But the discourses are clearly connected in his view, and others might be more inclined than he to blur them.

To do "professional" anthropology, one must maintain connections with university centers and their circuits of publication and sociality. How close must these connections be? How central? When does one begin to lose disciplinary identity at the margins? These questions have always faced scholars working for governments, corporations, activist social organizations, and local communities. They continue to trouble, and discipline, the work of the differently located anthropologists I have been discussing. Moreover, the university itself is not a single site. Though it may have Western roots, it is hybridized and transculturated in non-Western places. Its ties to nation, to "development," to region, to post-, neo-, and anti-colonial politics can make it a significantly different base of anthropological operations, as Hussein Fahim's pioneering collection, *Indigenous Anthropology in Non-Western Countries* (1982), makes clear. In principle at least, universities are sites of comparative theory, of communication and critical argument among scholars. The ethnographic or ethnohistorical interpretations of nonuniversity authorities are seldom recognized as fully scholarly discourse; rather, they tend to be seen as local, amateur knowledge. In anthropology the research that produces such knowledge, however intensive and interactive, is not *fieldwork*.



The disciplinary "Other" who perhaps most epitomizes the border at issue here is the figure of the *local historian*. This supposedly partisan chronicler and keeper of the community's records is even harder to integrate with conventional fieldwork than the emerging figure of the diasporic postcolonial, the oppositional minority scholar, or even the traveling native. Tainted by a presumed immobility and by assumptions of amateurism and boosterism, the local historian, like the activist or culture-worker, lacks the required professional "distance." As we have seen, this distance has been naturalized in spatial practices of the "field," a circumscribed place one enters and leaves. Movement in and out has been considered essential to the interpretive process, the management of depth and discretion, absorption and the "view from afar" (Lévi-Strauss, 1985).

The disciplinary border that keeps locally based authorities in the position of informants is, however, being renegotiated. Where and how the boundary is redrawn—which spatial practices will be accommodated by the evolving tradition of anthropological fieldwork and which will be excluded—remains to be seen. But in this context it may be useful to ask how the legacy of fieldwork-as-travel helps account for an issue raised during recent presidential sessions on diversity at the American Anthropological Association: the fact that North American minorities are entering the field in relatively small numbers. Anthropology has difficulty reconciling goals of analytic distance with the aspirations of Gramscian "organic intellectuals." Has the discipline adequately confronted the problem of doing sanctioned, "real" fieldwork in a community one wants *not* to leave? Departure, taking distance, has long been crucial to the spatial practice of fieldwork. How can the discipline make room for research that is importantly about return, reterritorialization, belonging—attachments that go beyond gaining rapport as a research strategy? Robert Alvarez (1994) provides a revealing discussion of these issues, showing how different kinds of community involvement in the course of research are valued and devalued by the discipline in ways that tend to reproduce a white hegemony.

The definition of "home" is fundamentally at issue here. In local/global situations where displacement appears increasingly to be the norm, how is collective dwelling sustained and reinvented? (See Bammer, 1992.) Binary oppositions between home and abroad, staying and moving, need to be thoroughly questioned (Kaplan, 1994). These oppositions have often been naturalized along lines of gender (female, domestic space versus male travel), class (the active, alienated bourgeoisie versus the stagnant, soulful

poor), and race/culture (modern, rootless Westerners versus traditional, rooted "natives"). The fieldwork injunction to go elsewhere construes "home" as a site of origin, of sameness. Feminist theory and gay/lesbian studies have, perhaps most sharply, showed home to be a site of unrestful differences. Moreover, in the face of global forces that coerce displacement and travel, staying (or making) home can be a political act, a form of resistance. Home is not, in any event, a site of immobility. These few indications, of which much more could be said, should be enough to question anthropological assumptions of fieldwork as travel, going *out* in search of *difference*. To a degree these assumptions continue to apply in practices of "repatriated" fieldwork (Marcus and Fischer, 1986) and of "studying up" (Nadar, 1972). The field remains *somewhere else*, albeit within one's own linguistic or national context.

An unsettling discussion of "home" with reference to anthropological practice is provided by Kamela Visweswaran (1994). She argues that feminist ethnography, part of an ongoing struggle to decolonize anthropology, needs to recognize the "failure" that is inevitably bound up with the project of cross-cultural translation in power-charged situations. Precisely at "those moments when a project is faced with its own impossibility" (98), ethnography can struggle for accountability, a sense of its own positioning. Building on Gayatri Spivak's formulation of every cultural/political subject's "sanctioned ignorances," Visweswaran argues that by openly confronting failure, feminist ethnography discovers both limits and possibilities. Among the latter are critical movements "homeward." In a section titled "Homework, Not Fieldwork," she develops a concept of ethnographic work not based on the home/field dichotomy. "Homework" is not defined as the opposite of exoticist fieldwork; it is not a matter of literally staying home or studying one's own community. "Home," for Visweswaran, is a person's location in determining discourses and institutions—cutting across locations of race, gender, class, sexuality, culture. "Homework" is a critical confrontation with the often invisible processes of learning (the French word *formation* is apt here) that shape us as subjects. Playing on the pedagogical senses of the term, Visweswaran proposes "homework" as a discipline of unlearning as much as of learning. "Home" is a locus of critical struggle that both empowers and limits the subject wherever she or he conducts formal research. By deconstructing the home/field opposition, Visweswaran clears space for unorthodox routings and rootings of ethnographic work.

In a related but not identical vein, Gupta and Ferguson (1997) urge an

anthropology focused on "*shifting locations* rather than *bounded fields*." Theirs is a reformist rather than a deconstructive project. While rejecting the tradition of spatially restricted research, they preserve certain practices long associated with fieldwork. Anthropology still studies "Others" intensively and interactively. It provides, they remind us, one of the few Western academic sites where unfamiliar, marginalized, nonelite peoples are seriously attended to. Long-term immersion, interest in informal knowledge and embodied practices, and an injunction to *listen* are all elements of the fieldwork tradition they value and hope to preserve. Moreover, Gupta and Ferguson's notion of *shifting locations* suggests that even when the ethnographer is positioned as an insider, a "native" in her or his community, some taking of distance and translating differences will be part of the research, analysis, and writing. No one can be an insider to all sectors of a community. How the shifting locations are managed, how affiliation, discretion, and critical perspective are sustained, have been and will remain matters of tactical improvisation as much as of formal methodology. Thus, whatever comes to be recognized as a reformed fieldwork will entail David Scott's "tacking between cultural spaces," though not necessarily or solely along colonial or neocolonial axes of center and periphery.

Moreover, the constitutive displacements need not be between "cultural" spaces, at least not as the term is conventionally defined, in spatial terms. An ethnography focused on *shifting locations* would assume only that the borders negotiated and crossed were salient to a co-constructed project in a specific "contact zone" (Pratt, 1992). This would mean not that the borders in question were invented or unreal, but only that they were not absolute and could be cross-cut by other borders or affiliations also potentially relevant to the project. These other constitutive locations might become central in other historical and political conjunctures or in a differently focused project. One cannot represent "in depth" all salient differences and affinities. For example, a middle-class researcher studying among working people may find class to be a critical location, even if his or her research topic is explicitly focused elsewhere—say, on gender relations in secondary schools. In this case, race might or might not be a site of crucial difference or affinity.

A project will always "succeed" on certain axes and "fail" (in Visweswaran's constitutive sense) on others. Thus, we should not confuse a more or less conscious research strategy of *shifting locations* with *being located* (often antagonistically) in the ethnographic encounter. For an

Indian Hindu working in Egypt, religion may be imposed as a prime differentiating factor, asserting its salience for a research project on agricultural techniques, in spite of the author's desires (Ghosh, 1992). Moreover, the process need not be antagonistic. A student of his or her own community may be located, firmly and lovingly, as "family," thus putting real restrictions on what can be probed and revealed. A gay or lesbian ethnographer may be constrained to highlight or downplay sexual location, depending on the political context of research. Or an anthropologist from Peru may find him- or herself negotiating a national boundary when working in Mexico, but a racial one in the United States. The examples could be multiplied.

None of these locations is optional. They are imposed by historical and political circumstances. And because locations are multiple, conjunctural, and cross-cutting, there can be no guarantee of shared perspective, experience, or solidarity. I build here on a nondismissive critique of identity politics that has been compellingly stated by June Jordan (1985) and developed by many others (for example, Reagon, 1983; Mohanty, 1987). In ethnography, what was previously understood in terms of *rapport*—a kind of achieved friendship, kinship, empathy—now appears as something closer to *alliance building*. The relevant question is less "What fundamentally unites or separates us?" and more "What can we do for one another in the present conjuncture?" What, from our similarities and differences, can we bend together, hook up, articulate? (See Hall, 1986: 52–55; Haraway, 1992: 306–315.) And when identification becomes too close, how can a disarticulation of agendas be managed, in the context of alliance, without resorting to claims to objective distance and tactics of definitive departure? (For a sensitive account of these issues in the context of lesbian ethnography, see Lewin, 1995.)

A stress on shifting locations and tactical affiliations explicitly recognizes ethnography's political dimensions, dimensions that can be hidden by presumptions of scientific neutrality and human rapport. But "political" in what senses? There are no guaranteed or morally unassailable positions. In the present context—a shift from rapport to alliance, from representation to articulation—rigid prescriptions of advocacy have a tendency to emerge. An older politics of neutrality with its goal of ultimate disengagement may simply be reversed—a binary starkly evident in the juxtaposition of eloquent, opposing essays by Roy d'Andrade and Nancy Schepper-Hughes in a 1995 forum of *Current Anthropology*. The place for

a politics of skepticism and critique (not to be confused with dispassion or neutrality), for engaged disloyalty, or for what Richard Handler (1985, following Sapir) calls "destructive analysis" seems endangered. An alliance model leaves little room for work in a politicized situation that pleases none of the contestants. I am not suggesting that such research is superior or more objective. It, too, is partial and located. And it should not be excluded from the range of situated research practices now contending for the name "anthropology."



These are just some of the dilemmas facing anthropological ethnography as its roots and routes, its different patterns of affiliation and displacement, are reworked in late twentieth-century contexts. What remains of *field-work*? What, if anything, is left of the injunction to travel, to get out of the house, to "enter the field," to dwell, interact intensively in a (relatively) unfamiliar context? A research practice defined by "shifting locations," without the prescription of physical displacement, extended face-to-face encounter, could, after all, describe the work of a literary critic, attentive, as many are today, to the politics and cultural contexts of different textual readings. Or, once freed of the notion of a "field" as a spatialized site of research, could an anthropologist investigate the shifting locations of her or his own life? Could "homework" be autobiography?

Here we cross a blurred border that the discipline is struggling to define. Autobiography can, of course, be quite "sociological"; it can move systematically between personal experience and general concerns. A certain degree of autobiography is now widely accepted as relevant to self-critical projects of cultural analysis. But how much? Where is the line to be drawn? When is self-analysis dismissed as "mere" autobiography? (One sometimes hears rather modest amounts of personal revelation in ethnographies described as solipsism or "navel gazing.") Writing an ethnography of one's subjective space as a kind of complex community, a site of shifting locations, could be defended as a valid contribution to anthropological work. It would not, I think, be widely recognized as fully or characteristically *anthropological* in the way that work in an externalized *field* still is. One could hardly count on being awarded a Ph.D., or finding a job in an anthropology department, for autobiographical research. The legacy of the field in anthropology requires, at least, that "first-hand" research involve extended face-to-face interactions with members of a community.

Practices of displacement and encounter still play a defining role. Without these, what are under discussion are not new versions of fieldwork but a range of quite different practices.

In this essay, I have tried to show how definite spatial practices, patterns of dwelling and traveling, have constituted fieldwork in anthropology. I have argued that the disciplining of fieldwork, of its sites, routes, temporalities, and embodied practices, has been critical in maintaining the identity of sociocultural anthropology. Currently contested and under renegotiation, fieldwork remains a mark of disciplinary distinction. The most disputed elements of traditional fieldwork are, perhaps, its injunction to leave home and its inscription within relations of travel that have depended on colonial, race-, class-, and gender-based definitions of center and periphery, cosmopolitan and local. The linked requirement that anthropological fieldwork be intensive and interactive is less controversial, although criteria for measuring "depth" are more debatable than ever. Why not simply purge the discipline's exoticist travel legacy while sustaining its intensive/interactive styles of research? In a utopian mode one might argue for such a solution, and indeed things seem to be moving in this general direction. A radical course is urged by Deborah D'Amico-Samuels, in an essay that anticipates many of the critiques previously referred to. She questions traditional spatial and methodological definitions of the "field," concluding rigorously: "the field is everywhere" (1991: 83). But if the field is everywhere, it is nowhere. We should not be surprised if institutional traditions and interests resist such radical dissolutions of fieldwork. Thus, some forms of travel, of disciplined displacement in and out of one's "community" (seldom a single place, in any event), will probably remain the norm. And this disciplinary "travel" will require at least a serious sojourn in the university. I conclude, provocatively, in this hazardous future tense.

Travel, redefined and broadened, will remain constitutive of fieldwork, at least in the near term. This will be necessary for institutional and material reasons. Anthropology must preserve not only its disciplinary identity but also its credibility with scientific institutions and funding sources. Given a shared genealogy with other natural- and social-science research practices, it is no accident that the field has, at times, been called anthropology's "laboratory." Criteria of objectivity associated with a detached, outside perspective are strongly represented in the academic and government milieus that control resources. Thus, sociocultural anthropol-

ogy will remain under pressure to certify the scientific credentials of an interactive, intersubjective methodology. Researchers will be constrained to take a certain "distance" from the communities they study. Of course, critical distance can be defended without appealing to ultimate grounds of authority in scientific objectivity. At issue is how distance is manifested in research practices. In the past, physically leaving the "field"—to "write up" research results in the presumably more critical, objective, or at least comparative environment of the university—was seen to be an important guarantee of academic independence. As we have seen, this spatialization of "inside" and "outside" locations no longer enjoys the credibility it once did. Will anthropology find ways to take seriously new forms of "field" research that diverge from earlier models of university-centered travel, spatial discontinuity, and ultimate disengagement?

As anthropology moves, haltingly, in postexoticist, postcolonial directions, a diversification of professional norms is under way. The process, accelerated by political and intellectual critiques, is reinforced by material constraints. In many contexts, given falling levels of funding, sociocultural fieldwork will increasingly have to be conducted "on the cheap." For graduate students, relatively expensive long-term sojourns abroad may be out of the question, and even a year of full-time research in a U.S. community can be too expensive. While traditional fieldwork will certainly maintain its prestige, the discipline may come to resemble more closely the "national" anthropologies of many European and non-Western countries, with short, repeated visits the norm and fully supported research years rare. It is important to recall that professional fieldwork in the Malinowskian mold depended materially on the mobilization of funding for a new "scientific" practice (Stocking, 1984a). "Subway ethnography," like Karen McCarthy Brown's (discussed above), will be increasingly common. But even as visiting and "deep hanging out" replace extended co-residence and the tent-in-the-village model, legacies of exoticist fieldwork influence the professional habitus of the "field"—now conceived less as a discrete, other place than as a set of embodied research practices, patterns of discretion, of professional distance, of coming and going.

I have located fieldwork in a long, increasingly contested tradition of Western travel practices. I have suggested, too, that other travel traditions and diasporic routes can help renovate methodologies of displacement, leading to metamorphoses of the "field." "Travel" denotes more or less voluntary practices of leaving familiar ground in search of difference,

wisdom, power, adventure, an altered perspective. These experiences and desires cannot be limited to privileged male Westerners—although that elite has powerfully defined the terms of travel orienting modern anthropology. Travel needs to be rethought in different traditions and historical predicaments. Moreover, when criticizing specific legacies of travel, one should not come to rest in an uncritical localism, the inverse of exoticism. There is truth in the cliché, “Travel broadens.”<sup>28</sup> Of course, the experience offers no guaranteed results. But, often, getting away lets uncontrollable, unexpected things happen (Tsing, 1994). An anthropologist friend, Joan Larcom, once told me, ruefully and gratefully: “Fieldwork gave me some experiences I didn’t think I deserved.” I remember thinking that a discipline requiring this of its adepts must be on to something. Is it possible to validate such experiences of displacement without reference to a mystified, professional “rite of passage”?

Sojourning somewhere else, learning a language, putting oneself in odd situations and trying to figure them out can be a good way to learn something new, simultaneously about oneself and about the people and places one visits. This commonplace truth has long encouraged people to engage with cultures beyond their own. It underlies what still seems most valuable in the linked/distinct traditions of travel and ethnography. Intensive fieldwork does not produce privileged or complete understandings. Nor does the cultural knowledge of indigenous authorities, of “insiders.” We are differently situated as dwellers and travelers in our cleared “fields” of knowledge. Is this multiplicity of locations merely another symptom of postmodern fragmentation? Can it be collectively fashioned into something more substantial? Can anthropology be reinvented as a forum for variously routed fieldworks—a site where different contextual knowledges engage in critical dialogue and respectful polemic? Can anthropology foster a critique of cultural dominance which extends to its own protocols of research? The answer is unclear: powerful, newly flexible, centralizing forces remain. The legacies of the “field” are strong in the discipline and deeply, perhaps productively, ambiguous. I have focused on some defining spatial practices that must be turned to new ends if a multiply-centered anthropology is to emerge.