Anthropological Locations

Boundaries and Grounds of a Field Science

EDITED BY
Akhil Gupta and James Ferguson

UNIVERSITY OF CALIFORNIA PRESS
Berkeley Los Angeles London

ONE

Discipline and Practice: "The Field" as Site, Method, and Location in Anthropology

Akhil Gupta and James Ferguson

I. INTRODUCTION

The practice of fieldwork, together with its associated genre, ethnography, has perhaps never been as central to the discipline of anthropology[1] as it is today, in terms of both intellectual principles and professional practices. Intellectually, ethnography has long ceased to be conceived of as "mere description," raw material for a natural science of human behavior. Whether via the literary turn (from "thick description" to "writing culture") or the historic one (political economy and the turn to regional social history), mainstream social/cultural anthropology as practiced in leading departments in the United States and the United Kingdom[2] has come to view ethnographic explication as a worthy and sufficient intellectual project in its own right. Indeed, it is striking that the generalist and comparativist theorists who dominated anthropology at midcentury (e.g., Radcliffe-Brown, Leslie White, and George Murdock) seem in the process of being mnemonically pruned from the anthropological family tree, while the work of those remembered as great fieldworkers (Malinowski, Boas, Evans-Pritchard, Leenhardt, etc.) continues to be much more widely discussed.

In terms of professional socialization and training, too, ethnographic fieldwork is at the core of what Stocking has called anthropology's fundamental "methodological values"—"the taken-for-granted, pretheoretical notions of what it is to do anthropology (and to be an anthropologist)" (1992a: 282). As all graduate students in social/cultural anthropology know, it is fieldwork that makes one a "real anthropologist," and truly anthropological knowledge is widely understood to be "based" (as we say) on fieldwork. Indeed, we would suggest that the single most significant factor determining whether a piece of research will be accepted as (that magical word) "anthropological" is the extent to which it depends on experience "in the field."

Yet this idea of "the field," although central to our intellectual and professional identities, remains a largely unexamined one in contemporary anthropology. The concept of culture has been vigorously critiqued and dissected in recent years (e.g., Wagner 1981; Clifford 1988; Rosaldo 1989a; Fox, ed., 1991); ethnography as a genre of writing has been made visible and critically analyzed (Clifford and Marcus 1986; Geertz 1988); the dialogic encounters that constitute fieldwork experience have been explored (Crapanzano 1980; Rabinow 1977; Dumont 1978; Tedlock 1983); even the peculiar textual genre of fieldnotes has been subjected to reflection and analysis (Sanjek 1990). But what of "the field" itself, the place where the distinctive work of "fieldwork" may be done, that taken-for-granted space in which an "Other" culture or society lies waiting to be observed and written? This mysterious space—not the "what" of anthropology but the "where"—has been left to common sense, beyond and below the threshold of reflexivity.

It is astonishing, but true, that most leading departments of anthropology in the United States provide no formal (and very little informal) training in fieldwork methods—as few as 20 percent of departments, according to one survey.[3] It is also true that most anthropological training programs provide little guidance in, and almost no critical reflection on, the selection of fieldwork sites and the considerations that deem some places but not others as suitable for the role of "the field." It is as if the mystique of fieldwork were too great in anthropology for the profession even to permit such obvious and practical issues to be seriously discussed, let alone to allow the idea of "the field" itself to be subjected to scrutiny and reflection.

In turning a critical eye to such questions, our aim is not to breach what amounts to a collectively sanctioned silence simply for the pleasure of upsetting traditions. Rather, our effort to open up this subject is motivated by two specific imperatives.

The first imperative follows from the way the idea of "the field" functions in the micropolitical academic practices through which anthropological work is distinguished from work in related disciplines such as history, sociology, political science, literature and literary criticism, religious studies, and (especially) cultural studies. The difference between anthropology and these other disciplines, it would be widely agreed, lies less in the topics studied (which, after all, overlap substantially) than in the distinctive method anthropologists employ, namely fieldwork based on participant observation. In other words, our difference from other specialists in academic institutions is constructed not just on the premise that we are specialists in difference, but on a specific methodology for uncovering or understanding that difference. Fieldwork thus helps define anthropology as a discipline in both senses of the word, constructing a space of possibilities while at the same time drawing the lines that confine that space. Far from being a mere research technique, fieldwork has become "the basic constituting experience both of anthropologists and of anthropological knowledge" (Stocking 1992a: 282).

Since fieldwork is increasingly the single constituent element of the anthropological tradition used to mark and police the boundaries of the discipline, it is impossible to rethink those boundaries or rework their contents without confronting the idea of "the field." "The field" of anthropology and "the field" of "fieldwork" are thus politically and epistemologically intertwined; to think critically about one requires a readiness to question the other. Exploring the possibilities and limitations of the idea of "the field" thus carries with it the opportunity—or, depending on one's point of view, the risk—of opening to question the meaning of our own professional and intellectual identities as anthropologists.

The second imperative for beginning to discuss the idea of "the field" in anthropology follows from a now widely expressed doubt about the adequacy of traditional ethnographic methods and concepts to the intellectual and political challenges of the contemporary postcolonial world. Concern about the lack of fit between the problems raised by a mobile, changing, globalizing world, on the one hand, and the resources provided by a method originally developed for studying supposedly small-scale societies, on the other, has of course been evident in anthropological circles for some time (see, for instance, Hymes 1972; Asad 1973). In recent years, however, questioning of the traditional fieldwork ideal has become both more widespread and more far-reaching. Some critics have pointed to problems in the construction of ethnographic texts (Clifford and Marcus 1986), some to the structures and practices through which relationships are established between ethnographers and their "informants" in the field (Crapanzano 1980; Dumont 1978; cf. Harrison, ed., 1991). Others have suggested that the problem lies as much in the fact that the world being described by ethnographers has changed dramatically without a corresponding shift in disciplinary practices since "fieldwork" became hegemonic in anthropology. Appadurai has posed the problem in the following terms:

> As groups migrate, regroup in new locations, reconstruct their histories, and reconfigure their ethnic "projects," the *ethno* in ethnography takes on a slippery, nonlocalized quality, to which the descriptive practices of anthropology will have to respond. The landscapes of group identity—the ethnoscapes—around the world are no longer familiar anthropological objects, insofar as groups are no longer tightly territorialized, spatially bounded, historically self-conscious, or culturally homogeneous. . . . The task of ethnography now becomes the unraveling of a conundrum: what is the nature of locality, as a lived experience, in a globalized, deterritorialized world? (Appadurai 1991: 191, 196)[4]

In what follows, we will further explore the challenge of coming to terms with the changed context of ethnographic work. For now, it is sufficient to note a certain contradiction. On the one hand, anthropology appears determined to give up its old ideas of territorially fixed communities and stable, localized cultures, and to apprehend an interconnected world in which people, objects, and ideas are rapidly shifting and refuse to stay in place. At the same time, though, in a defensive response to challenges to its "turf" from other disciplines, anthropology has come to lean more heavily than ever on a methodological commitment to spend long periods in one localized setting. What are we to do with a discipline that loudly rejects received ideas of "the local," even while ever more firmly insisting on a method that takes it for granted? A productive rethinking of such eminently practical problems in anthropological methodology, we suggest, will require a thoroughgoing reevaluation of the idea of the anthropological "field" itself, as well as the privileged status it occupies in the construction of anthropological knowledge.

This book therefore explores the idea of "the field" at each of the two levels described above. Some of the authors investigate how "the field" came to be part of the commonsense and professional practice of anthropology, and view this development in the contexts both of wider social and political developments and of the academy's micropolitics. Other authors, researchers whose own work stretches the conventional boundaries of "fieldwork," reflect on how the idea of "the field" has bounded and normalized the practice of anthropology—how it enables certain kinds of knowledge while blocking off others, authorizes some objects of study and methods of analysis while excluding others; how, in short, the idea of "the field" helps to define and patrol the boundaries of what is often knowingly referred to as "real anthropology."

In the remaining sections of this chapter, we develop some general observations about how the idea of "the field" has been historically constructed and constituted in anthropology (Part II) and trace some key effects and consequences of this dominant concept of "the field" for professional and intellectual practices (Part III). We want not only to describe the configurations of field and discipline that have prevailed in the past but also to help rework these configurations to meet the needs of the present and the future better. "The field" is a (arguably *the*) central component of the anthropological tradition, to be sure; but anthropology also teaches that traditions are always reworked and even reinvented as needed. With this in mind, we search (in Part IV) for intellectual resources and alternative disciplinary practices that might aid in such a reconstruction of tradition, which we provisionally locate both in certain forgotten and devalued elements of the anthropological past and in various marginalized sites on the geographical and disciplinary peripheries of anthropology. Finally, in Part V, we propose a reformulation of the anthropological fieldwork tradition that would decenter and defetishize the concept of "the field," while developing methodological and epistemological strategies that foreground questions of location, intervention, and the construction of situated knowledges.

Whether anthropology *ought* to have a unique or distinctive approach that sets it apart from other disciplines is not a question of great intrinsic interest to us. Certainly, there are many more interesting questions to ask about any given piece of work than whether or not it "belongs" within anthropology. But we accept James Clifford's point (chapter 10 in this book) that as long as the current configuration of disciplines obtains,[5] the slot labeled "anthropology" will be obliged, in one way or another, to distinguish and justify itself. We agree, too, that the anthropological "trademark" of fieldwork seems certain to be central to any such disciplinary strategies of self-definition and legitimation, at least in the near future. With this in mind, it seems most useful to us to attempt to redefine the fieldwork "trademark" not with a time-honored commitment to the *local* but with an attentiveness to social, cultural, and political *location* and a willingness to work self-consciously at shifting or realigning our own location while building epistemological and political links with other locations (an idea that we develop in Part V). Such "location-work," we suggest, is central to many of the most innovative reconceptualizations of anthropological fieldwork practices in recent years, some of which are illustrated in this book. The fact that such work fits only uneasily within the traditional disciplinary bounds of a "real anthropology" defined by "real fieldwork" has caused a good many recent tensions within the discipline. A serious consideration of what the conventional anthropological commitment to "field" and "fieldwork" entails, and a willingness to rethink how such a commitment might be conceptualized, could contribute to a better understanding of such tensions and ways in which they might be addressed constructively.

II. GENEALOGY OF A "FIELD SCIENCE"

Anyone who has done fieldwork, or studied the phenomenon, knows that one does not just wander onto a "field site" to engage in a deep and meaningful relationship with "the natives." "The field" is a clearing whose deceptive transparency obscures the complex processes that go into constructing it. In fact, it is a highly overdetermined setting for the discovery of difference. To begin with, it is the prior conceptual segmentation of the world into different cultures, areas, and sites that makes the enterprise of fieldwork possible. How does this territorialization take place? Through what conventions and inherited assumptions is it possible for the world to appear, through the anthropological lens, as an array of field sites?

Natural History and the Malinowskian "Field"

One place to begin thinking about these questions is to note how the idea of "the field" entered the discipline. We do not aim here to construct a full intellectual history of the idea of "the field," nor do we possess the historiographical expertise to do so, though scholars of the history of anthropology such as George Stocking (ed., 1983, 1991, 1992a), Henrika Kuklick (1991, and chapter 2 of this book), and Joan Vincent (1990) have already made important contributions toward that task. Instead, we wish to raise, in a genealogical spirit, a more restricted and focused set of questions about the key relationships that led to the constitution of anthropology as a field of knowledge that depends on fieldwork as the distinctive mode of gathering knowledge.[6]

In this spirit, it is interesting to note that the term *fieldwork*, apparently introduced into anthropology by the former zoologist A. C. Haddon, was derived from the discourse of field naturalists (Stocking 1992a; Kuklick, chapter 2). As Stocking observes, Haddon conceived his first fieldwork in the Torres Straits squarely within the terms of natural history: "to study the fauna, the structure, and the mode of formation of coral reefs" (1992a: 21). Indeed, Kuklick (chapter 2) vividly demonstrates that the anthropological "discovery" of fieldwork needs to be set in the context of a more general set of transformations in the late nineteenth- and early twentieth-century practices of *all* naturalists. Like other "field sciences," such as zoology, botany, and geology, anthropology at the start of the century found both its distinctive object and its distinctive method in "the detailed study of limited areas" (Kuklick, chapter 2; cf. Stocking 1992a). Anthropology's origin as a naturalistic science of the early human is therefore closely tied to the eventual role of fieldwork as its dominant disciplinary practice. To do fieldwork was, in the beginning, to engage in a branch of natural history; the object to be studied, both intensively and in a limited area, was primitive humanity in its natural state.[7]

Many early twentieth-century fieldworkers explicitly recognized, of course, that their subjects were in fact *not* living in a pristine, "natural" condition; so-called "salvage anthropology" was a self-conscious attempt to reconstruct such a state from the observation and questioning of natives living under the patently "unnatural" conditions of a postconquest colonial world. David Tomas (1991) shows, for example, how Radcliffe-Brown complained that the informants he met on a penal settlement (established by the colonial government in the Andaman Islands to imprison those who rose against it in the Great Indian Mutiny of 1857) no longer remembered "the things of the old time"; he therefore tried to interview others who "do not know a single word of any language but their own" (in Tomas 1991: 96). His eventual plan was to go to the Nicobars where the data were less likely to be contaminated by the natives' previous contact with white people like himself (Tomas 1991: 95–96). The early Boasians in the United States faced similar difficulties in seeking to build comprehensive descriptions of peoples and societies that had been substantially decimated by conquest, genocide, and disease.

With the Malinowskian revolution in fieldwork,[8] anthropological naturalism came to be asserted in an even stronger form. Through an active forgetting of conquest and colonialism, fieldworkers increasingly claimed not simply to *reconstruct* the natural state of the primitive, but to *observe* it directly. Thus did social anthropology become defined as "the study of small-scale society—ahistorical, *ethno*-graphic, and comparative," with extended participant observation its distinctive method (Vincent 1991: 55). Yet it is worth remembering just how late a development this was. It is not only that, as Kuklick shows (chapter 2), the gentlemen-scholars of the nineteenth century scorned the idea of actually going to "the field" (regarding the "collection" of data as a task for unskilled and low-status workers—in some places, for slaves). For even *after* the Trobriand Islanders provided anthropology with its mythic fieldwork charter, many of Malinowski's own students (according to George Stocking, personal communication, 10 November 1993) did library dissertations before ever going into "the field," as did their Boasian contemporaries (and, indeed, Malinowski himself). As Stocking has shown, it was Malinowski's ambition and "entrepreneurial talent," rather than simply the intrinsic intellectual merits of his program, that enabled him to secure the support of the Rockefeller Foundation for his vision of anthropology, which only then (i.e., after 1930) enabled him to institutionalize his perspective. (For example, all Rockefeller-funded fieldworkers of the International African Institute were required to spend a year in Malinowski's seminar [Stocking 1992a]). Malinowski's success in normalizing "his method" may have owed more to his institutional skills and to the leaving of progeny who continued his legacy than to anything inherent in extended participant observation itself (cf. Kuklick 1991; Vincent 1990).

A key result of the Malinowskian triumph, however, was that a naturalistic ideal that had been dismissed as impractical in the actual fieldwork of such founding fathers as Radcliffe-Brown and Boas came to be retrospectively asserted as the discipline's foundational methodological strategy. Fieldwork in sociocultural anthropology in this way came to share with fields such as primatology the requirement that its subjects be directly observed in their natural surroundings (see Haraway 1989). Those living outside their native state (for example, Native Americans working in towns; Aborigines employed on ranches; or, in Radcliffe-Brown's case cited above, prisoners forcibly held in a penal settlement) came to be considered less suitable anthropological objects because they were outside "the field," just as zoological studies of animals in captivity came to be considered inferior to those conducted on animals in the wild. The naturalistic genre of ethnography was an attempt to recreate that natural state textually, just as the dioramas painstakingly constructed in natural history museums aimed not only to describe but also to recreate the natural surroundings of primates and other animals (Haraway 1989: 26–58). Thus, when Ulf Hannerz (1986) complained that ethnography was still obsessed with "the most *other* of others," he was critiquing a long-standing ethnographic attitude that those most Other, and most isolated from "ourselves," are those most authentically rooted in their "natural" settings (cf. Malkki 1992).

This conception, of course, was and is undergirded by the metaphor of the "field" to denote the sites where anthropologists do their research. The word *field* connotes a place set apart from the urban—opposed not so much to the transnational metropolises of late capitalism as to the industrial cities of the era of competitive capitalism, as befits the word's period of origin (Fox 1991b). Going to the "field" suggests a trip to a place that is agrarian, pastoral, or maybe even "wild"; it implies a place that is perhaps cultivated (a site of culture), but that certainly does not stray too far from nature. What stands metaphorically opposed to work in the field is work in industrial places: in labs, in offices, in factories, in urban settings—in short, in civilized spaces that have lost their connection with nature. As a metaphor we work by, "the field" thus reveals many of the unspoken assumptions of anthropology. This is not, of course, to say that anthropologists do not work in industrial or urban settings, or that they do not call those sites "fields"—we are not being literalist, merely noting that it is not just coincidence that pastoral and agrarian metaphors shepherd anthropologists in their daily tasks.[9]

Areas and Sites

Anthropology, more than perhaps any other discipline, is a body of knowledge constructed on regional specialization, and it is within regionally circumscribed epistemic communities that many of the discipline's key concepts and debates have been developed (Fardon 1990; Appadurai 1988b). More than comparativists in other fields—political science, sociology, literature, history, law, religion, and business—anthropologists combine language learning and regional scholarship with long-term residence in "the field." Regional expertise is thus built into the anthropological project, constituting the other face of a discipline (at least implicitly) predicated on cultural comparison (Marcus and Fischer 1986). As we have argued elsewhere (Gupta and Ferguson 1992, 1997), it is precisely the naturalization of cultural difference as inhering in different geographical locales that makes anthropology such a regional science. From this, too, there follows the built-in necessity of travel: one can only encounter difference by going elsewhere, by going to "the field."

It is possible to situate "the field" more precisely as a site constructed through the shifting entanglements of anthropological notions of "culture areas," the institutional politics of "area studies," and the global order of nation-states. The notion of culture areas, supplemented by ideas such as peoplehood and ethnicity (e.g., "the Kurds"), religion (e.g., "the Islamic world"), language (e.g., "Bantu-speaking Africa"), and race (e.g., "Melanesia" [see Thomas 1989a] or "Black Africa" [see Amory chapter 5]), attempted to relate a set of societies with common traits to each other. Thus the Mediterranean with its honor-and-shame complex constituted one culture area (Herzfeld 1987; Passaro chapter 8), while South Asia with the institution of caste hierarchy formed another (Appadurai 1988b), and Polynesia with its centralized chiefdoms constituted a third (Thomas 1989a). Although we anthropologists devote far less attention today to mapping "culture regions" than we used to (e.g., Wissler 1923; Murdock 1967; but cf. Burton et al. 1996), the culture area remains a central disciplinary concept that implicitly structures the way in which we make connections between the particular groups of people we study and the groups that other ethnographers study (cf. Fardon 1990; Thomas 1989a).[10]

However, and this is where issues become more complicated, ideas about culture areas in the anthropological literature are refracted, altered, and sometimes undermined by the institutional mechanisms that provide the intellectual legitimacy and financial support for doing fieldwork. To take but one example, the setting up of area studies centers in American universities has long been underwritten by the U.S. government. The definition of areas, the emphasis placed on various activities, and the importance of particular topics as research priorities have mostly been thinly disguised (if that) projections of the state's strategic and geopolitical priorities. As the state's interests shift, so do funding priorities and the definition of areas themselves. A few years ago, for instance, there was an effort to carve out a new area, "Inner Asia," which would be distinct from Eastern Europe and Soviet studies on the one hand, and the Middle East and China on the other. The timing of this development remains mysterious unless one understands the concern with the war in Afghanistan and the fear of the possible ascendance of "Islamic republics" in the regions adjacent to what was then the Soviet Union.

As the institutional mechanisms that define areas, fund research, and support scholarship change, they intersect in complicated ways with changing ideas about "culture areas" to produce "fields" that are available for research. Thus, no major funding agency supports research on "the Mediterranean" or "the Caribbean." Some parts of the Mediterranean culture area are funded by European area studies and the others by Middle Eastern area studies. The more culturally exotic and geostrategically embattled parts thus become proper "anthropological" field sites, whereas Western Europe (which, besides having "less culture" [cf. Rosaldo 1988], is part of NATO) is a less appropriate "field," as the many Europeanists who struggle to find jobs in anthropology departments can attest.[11]

Similarly, anthropological ideas about culture areas and geographical specializations have been transformed by their encounter with the rude realities of decolonization. For instance, anthropologists working in Africa today normally construct their regional specializations in national terms that would have made no sense prior to the 1960s. Thus Victor Turner was not, as he would be styled today, a "Zambianist" but an "Africanist"; his *Schism and Continuity in an African Society* was "A Study of Ndembu Village Life," and the reader would have to comb the text with some care to find out that the study was in fact conducted in what was then northern Rhodesia. Evans-Pritchard's research freely crossed between the Belgian Congo (Azande), the Anglo-Egyptian Sudan (Nuer), and British East Africa (Luo); his regional specialization was not defined by such political territorializations. Yet just as Evans-Pritchard's work was enabled by the brute fact of colonial conquest,[12] so, too, the field sites in which contemporary anthropologists work are shaped by the geopolitics of the postcolonial, imperial world. Decolonization has transformed field sites not merely by making it difficult, if not impossible, to move across national borders, but by affecting a whole host of mechanisms, from the location of archives to the granting of visas and research clearance. The institutions that organized knowledge along colonial lines have yielded to ones that organize it along national ones.[13]

A "good" field site is made, however, not only by considerations of funding and clearance, but by its suitability for addressing issues and debates that matter to the discipline. As Jane Collier shows (chapter 6), the idea of substantive "subfields" such as "legal anthropology," "economic anthropology," "psychological anthropology," and so on was until recently a key device through which such issues and debates were constituted. The problematics and conventions of such subfields helped to shape not only the topic of investigation, but also the conception of the field site itself, in a number of ways. First, as we have noted, culture areas have long been linked to subject areas; thus India, with its ideologies of caste and purity, was long taken to be an especially good site for an anthropologist of religion (Appadurai 1988b), and Africa (with its segmentary lineages) was thought ideal for the political anthropologist, just as Melanesia (with its elaborate systems of exchange) invited economic anthropologists (cf. Fardon 1990). But subfields have also carried more specific assumptions about fieldwork and methodology. The "fieldwork" of a legal anthropologist, for instance, might be expected to include the examination of written court records, while that of a psychological anthropologist working in the same area likely would not; in this manner, different subfields could construct the site to be studied in different ways. As Collier shows, however, the very idea of coherent "subfields" has broken down in recent years. The growing willingness to question received ideas of "field" and "fieldwork" may well be related to the recent decline of the well-defined subfields that once helped to define and bound field sites.

Field sites thus end up being defined by the crosshatched intersection of visa and clearance procedures,[14] the interests of funding agencies, and intellectual debates within the discipline and its subfields. Once defined in this way, field sites appear simply as a natural array of choices facing graduate students preparing for professional careers. The question becomes one of choosing an appropriate site, that is, choosing a place where intellectual interests, personal predilections, and career outcomes can most happily intersect. This is to be expected. What is more surprising is the recurrence of anecdotes in which experienced fieldworkers relate how they "stumbled" on to their field sites entirely "by chance."[15] Just as the culturally sanctioned discourse of "hard work" and "enterprise" enables the structurally patterned outcomes of career choice in competitive capitalism to disappear from view, so do the repeated narratives of discovering field sites "by chance" prevent any systematic inquiry into how those field sites came to be good places for doing fieldwork in the first instance. The very significant premises and assumptions built into the anthropological idea of "the field" are in this way protected from critical scrutiny, even as they are smuggled into the discipline's most central practices of induction, socialization, and professional reproduction.

III. IMPLICATIONS OF AN ARCHETYPE

As Stocking has pointed out (1992a: 59), the classical Malinowskian image of fieldwork (the lone, white, male fieldworker living for a year or more among the native villagers) functions as an *archetype* for normal anthropological practice.[16] Because an archetype is never a concrete and specific set of rules, this ideal of fieldwork need not carry with it any specific set of prescriptions; its link to practice is looser than this, and more complex. Since the archetypal image is today often invoked ironically and parodically, it can easily be made to appear an anachronism—a caricature that everyone knows, but nobody really takes seriously anymore. Yet such easy dismissals may be premature. After all, archetypes function not by claiming to be accurate, literal descriptions of things as they are, but by offering a compelling glimpse of things as they should be, at their purest and most essential. In the contemporary United States, for instance, the image of the so-called "all-American" look (healthy, wholesome, and white) has the power of an archetype. Americans know, of course, that most Americans do not look like this. If asked, most would surely say that dark-skinned Americans are every bit as "American" as light-skinned ones. Yet at a more fundamental and spontaneous level, when people think of "an American"—a "*real* American"—it is the "all-American" image that is likely to come to mind. Such archetypes operate ideologically in a way that is peculiarly hard to pin down; their effects are simultaneously ineffable and pervasive. Yet it is impossible to understand the full implications of the anthropological concept of "the field" without taking account of the deep-seated images of the "real fieldworker," the "real anthropologist," that constitute a significant part of the "common sense" (in the Gramscian usage of the term) of the discipline.

In sketching some of the key consequences of the construction of the field of anthropology through the practice of fieldwork, we focus on three themes in particular: first, the radical separation of "the field" from "home," and the related creation of a hierarchy of purity of field sites; second, the valorization of certain kinds of knowledge to the exclusion of other kinds; and third, the construction of a normative anthropological subject, an anthropological "self" against which anthropology sets its "Others." We emphasize, again, that these are not simply historical associations, but archetypal ones that subtly but powerfully construct the very idea of what anthropology is. We will argue that even *ideas* about "the field" that are explicitly disavowed by contemporary anthropologists in intellectual terms continue to be deeply embedded in our professional *practices.*

"Field" and "Home"

The distinction between "the field" and "home" rests on their spatial separation.[17] This separation is manifested in two central anthropological contrasts. The first differentiates the site where data are collected from the place where analysis is conducted and the ethnography is "written up." To do ethnographic work is thus to do two distinct types of writing. One kind is done "in the field." These "fieldnotes" are close to experience, textually fragmentary, consisting of detailed "raw" documentation of interviews and observations as well as spontaneous subjective reactions (Sanjek 1990). The other sort, done "at home," is reflective, polished, theoretical, intertextual, a textual whole—this is the writing of ethnographic papers and monographs. The former is done in isolation, sometimes on primitive equipment, in difficult conditions, with people talking or peering over one's shoulder; writing at "home" is done in the academy, in libraries or studies, surrounded by other texts, in the midst of theoretical conversation with others of one's kind. Moreover, the two forms of activity are not only distinct, but sequential: one commonly "writes up" after coming back from "the field." Temporal succession therefore traces the natural sequence of sites that completes a spatial journey into Otherness.

The second place the sharp contrast between "field" and "home" is expressed is in the standard anthropological tropes of entry into and exit from "the field." Stories of entry and exit usually appear on the margins of texts, providing the narrative with uncertainty and expectation at the beginning and closure at the end. According to Mary Louise Pratt (1986), the function of narratives of entry and exit is to authenticate and authorize the material

that follows, most of which used to be written from the standpoint of an objective, distanced, observer.[18] Such stories also form a key piece of the informal lore of fieldwork that is so much a part of socialization into the discipline. Colonial-style heroic tales of adventurers battling the fierce tropics are, of course, out of favor nowadays, and the usual cliches of anthropological arrival are perhaps more often invoked today in a self-consciously ironic mode. But what needs to be emphasized is that *all* tropes of entry and exit, however playful, parodic, or self-conscious, may still function to construct the difference between "the field" and "home." The image of arriving in "another world" whose difference is enacted in the descriptions that follow, tends to minimize, if not make invisible, the multiple ways in which colonialism, imperialism, missionization, multinational capital, global cultural flows, and travel bind these spaces together. Again, most anthropologists today recognize this, but even as we reject ideas of isolated peoples living in separate worlds, the tropes of entry and exit and the idea of a separation of "fieldwork" from "writing up" continue to structure most contemporary ethnography.[19]

The very distinction between "field" and "home" leads directly to what we call a *hierarchy of purity* of field sites. After all, if "the field" is most appropriately a place that is "not home," then some places will necessarily be *more* "not home" than others, and hence more appropriate, more "fieldlike." All ethnographic research is thus done "in the field," but some "fields" are more equal than others—specifically, those that are understood to be distant, exotic, and strange. Here the parallel is striking with the older conception of anthropology as a field science, in which some sites offered better approximations of "the natural state" than others and were therefore preferred. Although anthropologists no longer think in terms of natural or undisturbed states, it remains evident that what many would deny in theory continues to be true in practice: some places are much more "anthropological" than others (e.g., Africa more than Europe, southern Europe more than northern Europe, villages more than cities) according to the degree of Otherness from an archetypal anthropological "home."

Largely because the idea of "the field" remains uninterrogated, such hierarchies of field sites live on in our professional practices. Among anthropologists who have done fieldwork, for instance, some are still understood to have done what is knowingly referred to as "real fieldwork"—that is, worked for a long time in an isolated area, with people who speak a non-European language, lived in "a community," preferably small, in authentic, "local" dwellings—while others have less pure field sites and thus are less fully anthropological. Anyone who doubts that such thinking continues to operate in the discipline should take a close look at anthropological job searches, where the question of who has or has not done "real fieldwork" (presumably in the "real field") is often decisive. Indeed, it is worth noting that the geographical categories by which such searches usually proceed[20] rule out from the start many outstanding job candidates who do not work, say, "in Africa" or "in Mesoamerica," but on such things as whiteness in the U.S. (Frankenberg 1993b) or on the practices of transnational "development" agencies (Escobar 1994). That anthropology's archetypal "home" (the dominant, majority culture of the contemporary United States) is still considered only a poor approximation of "the field" is shown perhaps most clearly by the fact that when job advertisements offer a position for a "North Americanist," what is called for is nearly always a specialist on ethnic and racial minorities, most often on those who occupy a special place in white North American "imperialist nostalgia" (Rosaldo 1989b), namely Native Americans.[21]

A very large number of anthropologists, of course, do work in the United States, and by no means all of them focus on Native Americans or minorities. Yet working in the United States has long had a low status in the field, and even a certain stigma attached to it. Exotic fieldwork, Kuklick points out (chapter 2), has been a "gatekeeper" in Anglo-American anthropology. Since it requires external funding, not everyone can do it, and those who can are therefore marked as a select group. Indeed, one of us was actually told in graduate school that fieldwork in the United States was "for people who don't get grants." Such prejudices may have diminished in recent years, but they have hardly disappeared. The fact that today more high-status American anthropologists (the ones who *do* get the grants) are working "at home" is significant, but it should also be noted that they are mostly anthropologists whose careers are already established and who take on second field sites closer to home (a pattern often remarked to fit well with considerations both of tenure and of child rearing). It remains extremely difficult for students who do their dissertation fieldwork entirely within the United States to get jobs at top departments. A quick survey of ten top American departments of anthropology reveals only 8 anthropologists (out of a total of 189) who claim a primary specialization in the nonnative United States. Only 1 of these 8 had received a Ph.D. within the last fifteen years.[22] (See also the personal testimony of Passaro and Weston in chapters 8 and 9.)

In pointing out the existence of such a hierarchy of field sites, we do not mean to suggest that anthropologists ought to give up working "abroad," or that the only fieldwork worth doing is "at home." On the contrary, many of the reasons that have led anthropologists to leave their homes for faraway field sites seem to us excellent ones. If nothing else, the anthropological insistence that "out of the way places" matter (Tsing 1993, 1994b) has done much to counter the Eurocentric and parochial understandings of culture and society that dominate most Western universities. What we object to is not the leaving of "home," but the uncritical mapping of "difference" onto exotic sites (as if "home," however defined, were not also a site of difference [cf. chapters 8 through 10; cf. also Greenhouse 1985]) as well as the implicit presumption that "Otherness" means difference from an unmarked, white Western "self" (which has the effect of constructing the anthropologist as a very particular sort of subject, as we discuss below). The issue, then, is not whether anthropologists should work "abroad" or "at home," but precisely the radical separation between the two that is taken for granted as much by those who would insist that anthropology remain "at home" as by those who would restrict its mission to fieldwork "abroad."

Fieldwork-Based Knowledge

A second consequence of anthropology's emphasis on "the field" is that it enables certain forms of knowledge, but blocks off others. With the idea that knowledge derived from experience in "the field" is privileged comes a foregrounding of face-to-face relations of community, while other, less localized relations disappear from view (see Thomas 1991). Ethnographic knowledge is heavily dependent on the presence and experience of the fieldworker. More than any other discipline, the truths of anthropology are grounded in the experience of the participant observer. This experience yields much that is valuable, but also severely circumscribes the knowledge obtained. Why, for instance, has there been so little anthropological work on the translocal aspects of transnational corporations and multilateral institutions (cf. Nash 1979; Ghosh 1994)? Why are there so few ethnographic treatments of the mass media?[23] More generally, why do translocal phenomena of various kinds evade classical methods of participant observation?

Though anthropologists often picture themselves as specialists in "the local," we suggest that the idea of locality in anthropology is not well thought out. Clearly geographical contiguity and boundedness are insufficient to define a "local community"; otherwise, high-rise buildings in urban metropolises would automatically qualify, and office-dwellers crammed together for large parts of the day would constitute ideal subjects for fieldwork. That we don't readily think of these "localities" as field sites should give us pause. Is the idea of the local a way of smuggling back in assumptions about small-scale societies and face-to-face communities that we thought we had left behind? Why is it that, for example, local politics is so anthropological, whereas national or international politics is not ("natives" as political actors are rarely described in terms that would situate them within a political world we share —"left-wing," "rightist," or "Social Democrat")?[24] Similarly, the household economy has long been considered eminently anthropological, but the study of labor unions or international finance much less so. One can, of course, use a "local" site to study a "nonlocal" phenomenon. But what makes a site "local" in the first place? In an oft-cited passage, Geertz has pointed out that "Anthropologists don't study villages (tribes, towns, neighborhoods . . .); they study *in* villages" (1973a: 22). But what remains unasked, conspicuously, is *why* we study "in villages" in the first place.[25]

As with field sites, then, there is clearly also a hierarchy of topics or objects of study, ranked according to their anthropological-ness. Things that are unfamiliar, "different," and "local" (read: not like at home) become defined as suitable anthropological objects, whereas phenomena and objects that are similar to "home" or already in some way familiar are deemed to be less worthy of ethnographic scrutiny. Thus an account of an indigenous ritual, especially if it is strange, exotic, and colorful, is almost automatically "anthropological," and eminently suited to publication in a leading anthropological journal; television viewing, meanwhile, has remained until recently largely terra incognita for anthropology.[26] Even if one were to accept the problematic idea that anthropology's mission is that of "cultural critique," the topics that are deemed suitably "anthropological" already circumscribe the form and scope of that critique.

The Fieldworker as Anthropological Subject

We now turn to the third of our themes, the construction of an archetypal fieldworker and the consequent ordering of the identities of ethnographers. Anthropologists often speak, sometimes half-jokingly, of fieldwork as a "rite of passage," a ritual of initiation into a mature professional identity. We suggest that it would be useful to take this formulation seriously, instead of allowing it to pass as a joke, by asking precisely what kind of a social being such a ritual of initiation produces. If a heroized journey into Otherness is indeed a rite of passage, what sort of subject might we expect to be formed by such a rite?

We have seen that ideas about Otherness remain remarkably central to the fieldwork ritual. But any conception of an Other, of course, has implications for the identity of the self. We will argue that even in an era when significant numbers of women, minorities, and Third World scholars have entered the discipline, the self that is implied in the central anthropological ritual of encountering "the Other" in the field remains that of a Euro-American, white, middle-class male. We will demonstrate how this unmarked category is constructed through an examination of disciplinary practices that endow certain kinds of research questions, methods, and textual production with "excellence."

The rhetoric of meritocracy, with its powerful roots in capitalist ideology and the competitive conditions of academic production, and its seeming objectivity, appears to be socially neutral in the sense that it does not automatically privilege certain groups of people. Who wouldn't agree with the goal of hiring the best scholars, rewarding the best researchers, and training students so that they become the best anthropologists? The problem is, of course, that there is no neutral grid through which such judgments can be made.[27] The hierarchy of field sites noted above assigns positions based on degrees of Otherness. But Otherness from whom? Is Africa more Other

than Europe for a Third World anthropologist? For an African American? For whom are minority populations in the United States more worthy anthropological objects? The hierarchy of field sites privileges those places most Other for Euro-Americans and those that stand most clearly opposed to a middle-class self. Similarly, the notion of going to "the field" from which one returns "home" becomes problematic for those minorities, postcolonials, and "halfies"[28] for whom the anthropological project is *not* an exploration of Otherness. Such people often find themselves in a double bind: some anthropologists regard them with suspicion, as people who lack the distance necessary to conduct good fieldwork; on the other hand, well-intentioned colleagues thrust on them the responsibility of speaking their identity, thus inadvertently forcing them into the prison-house of essentialism (cf. chapter 9).

Amory (chapter 5) shows how ideas about Otherness, and the taking for granted of an unmarked, white subject, have helped to shape the field of African studies in the United States, and to produce a durable division between it and Afro-American studies. She shows that African American scholars were discouraged from working in Africa, on the grounds that they were "too close" and would not manage to be "objective," while white scholars were judged to have the appropriate distance from the black "Other." This helps to explain the fact that the contemporary field of African studies (like the field of anthropology itself) contains remarkably few black American scholars.[29] Unexamined assumptions about Otherness that came along with the idea of "a good field site" thus turned out to be racially exclusionary.

Likewise, the implicit standard against which "good fieldwork" often continues to be judged is highly gendered. The archetypal ideal of the lone, manly anthropologist out in the bush, far away from the creature comforts of First World life, derives, as Kuklick notes, from Romantic notions of (implicitly masculine) personal growth through travel to unfamiliar places and endurance of physical hardship (chapter 2). To be sure, women as well as men have over the years credentialed themselves—and even become powerful figures in the discipline—through the fieldwork rite of passage, and anthropology has historically been less closed to women than many other disciplines. Indeed, a certain romantic image of the female anthropologist seems to have a fairly prominent place in the American public imagination (probably due largely to the celebrity of primatologists such as Jane Goodall and Diane Fossey—though it is worth remembering that Margaret Mead was also a highly visible and influential public figure in her time). But it is no slight to the achievements of such women to say that they established themselves as "real anthropologists" only by beating the boys at their own (fieldwork) game. Many other women were not so lucky; historically, a very high proportion of women trained in anthropology have failed to secure institutional positions appropriate to their training (Behar and Gordon 1995).

Passaro (chapter 8) suggests that the image of field research as heroic adventure or quest remains with us today in the widespread, if often implicit, expectation that authentic fieldwork ought to involve physical hardship and even danger. Such expectations are far from neutral in gender terms.[30] For example, young women are discouraged from attempting "difficult" rural fieldwork in some areas of North India, because of the ever-present threat of rape and sexual violence; later, in the Western academy, their failure to spend long periods in rural areas where the "real" India lives is construed to show the absence of "good fieldwork"—a question of merit, not gender discrimination.

Similarly, the notion that field sites should be selected solely for disinterested scholarly reasons continues to be highly influential. Although it is widely recognized that this is not how most of us choose our field sites, the vocabulary of justification employed in grant proposals, books, and research reports requires that such choices be cast in terms of the theoretical problems that the research site was especially suited to think about. Such a view privileges those who have no compelling reason to work in particular localities or with particular communities other than intellectual interest. For those interested in working with their "own" communities, engaged in activist organizing, or responsible for supporting financially strapped, extended families, exoticism has no inherent value. Leaving their commitments and responsibilities for the sake of untethered "research interests" is for many anthropologists a Faustian bargain, a betrayal of those people whose lives and livelihoods are inextricably linked to their own. Once again, what pass for universal, meritocratic norms end up supporting a particular structural and ideological location, one occupied most often by white, middle-class men.[31] In this context, we might understand the recent figures showing that, as of the 1992–1993 academic year, fully 90 percent of all full-time anthropology faculty in the United States were white, and 70 percent were male (American Anthropological Association 1994: 288, 291).

We do not want to be misunderstood as suggesting that an academic discipline can or should attempt to do without standards of excellence. Our point is only that the social and political implications that any such standards must contain ought to be made explicit and open to debate and negotiation. The alternative to evaluating anthropologists according to prevailing norms of fieldwork is not to forgo all evaluation (which would be neither possible nor desirable), but to develop different and better-justified criteria of evaluation, based on a different conception of what should count as "good work" in anthropology. Where might such a conception come from, and how might it be legitimated? It is with such questions in mind that we briefly survey some alternative traditions of "field" and "fieldwork" on which it might be possible to draw.

IV. HETERODOXIES AND HEGEMONIES: ALTERNATIVE TRADITIONS OF "FIELD" AND "FIELDWORK"

Thus far, we have emphasized the constitutive role of a certain dominant tradition of "the field" (what we have called the Malinowskian tradition) in shaping the bounds of anthropology and defining what sorts of work will be permitted within that disciplinary space. What we have left unmentioned in tracing the dominant Malinowskian orthodoxy and its effects are the various heterodox practices of "field" and "fieldwork" that have existed in different ways both within and, as it were, adjacent to the constituted field of anthropology. Lacking the space to explore this issue in depth, we will simply point to, and give brief examples of, three different kinds of heterodoxy. First, we will discuss the diversity of actual practices and conceptions of "the field" submerged in the history of the dominant Anglo-American stream of anthropology. Second, we will briefly address the heterodoxy of practices of "the field" in various national and geographical sites that lie at some distance from anthropology's hegemonic geopolitical "core" (i.e., national traditions other than those in the U.S., U.K., and France, and the issue of "Third World" anthropologies). Finally, we will consider the way that anthropological practices of "the field" have maintained their distinctiveness in relation to "fieldwork-like" practices in other genres of representation, such as folklore and ethnic studies, realist novels of experience, and "insider ethnography." By pointing to the existence of such heterogeneity, we hope both to complicate our so-far oversimple picture of anthropology's practices and conventions of "the field," and to suggest that it may be possible to draw on such heterodoxies as resources for the disciplinary rethinking that, as we argue in Part V, is both urgently needed and already well under way.[32]

Hidden Heterodoxies: Rereading Anglo-American Anthropology

In the usual renditions of the history of anthropology, the triumph of the Malinowskian fieldwork revolution is set against a backdrop of theoretical and methodological darkness called "diffusionism." Students are rarely called upon to *read* any of the early twentieth-century diffusionists, but are often treated to derogatory accounts of the "hyper-diffusionism" of such figures as Grafton Elliot Smith and William Perry, whose "speculative" schemes, "conjectural" history, and lack of "real" fieldwork experience are used as foils against which to set the Malinowskian achievement.

Joan Vincent (1990, 1991) has recently developed a provocative argument that diffusionism's poor reputation is largely undeserved, and that many of anthropology's later failings may be traced to the turn that led away from key questions of history and culture contact in the early decades of the century. The "fieldwork revolution," in this view, was a mixed blessing. To be sure, it brought certain objects into view, and established an empirical basis for a certain kind of anthropological inquiry. But at the same time, it shifted attention away from some of the crucial issues with which diffusionists had been most concerned.[33]

While Malinowski, for instance, was constructing an image of the Trobriands as an isolated and self-contained natural laboratory, the diffusionist Rivers was conspicuously concerned with just the thing that Malinowski seemed to be (until late in his career) so determined to ignore: the "rapid and destructive change" that the new imperialisms were inflicting on the peoples of Melanesia and elsewhere (Vincent 1990: 120). Rivers's edited work *Essays on the Depopulation of Melanesia* (1922) was meant to document such effects of "culture contact" as "blackbirding," the abusive form of labor recruitment in which some 100,000 Pacific Islanders were removed and forced to work as indentured plantation laborers (Vincent 1990: 120, 122, 198). According to Vincent (1991: 54), for a historically oriented scholar such as Wheeler, diffusionism meant not wild speculative schemes, but "an ethnology that was historical, that dealt with complex as well as primitive societies and that recognized culture contact, movement, and change." Even the much-abused arch-diffusionist Elliot Smith seems, from the vantage point of the 1990s, surprisingly ahead of his time, since (as Elkin put it) he "saw the whole civilized world as one Oikoumene (using Kroeber's term), of which diffusion, or the interpenetration of culture traits and complexes, was the means of ensuring continuity in space and time" (cited in Vincent 1990: 123). Given such interests, it is only natural that Elliot Smith should have been wary of the narrowing scope implied by "the fieldwork revolution," irreverently demanding to know why "the sole method of studying mankind is to sit on a Melanesian island for a couple of years and listen to the gossip of the villagers" (cited in Stocking 1992a: 58).

No doubt, the virtues of Edwardian diffusionism can be exaggerated, and Vincent may be stretching a point when she claims for diffusionist theory a "latent function in countering the dehistoricization of a dominated people" (1990: 123). But, whatever its faults, diffusionism did show an interest in larger political and economic contexts and dynamic historical sequences that would be rediscovered much later. And Vincent is surely right to insist that it was not only functionalism as a theory, but fieldwork as a hegemonic method, that helped to drive such questions out of the anthropological mainstream for so many years. As she points out in showing how the British methodology handbook *Notes and Queries* constructed the domain of "politics" on the eve of the "fieldwork revolution," "the *method* of study—close and prolonged observation—was beginning to shape the *field* of study; the closed system was in the making" (Vincent 1990: 116).

The Boasian tradition in the United States had a significantly different orientation, and the emphasis on culture history and the collection of texts and text-analogues gave early twentieth-century American anthropology a relationship to "the field" that was initially quite different from the British natural history approach. Boas himself conceived of the anthropological task less in terms of observing functioning societies, and more as a matter of compiling documentation for disappearing cultures, with the aim both of reconstructing histories of migration and diffusion and of assembling an archive of primary materials so that the indigenous cultures of the Americas might live on in libraries and museums, much as the ancient and premodern cultures of Europe did (Stocking 1992a: 62–63). While there is much to object to in this paradigm (not least the fatalistic indifference to the contemporary struggles and predicaments of actually existing Native Americans [cf. Stocking 1992a: 163]), it is also worth noting that this approach implied a healthy skepticism about the idea of encountering intact, observable "primitive societies" that could be holistically described through the direct experience of participant observation. Methodologically, Boasian "salvage anthropology" was eclectic, combining firsthand interviews and observations with the analysis of historical texts, folklore, archaeological materials, oral history, and the recollections and expert knowledge of key informants.

As American anthropology outgrew its "salvage" phase, two different paths seem to have been available. One was to adopt the Malinowskian model of direct observation of contemporary (and exotic) "primitive societies." Here, the highly visible figure of Margaret Mead in her pioneering work in Samoa and New Guinea marked a major turn away from the historicist concerns of early Boasian anthropology and toward a model of fieldwork that converged with British practice.[34] The other, less celebrated path did not lead out from the United States to new, "primitive" sites abroad, but out from the Indian reservation and into the larger American society, via the question of "acculturation."

Like diffusionism, acculturation studies involved the blurring of "here" and "there," and challenged the idea of a clearly demarcated space of Otherness. Acculturation was the domain of "creole" cultures, of what Sidney Mintz (1970: 14) once described (speaking of the Afro-American diaspora) as "not the things anthropologists' dreams are made of":

> Houses constructed of old Coca-Cola signs, a cuisine littered with canned corned beef and imported Spanish olives, ritual shot through with the cross and the palm leaf, languages seemingly pasted together with "ungrammatical" Indo-European usages, all observed within the reach of radio and television.

Like diffusionism, acculturation studies have long suffered from a bad reputation within the discipline. In the 1930s, the editor of the *American Anthropologist* even opined that acculturation studies were not in fact anthropology at all, belonging instead in political science (Vincent 1990: 198; Spicer 1968: 22). Even today, the very word *acculturation* is likely to elicit yawns (if not shudders) from contemporary anthropologists trained to critique the functionalist, depoliticized acculturation studies of the 1950s and 1960s (cf. Spicer 1968).[35] Yet much of the early work on acculturation was an attempt to bring anthropology to bear on contemporary domestic social problems and to engage anthropological expertise with political issues such as racism and immigration (Vincent 1990: 197–222). Indeed, Vincent goes so far as to claim early acculturation theory as part of a "subterranean trend within the discipline" that "contained, albeit implicitly, [an] attack on racial domination, imperialism, and monopoly capitalism" (1990: 222). This may overstate the case. But given anthropology's current theoretical problems and political commitments, it is far from clear that this is an area of the disciplinary history that ought to be despised or ignored. Indeed, at least some of the heterodox forms of anthropology that flourished in the problem-oriented work of the 1930s and 1940s would seem to be of considerable contemporary relevance.[36]

The Depression, of course, put domestic poverty and social issues on the anthropological map. Anthropologists were led to study not only minority groups and questions of "assimilation" and "culture clash," but also aspects of "mainstream America" that had conventionally been considered to lie beyond the bounds of the discipline. Thus Walter Goldschmidt, for example, originally trained as a Native Americanist in the Boasian tradition, shifted his attention to agribusiness and changing class structure in a California farming town (Goldschmidt 1947). Other anthropologists were similarly inspired to apply anthropology to domestic social problems by new social programs such as the Works Progress Administration, which funded a wide range of social research with the twin aims of creating a base of knowledge to support "New Deal"–style social reforms, and creating research projects in which the unemployed could be given jobs. A full study of the impact of such programs on anthropological practice has yet to be completed. But it is clear that this form of anthropological intervention did involve some significant heterodoxy, not only in the selection of research topics, but (our particular concern here) in practices of "the field."

One example of such heterodoxy is Paul Radin's ethnography *The Italians of San Francisco: Their Adjustment and Acculturation* (Radin 1970 [1935]).[37] First published in 1935, in the midst of the Depression, this project is a vivid illustration of a road not taken in mainstream American anthropology. The study is unconventional in a number of ways, perhaps most notably in its explicit left politics, its strong commitment to a historical account, and its concern to present the voices and life stories of informants. Indeed, the text reads in some ways more like a leftist social history from the 1960s than a Boasian ethnography of the 1930s. But Radin's study is of special interest to us here for its heterodox experimentation with "field methods." Radin, a student of Boas's and friend of Sapir's, was one of the most meticulous of the Boasian fieldworkers.[38] But this was a large-scale study, requiring large numbers of investigators, instead of the usual lone anthropologist. What is more, since this was an employment project, "the investigators had to be taken from the county relief rolls" (1970: 5). Much as Malinowski had once made a virtue of necessity by treating his imposed lengthy isolation on the Trobriands as a methodological breakthrough, Radin (1970: 5–6) explains:

> The limitations thus imposed, far from militating against the accuracy of the information, actually increased it, for academically and professionally qualified observers are often the worst people to send out to secure sociological material. Their very training erects an undesirable barrier between themselves and the persons to be interrogated and this barrier is increased by the fact that they have frequently no experience in establishing contacts with strangers.

Here, Radin directly contradicts the most sacred premise of a newly professionalized anthropology, the premise that only professionally trained observers could be trusted to collect ethnographic data. On the contrary, Radin claims, intellectually elite and socially aloof Ph.D.s, by virtue of their social distance, made very poor interviewers of working-class Italians, while many of his unemployed research assistants were much better qualified:

> The essential qualification for an observer is that he possess the gift for establishing a direct and immediate contact with his source of information in as unobtrusive as possible a manner. The persons almost ideally adapted for bringing about such a relation are salesmen and business solicitors such as insurance agents, real estate agents, etc. (1970: 6)

The anthropological heresy is complete: the real secret of ethnographic rapport is to have the fieldwork done by unemployed insurance salesmen and real estate agents! One could hardly ask for a more vivid illustration of the point that conventions of fieldwork are shaped not simply by intrinsic methodological merits, but by the institutional conditions of intellectual production.

It is easy enough to laugh at the image of the insurance salesman as welfare-fieldworker. But the issues raised by Radin's heterodoxy are serious ones. After all, how many of the "lone anthropologists" doing fieldwork in "other cultures" have actually worked alone? What does the heavy reliance of so many ethnographers on "native" research assistants do to our conceits about the intrinsic virtues of the "professionally trained observer"? Radin's strategy neatly reverses the hard-won Malinowskian/Boasian dogma that only people with university degrees in anthropology can really get the facts right. Radin argues, plausibly enough, that such professionals are socially separated from those they would understand by their very training, and that local intellectuals or specialists may be better positioned, at least for certain sorts of data collection.

How different might anthropology look today if the academic mainstream had accepted Radin's argument that inexperienced and often socially awkward First World graduate students are not necessarily the best of all possible observers? What different ways of theorizing the relation between professional researcher and local expert might have been developed? What relevance might this have for the contemporary anthropological task of forging new and less colonial modes of engagement between anthropologists and the intellectuals who inhabit the societies they study?[39] There may well lie some questions worth going back to in the forgotten corners of the history of heterodox anthropological fieldwork.

Another form of heterodoxy, of course, appeared in the 1960s and early 1970s, with the rise of a host of politically engaged challenges to anthropology-as-usual (e.g., Hymes 1972; Gough 1967, 1968; Asad, ed., 1973; Tax 1975; Huizer and Mannheim 1979). In some cases, it seems to us, the political radicalism of such projects was hindered by a conventional conception of the relation of anthropologist to "the field"; thus, the programs of "action anthropology" (cf. Tax 1975) too often tended to assume a white, middle-class anthropologist who would go "there," into "the field," and be a catalyst, organizer, or broker for "the local people." As we will suggest in Part V, a questioning of the neat separation of "here" and "there," "home" and "field," can suggest other, more complex models of political engagement.

But it is also striking that many of the critics of the 1960s and early 1970s did call into question not only the usual anthropological focus on the "Other," the different and the exotic (what Mintz [cited in Hymes 1972: 30] called the "preoccupation with purity"), but also at least some of the taken-for-granted conventions of "field" and "fieldwork" (e.g., Hymes 1972: 32; Willis 1972: 148). Yet while the political challenges of the 1960s radicals provoked a vigorous disciplinary discussion of anthropology's political commitments, its relations to imperialism and colonialism, the possibility of a Marxist anthropology, and so on, the received ideas of field and fieldwork remained mostly above the fray.

For many who worked in this vein, to be sure, the anthropological world of "peoples and cultures" was reconceptualized as an interconnected capitalist world system characterized by relations of exploitation. With such a perspective, one might aim to study not this or that isolated, traditional society, but such things as the impact of multinational capital on this or that community, or the articulation of local production systems with migrant labor or cash cropping. But that an anthropological dissertation would normally involve an ethnographic study of a *local community* (however "linked" it might be with a wider system), and that such a study would make use of the usual fieldwork methodology (stereotypically, "twelve months in a village"), appropriately supplemented with historical "background," remained the common sense of the discipline.[40]

This development, the ultimate triumph of a version of the hegemonic "Malinowskian" practice of "the field," brings our discussion back to where we began. For it is necessary to remember that the heterodoxies we have briefly sketched here, however interesting or provocative, remained heterodox; ultimately, all were marginalized and contained. We have revisited them here with the aim less of rewriting the anthropological past than of rereading it—combing our disciplinary history for resources that might contribute to a "reinvention" of the fieldwork tradition. It should be clear that we are not advocating the wholesale adoption of any of the various heterodox fieldwork practices we have discussed—neither a return to Edwardian diffusionism nor a resurrection of WPA anthropology is what we have in mind. Our aim is not to propose a single alternative to the conventional image of "the field," but only to denaturalize the Malinowskian model, and to rediscover it—not as the necessary methodological foundation of all anthropology, but as one methodological possibility that, in its striking academic-political success, has allowed us to forget the existence, within our own disciplinary history, of alternatives.

From the Margins: Alternative Regional and National Traditions of Field and Fieldwork

In his recent memoir *After the Fact* (1995), Clifford Geertz, whose reputation as a fieldworker has attained near-mythic proportions, provides a vivid description of his first fieldwork experience. After experiencing the normal graduate student anxiety over the choice of a fieldsite ("Where was our Trobriands, our Nuerland, our Tepoztlan to be?" [1995: 101–102]), he was recruited quite "accidentally" to be part of a multidisciplinary nine-member team led by a professor in Harvard's Social Relations Department. Their destination was Java, where they were to be paired with counterparts from an Indonesian university.

The three professors directing the project on the Indonesian side wanted to use the opportunity to train some of their own students to do anthropological research. According to Geertz, the Indonesians had the "unworkable" idea, learned from the Dutch, that field research might be conducted out of an old Dutch resort hotel, calling people in from the countryside to be interviewed in groups and asking them questions from a prepared schedule of topics. "It would be hard to conceive an image of social research more entirely opposed to our notions," Geertz observes, ". . . than this extraordinary reincarnation of the pith-helmet procedures of colonial ethnology" (1995: 105). "Caught between academic mentalities, one ambitious, confident, and ultramodern, one nostalgic, defensive, and obsolescent" (1995: 105–106), the Americans sought to evade the demands of their hosts, given their "conviction that what [they] wanted to do demanded free, intimate, and long-term relations with those [they] were studying, isolated from external oversight and the attentions of the state," a "maximally uncontrolled situation: the Trobriands in Java" (1995: 106). In the end, the Indonesians yielded (though not before the minister of culture had delivered "a three hour harangue about arrogance, faithlessness, and the fact that the world was changing and whites had damn better realize it" [1995: 108]), and the Americans were able to settle into a "local community" favorably situated "much too far for anyone to commute, much too rustic for anyone to want to" (1995: 107). The anthropologists, now free from both supervision and the need to collaborate, were left alone at last: "here, finally, was 'the field'" (1995: 109).

What this extraordinary account makes clear is that the chief division between the Indonesians and the Americans lay less in their theoretical orientation than in their conceptions of what constituted "the field" and how one was to go about doing fieldwork. Instead of responding to their hosts' expressed desire to train students and work collaboratively, the Americans reacted in horror to field methods different from their own, dismissing them as leftovers of colonial ethnology. It is easy to agree with Geertz that the Indonesians' proposed approach might not be the best way to build the rapport, trust, and informal understanding that conventional Malinowskian fieldwork at its best can create. And there are indeed often compelling reasons for anthropologists to wish to speak to their informants informally, alone, and in confidence—and, indeed, to seek to evade "the attentions of the state." But there remains a certain irony in the dismissal of a methodological proposal that included nationalist demands for student training and local collaboration as "pith-helmet procedures of colonial ethnography," particularly when we bear in mind the baldly neocolonial relations that allowed a team of Ford Foundation–funded American graduate students to descend upon the newly independent nation of Indonesia in 1951 and proceed to disregard completely the conditions of research that had been set by local academics. As Geertz makes clear, the Americans sought "free, intimate, and long-term relations" not with Indonesian scholars, but with Indonesian natives; thus the U.S. team sought to break away from their "hosts" as quickly and completely as possible. In this way, the Americans attained the archetypal anthropological "field"—a space of freedom in which they might study the natives in an environment undisturbed by the presence of educated, urban Javanese.

Our point is not to find fault with Geertz's conduct in this episode. On the contrary, our analysis leads us to regard with some sympathy the discomfort and distress of a U.S.-trained anthropology graduate student denied the right to do "fieldwork" in the recognizable Malinowskian fashion. As we have insisted, on such points are careers made and broken. And nothing we have said is meant to detract from Geertz's justly celebrated achievements as a fieldworker. Indeed, we believe that Geertz, in the incident described, did only what any good fieldworker of the time would have done. Our interest in his case lies in the fact that he has given us an unusually explicit description of what being a "good fieldworker" entailed: namely, constructing "a good field." It is here that his account is so telling, for it allows us to see with special clarity how a certain dominant practice of "the field" asserted itself, and to what effect. Faced with a situation that might have led to an interrogation of their methods, and even to constructive and creative ways to bridge the gap that separated them from their Indonesian counterparts, the American scholars could only react with disbelief at the "nostalgic, defensive, and obsolescent" views of their hosts. It is important for the purposes of our argument to note that it was differences of field methods, and not of theories and subject matter, that in this instance most firmly divided the American ethnographers from their Indonesian counterparts. For all the anthropological devotion to the understanding of difference, this was one difference that proved insurmountable.

As this episode suggests, a detailed study of regional "anthropologies" could contribute much to understanding the different ways in which "the field" has been constituted, and instituted, in diverse locations. In most standard accounts of the history of anthropological theory, the canonical narrative examines the relationship between national traditions of anthropology only in the United States, Britain, and France. Other national traditions are marginalized by the workings of geopolitical hegemony, experienced as a naturalized common sense of academic "center" and "periphery." Anthropologists working at the "center" learn quickly that they can ignore what is done in peripheral sites at little or no professional cost, while any peripheral anthropologist who similarly ignores the "center" puts his or her professional competence at issue ("They're so out of it, they haven't even heard of x").[41]

If a diversity of practices and conventions of "field" and "fieldwork" exists in such "peripheries," as we suspect, there might be much to learn from comparing the different fields of knowledge that such different practices and conventions open up. Most anthropologists working in the U.S. or U.K. (and we include ourselves here) know very little about the history of anthropology (and such related fields as ethnology and folklore), even in such strong and long-established "national" traditions as those of Mexico, Brazil, Germany, Russia, or India. We do not propose (nor do we consider ourselves qualified) to discuss these traditions in any depth here. And it is no doubt misleading to imagine discrete and autonomous "national" traditions in an academic world structured around the global hegemony of a North Atlantic center, which often does give the universities of the periphery and semiperiphery a derivative character.[42] But it seems clear that, in spite of homogenizing tendencies rooted in colonial and neocolonial histories, practices of "the field" and definitions of the discipline are indeed significantly different away from the hegemonic centers of intellectual production.

Asked about his experiences in Mexico, the United States, and France, the Brazilian anthropologist Cardoso de Oliveira (Correa 1991) spoke enthusiastically about his intellectual exchanges with his Mexican colleagues in contrast to his description of the wonderful *facilities* for research at Harvard and Paris. Apparently, the situation in which Brazilian anthropologists found themselves doing "fieldwork" in their own country had more in common with the problems and dilemmas faced by Mexican anthropologists than those anthropologists located in First World institutions. At a time when British and American theories of ethnicity were emphasizing more depoliticized conceptions of "social change" and "acculturation" respectively, Cardoso de Oliveira was developing his theories of "interethnic friction," which were in turn influenced by Rodolfo Stavenhagen's important theories of "internal colonialism" in Mexico (Correa 1991: 340; de Alcantara 1984: 113–116).[43] In like manner a generation earlier, Fernando Ortiz had found that, in writing from and about Cuba, it was useful to replace the concept of "acculturation" with a notion of "transculturation" to capture the "counterpoint" through which change occurred not simply "in a culture," but between and across interconnected cultures. Ortiz's "field" was not a bounded localized community, but (in a conception that foreshadows both Mintz 1985 and Gilroy 1993) a multistranded transatlantic traffic of commodities, people, and ideas that shaped a Cuban experience conceived as a "history of . . . intermeshed transculturations" (Ortiz 1995: 98; cf. Coronil 1995).

The regional heterogeneity of "anthropology," then, is not only a matter of diverging politics and histories, of different divisions of academic labor and distinctive institutional configurations. It is also, and at the same time, a matter of different conventions and practices of the field, with corresponding implications for the way anthropology is constituted and bounded as a discipline. In central and eastern Europe, for example, ethnography comes out of a tradition of national ethnology and folklore studies, and fieldwork is focused on the rural and "folk" cultures of the ethnographer's own society. "The field" is therefore always nearby and easy to visit; researchers spend a few weeks in rural areas collecting data and then come back to analyze them. Institutions are neither set up to grant research leaves of one year or more, nor are there funding agencies to support such "fieldwork." Furthermore, there is no assumption that after researchers return from "the field," their contacts with subjects will cease (Hofer 1968; Halpern and Hammel 1969; Jakubowska 1993). In many African universities, meanwhile, anthropology departments are nonexistent (thanks largely to the discipline's "colonialist" reputation), and anthropological research must be done (if at all) in affiliation with sociology, history, or economics departments, or in the guise of studying oral literature, or through externally funded development projects. In each case, anthropologists are obliged to come to terms with different norms and expectations about what kind of fieldwork is appropriate, how long it may last, and what sort of team organization, use of assistants, and so on are required.

In pointing to the existence of such diversity in fieldwork traditions, we are not advocating that North American anthropologists simply ought to adopt the fieldwork conventions of other national or regional practices of anthropology. Indeed, we would agree that there are often compelling reasons not to do so.[44] The point is not to valorize blindly such nondominant fieldwork traditions, but only to suggest that our discipline's much vaunted respect for cultural "difference" should include the recognition that anthropological methods that differ from one's own are not inherently suspect or inferior. Instead of decrying the "lack of professionalism" or "backwardness" of the discipline in other geographical contexts, we need to ask what kinds of knowledges these other practices of "the field" make possible. For those of us based in North American universities, what are our responsibilities when faced with practices of "the field" that are very different from our own? Is the only appropriate response to flee from those differences in the name of an "authentically anthropological" methodology, as Geertz's team did in Java?[45] And, if not, what would it mean to arrive at a "re-formed" method? Might such practical reworkings help bridge the rather conspicuous contemporary gap between our ambitious theoretical aspirations and our remarkably unreconstructed methodological habits?

Other Genres, Other Fields?

By definition, the borders of the discipline constitute those spaces where the hegemonic hold of canonical methods and disciplinary formations has been the weakest. These borders are not merely geographic, but can be seen in the heterogeneity of ethnographic representations that threaten to overrun the well-policed boundaries of anthropology. We will not deal with those obvious suspects that anthropology struggled to distinguish itself from at the beginning of the formation of the discipline, namely, travelogues, missionary reports, the narratives produced by colonial bureaucracies, and so forth. Rather, we wish to highlight a congeries of practices and represention of the field that interrupt the mutual constitution of the "field" as a specific empirical practice and the "field" as a discipline.

Although we cannot pursue this topic in any detail here, we have relied heavily on some of the excellent research already done in order to draw attention to three of these borders: the disciplinary challenge posed by folklore, sociology, and ethnic studies; the questions posed by heterodox representations of fieldwork such as novels of the field, novels by "natives," and nonrealist ethnographies; and, finally, the difficulties raised by heterodox "fieldwork" such as "insider" ethnography, or the use in ethnography of observations derived from the experience of growing up in "a culture."[46]

Folklore's ambivalent status in American anthropology is institutionally visible in its occasional inclusion in anthropology departments, history or literature departments, and at times in a separate program. Whereas collecting "folklore" was an intrinsic part of the Boasian method, its status may have diminished as participant observation became the regnant method in anthropology: a narrative based on what one observed and experienced was more "direct" (hence closer to the truth?) than a narrative based on collected texts or stories. The marginal status of folklore was accentuated when it intersected sociology and that genre of research that we now label "ethnic studies," as is painfully evident from the low status accorded to the pioneering researches of Zora Neale Hurston during her lifetime, and the continuing neglect of scholars such as Americo Paredes in the teaching of the anthropological canon. Similarly, the ethnographic and ethnohistorical research of scholars such as W. E. B. Du Bois, C. L. R. James, and St. Clair Drake is rarely mentioned in the same breath as that of Boas, Radcliffe-Brown, and Malinowski (cf. chapter 5).[47] Is it merely coincidence that anthropology's boundaries against folklore, ethnic studies, and sociology are constructed in such a way that scholars of color so often fall outside the boundaries of what is considered to be "real" anthropology? This is one place in which the consequences of using largely implicit standards to determine what is appropriate "fieldwork," and who its implied subject is, are clearly evident.

A second border that threatens to undo the self-evident connections between the discipline, field methods, and subject-formation is that constituted by heterodox representations of fieldwork. There has already been a fair amount of interest in novels of the field, often written by those denied the institutional legitimacy accorded to archetypal male fieldworkers out in the bush—their wives. The ethnographic novel, however, has also been a preferred form of representation by those (mostly women) for whom academic positions were impossible to attain (Zora Neale Hurston, Ella Deloria), or by those who wanted to reach a wider, genuinely popular audience for their work (Behar and Gordon 1995; Visweswaran 1994; Lamphere 1992). Ethnography, as a genre of realist description, has always drawn inspiration from fiction (Malinowski, for example, boasted of his ambition to be "the Conrad of anthropology" and read voraciously in "the field"). Writing ethnography novelistically is considered acceptable, as long as it does not go "too far"; elegant writing is a virtue, but becoming "too literary" is a serious fault

(cf. Landes 1994 [1947]). But many anthropologists are uneasy about reading realist novels ethnographically (cf. Handler and Segal 1990). For our purposes here, we will employ one example to help show the difficulties involved in maintaining this distinction.

Adwaita Mallabarman's *A River Called Titash* was originally published in Bengali in 1956, and Kalpana Bardhan has recently (1993) translated it into English. Mallabarman was born in 1914 of a Hindu fishing caste called the Malos in what is now Bangladesh. He was the first person of his caste to receive a high school education, then went on to a career in literary magazines and journalism. *A River Called Titash* is a loving recreation of the everyday practices and rituals of the Malo community and the Malo way of life, which, by the time the novel was completed in 1951, had been dismantled by the conflicts surrounding the partition of the subcontinent a few years earlier. The novel is a truly hybrid form, a curious mixture of ethnographic detail and conventional narrative. Sections that might easily have been lifted from a canonical ethnography are overlaid on a plot, much as the narrative fictions employed in ethnographies describe "a day in the life" of an ordinary villager or a "typical" rendering of a ritual. The excessively lyrical descriptions of the river rival Malinowski's vivid sketches of the play of color in the Trobriands (Stein 1995). Mallabarman was not trained as an anthropologist and did not write the novel as an "alternative" version of "his people" to oppose representations created by anthropologists. Yet he too was engaged in salvage ethnography by recording the Malo's lifeways, struggles, and rituals at a time when the enormous political changes that swept the subcontinent were destroying this existence. Novels such as *A River Called Titash* blur the boundary between "novel" and "ethnography" (cf. Michaels 1994). If the call to "decolonize" anthropology is to be taken seriously, why should we not juxtapose "natives'" representations of "themselves" and ethnographies written by those serving the colonial government? In this spirit, it would make sense to read *A River Called Titash* alongside a "professional" account written at that time, such as Leach's *Political Systems of Highland Burma* (1954).

Mallabarman's novel helps us to challenge a third border, that which separates "fieldwork" from other forms of dwelling (cf. Clifford 1992, and chapter 10 of this book). Is growing up in "a culture" a heterodox form of "fieldwork"? Mallabarman obviously draws on the knowledge and experience gained from living within a fishing community to paint a remarkably rich picture of village life, with an accretion of the subtle detail so necessary for "thick description" that could only have been acquired from a lifetime of "fieldwork." "Insider" ethnography[48] most clearly challenges the unspoken assumptions about what makes a site a "field" in anthropology. "Fieldwork" is a form of dwelling that legitimizes knowledge production by the familiarity that the fieldworker gains with the ways of life of a group of people. Unlike travelers and tourists, the fieldworker has experience, obtained by staying a long time, learning the language well, and participating in everyday life, which authorizes his or her discourse. Yet, paradoxically, if that experience is gained outside the institutional framework of a doctoral program in anthropology, it is consistently devalued. To argue that a "trained" observer is likely to "see" different things than an "untrained" observer is to state the obvious; yet, surely the claim that training enables certain things to come into light begs the question of what "training" might prevent one from seeing. A discipline in which "experience" is so central has been surprisingly unfriendly to the notion that "experience" is constantly reconfigured by memory. If an anthropologist can "write up" an ethnography based on data collected during doctoral fieldwork twenty or thirty years ago, why should it not be possible for "natives" to "write up" an ethnography based on their lives? In what sense might we think of one's "background"—growing up, as it were, in "the field"—as a kind of extended participant observation? In posing such questions, we do not mean to deny the evident differences between the two kinds of experience; we intend only to ask what the consequences are of treating such differences as both absolute and absolutely definitive of anthropology's disciplinary identity.

V. REINVENTING "THE FIELD": METHODOLOGY AND LOCATION

It is clear that anthropologists have in recent years been more and more inclined to depart from the conventions of archetypal fieldwork as they have taken on research projects not easily approached via the traditional model of immersion within a community (cf. chapter 10). Reflecting on their experiences of testing and even transgressing the disciplinary boundaries set by the expectations of "real fieldwork," several of the contributors to this book help point the way toward developing of new practices and conventions for the field. In this section, we will first briefly discuss how Weston, Passaro, Malkki, Des Chene, and Martin have contributed to a rethinking of field and fieldwork. We will then offer a general reformulation of the fieldwork tradition that we believe can preserve what is most vital and valuable in it, while not only leaving room for but properly valuing and legitimating the diverse and innovative new practices of the field that are evident in the contributions to this book and elsewhere.

Toward New Practices of the Field: Problems and Strategies

One of the most profound issues raised by recent work in anthropology is the question of the spatialization of difference. The unspoken premise that "home" is a place of cultural sameness and that difference is to be found "abroad" has long been part of the common sense of anthropology. Yet some of our contributors, drawing on recent work on gender and sexuality, begin

their "fieldwork" with the opposite premise—that "home" is from the start a place of difference.

In chapter 9, Kath Weston points out that studying such "difference at home" as gay and lesbian communities in the United States profoundly unsettles anthropological sensibilities. Who is the native and who is the ethnographer when "queers study queers"? Trying to speak as a professionally qualified ethnographer of gays and lesbians, Weston finds that she is heard as a "native"—speaking for "her own people," maybe even "an advocate" (cf. Narayan 1993). As a "Native Ethnographer," she must alternate between "I, Native" and "I, Ethnographer," losing "the nuance of the two as they are bound up together," the hybridity of the Native Ethnographer positioning. The reason, of course, is that the position "Native Ethnographer" itself blurs the subject/object distinction on which ethnography is conventionally founded. Speaking from such a position, at least within the discipline as currently constituted, implies not simply exclusion, but something more complicated that Weston calls "virtuality": a condition in which one is an anthropologist, but not "a real anthropologist," in which one has done fieldwork, but not "real fieldwork." The virtual anthropologist, Weston argues, must always be the one who lacks an authentic Other—unless she speaks *as* an authentic Other, in which case she ceases to be an authentic *anthropologist*. Yet, significantly, Weston suggests that the very studies that are most suspect in these terms are the ones that "could complicate [the] dichotomy between Us and Them in useful ways"; the virtual anthropologist may be the one who can contribute most to "the thoroughgoing reevaluation of the anthropological project that an understanding of hybridity entails."

Joanne Passaro's research (chapter 8) among the homeless in New York City raises some related issues. Like Weston, she reports encountering skepticism that researching the lives of homeless, transient people in her "own" society could constitute "real fieldwork." Well-meaning advisors pressed her to adopt a nativizing community-study model ("That family shelter sounds fascinating. Why not stay there and do an ethnography of it?"), imagining a stable territorial community even for people defined in the first place by their mobility, marginality, and lack of any stable "home." Tellingly, Passaro reports, "I often felt that my various disciplinary interrogators would be happiest if I discovered some sort of secret communication system among homeless people like the codes of hoboes earlier in the century," in which case a suitable "subculture" would have been found in which one could immerse oneself! Yet Passaro resisted the temptation to construct "a homeless village," and developed instead an innovative, hybrid methodology that involved a number of "sites that would afford . . . positionalities at varying points along a participant-observer continuum." Combining different sites and styles of "fieldwork" with various kinds of volunteer and advocacy work provided a successful, if unorthodox, methodological strategy for an ethnographic study that ended up yielding powerful and surprising insights into the predicaments of homeless people (cf. Passaro 1996).

In chapter 4, Liisa Malkki discusses a different way in which the methodological demands of one's research may require a reconfiguring of "the field." Her research among Hutu refugees in Tanzania led her to question one aspect of the fieldwork tradition that is commonly celebrated as a great virtue—its emphasis on the ordinary, the everyday, and the routine. As she points out, such an emphasis tends to direct attention away from those things that the refugees she worked with cared about most—the extraordinary and exceptional events that had made refugees of them, and the atypical and transitory circumstances of their lives in a refugee camp. She observes that a division of labor between anthropology and journalism has made all big, extraordinary happenings into "stories" to be covered by journalists, while the durable, ordinary, everyday occurrences are to be found in "sites" suitable for long-term anthropological fieldwork. What would it mean, she asks, to direct an anthropological gaze on singular, exceptional, and extraordinary events? What sorts of fieldwork would be appropriate to studying the "communities of memory" formed in the aftermath of such events? A different sort of engagement than that of the usual "anthropological investigation" of a geographical "field site" might, she suggests, be warranted.

For Mary Des Chene in chapter 3, the issue is the relation between fieldwork and history, and the way knowledge gained through archival research is received and valued within anthropology. As she points out, historical material is widely valued in anthropology as a supplement to "real fieldwork," but considerable anxiety is provoked if it begins to take center stage. Des Chene asks how different the two modes of acquiring knowledge really are, skillfully distinguishing the real differences from the mythology that valorizes fieldwork-based knowledge as necessarily truer or less mediated than other types. She also confronts the question of how ethnographic methods can be adapted for studying spatially dispersed phenomena, raising the issue of multisite ethnography (cf. Marcus 1995; Hastrup and Olwig 1996).

Finally, in chapter 7, Emily Martin also takes up the question of social and cultural processes that are not well localized spatially. She points out that even many ethnographers of science have retained an idea of a "scientific community" as spatially bounded, to be examined through the traditional methods of the community study. The reaction of one such traditionalist to Martin's own multisite methods ("Don't you know how to stay put?") tells us that the localizing conventions of "the field" remain strong even in an area such as the ethnography of science, which one might expect to have traveled far from the Malinowskian archetype. But Martin insists that key developments in science are also occurring simultaneously elsewhere in society and that we need different models and metaphors than those provided

by "the field" to grasp such changes. She proposes several new metaphors and shows how she used them in her own research, laying out a "tool kit" for exploring processes that occur neither in a single field site nor in some unlocated global space, but in many different spaces that are discontinuous from each other.

Retheorizing Fieldwork: From Spatial Sites to Political Locations

We begin our own efforts to rethink "the field" by building on recent critical reflections about how place has figured in anthropological conceptions of culture (cf. Gupta and Ferguson, eds., 1992, 1997; Appadurai 1988a). We have argued that the passage in and out of "the field" rests on the idea that different cultures inhere in discrete and separate places. Therefore, to go into "the field" is to travel to another place with its own distinctive culture, to live there is to enter another world, and to come back from "the field" is to leave that world and arrive in this one—the one in which the academy is located.[49] To challenge this picture of the world, one made up of discrete, originally separate cultures, is also to challenge the image of fieldwork as involving the movement in and out of "the field." "Where is the field?" D'Amico-Samuels (1991: 69) asks, when one studies gender, color, and class in Jamaica, writes about those experiences in New York, and participates in a seminar in Trinidad. "Which if any of these three experiences was fieldwork? Does fieldwork still carry the connotation of colonial geography—so that only activities in a Third World setting apply? . . . Do we think still of fieldwork in the archetype of the white-faced ethnographer in a sea of black or brown faces?" (1991: 72). Perhaps we should say that, in an interconnected world, we are never really "out of the field." Yet, if this is true, then what does change when anthropologists go from (usually) First World universities to various destinations around the world?

Ethnography's great strength has always been its explicit and well-developed sense of location, of being set here-and-not-elsewhere. This strength becomes a liability when notions of "here" and "elsewhere" are assumed to be features of geography, rather than sites constructed in fields of unequal power relations. But it is precisely this sense of location that is missing in a great deal of universalizing and positivist social science. Ethnography has always contained at least some recognition that knowledge is inevitably both "about somewhere" and "from somewhere," and that the knower's location and life experience are somehow central to the kind of knowledge produced. Yet, through the anthropological notion of "the field," this sense of location has too often been elided with locality, and a shift of location has been reduced to the idea of going "elsewhere" to look at "another society."

Taking as a point of departure the idea of "location" that has been developed in recent feminist scholarship,[50] we believe that it is possible to rethink the anthropological fieldwork tradition in quite a fundamental way, while preserving what we think are its real virtues. We wish to be clear that, however significant the problems with "the field" are, there remain many aspects of the fieldwork tradition that we continue to value—aspects that have allowed ethnographically oriented work in sociocultural anthropology (with all its faults) to serve as an extraordinarily useful corrective to the Eurocentrism and positivism that so often afflict the social sciences. We believe a well-developed attentiveness to location would preserve and build upon these aspects of the fieldwork tradition, which we will now discuss individually.

1. The fieldwork tradition counters Western ethnocentrism and values detailed and intimate knowledge of economically and politically marginalized places, peoples, histories, and social locations. Such marginalized locations enable critiques and resistances that would otherwise never be articulated (hooks 1990; Spivak 1988). Since anthropology departments continue to be among the few places in the Western academy not devoted exclusively or largely to the study of the lives and policies of elites, they constitute potentially important nodes for politically engaged intervention in many forms of symbolic and epistemic domination. We emphasize once again that our analysis of anthropology's "hierarchy of purity" of field sites is *not* meant to suggest that anthropologists should no longer work in far-flung and peripheral places—only that it is necessary to question the way that dominant conceptions and practices of "the field" have constructed such places. As Anna Tsing (1993) has recently demonstrated, by bringing marginality itself under the anthropological lens, instead of simply taking it for granted, it is possible to write about "out-of-the-way places" without distancing, romanticizing, or exoticizing them.
2. Fieldwork's stress on taken-for-granted social routines, informal knowledge, and embodied practices can yield understanding that cannot be obtained either through standardized social science research methods (e.g., surveys) or through decontextualized readings of cultural products (e.g., text-based criticism). One does not need to mystify or fetishize knowledge gained through long-term immersion in a social milieu to recognize its importance and value. Nor does one need to grant an unwarranted epistemological privilege to face-to-face interaction in order to appreciate the virtues of a research tradition that requires its practitioners to *listen* to those they would study, and to take seriously what they have to say.
3. Fieldwork reveals that a self-conscious shifting of social and geographical location can be an extraordinarily valuable methodology for understanding social and cultural life, both through the discovery of phenomena that would otherwise remain invisible and through the acquisition of new perspectives on things we thought we already understood. Fieldwork, in this light, may be understood as a form of motivated and stylized dislocation. Rather than a set of labels that pins down one's identity and perspective, location becomes visible here as an ongoing project. As in coalition politics a location is not just something one ascriptively *has* (white middle-class male, Asian American woman, etc.)—it is something one strategically *works* at. We would emphasize, however, that (as in coalition politics) shifting location for its own sake has no special virtue. Instead, the question of what might be called location work must be connected to the logic of one's larger project and ultimately to one's political practice. Why do we *want* to shift locations? *Who* wants to shift? Why? (D. Gordon 1993; Visweswaran 1994: 95–113; Enslin 1994).

What emerges, then, is a set of possibilities for rethought and revitalized forms of fieldwork. We are not advocating the abandonment of the practice of fieldwork, but rather its reconstruction—decentering "the field" as the one, privileged site of anthropological knowledge, then recovering it as one element in a multistranded methodology for the construction of what Donna Haraway (1988) has called "situated knowledges." We might emerge from such a move with less of a sense of "the field" (in the "among the so-and-so" sense) and more of a sense of a mode of study that cares about, and pays attention to, the interlocking of multiple social-political sites and locations.

Such a reconstruction of the fieldwork tradition is, as we have emphasized, already well under way in anthropological practice. Participant observation continues to be a major part of positioned anthropological methodologies, but it is ceasing to be fetishized; talking to and living with the members of a community are increasingly taking their place alongside reading newspapers, analyzing government documents, observing the activities of governing elites, and tracking the internal logic of transnational development agencies and corporations. Instead of a royal road to holistic knowledge of "another society," ethnography is beginning to become recognizable as a flexible and opportunistic strategy for diversifying and making more complex our understanding of various places, people, and predicaments through an attentiveness to the different forms of knowledge available from different social and political locations. Although more and more ethnography today is proceeding along these lines, however, the institutionalized disciplinary framework of reception and evaluation too often continues to see experiential, "field-based" knowledge as the privileged core of an ethnographic work that is then "fleshed out" with supplementary materials (cf. chapter 3).

Any serious decentering of "the field" has the effect, of course, of further softening the division between ethnographic knowledge and other forms of representation flowing out of archival research, the analysis of public discourse, interviewing, journalism, fiction, or statistical representations of collectivities. Genres seem destined to continue to blur. Yet instead of assuming that truly anthropological truths are only revealed in "the field," and attempting to seal off the borders of anthropology from the incursions of cultural studies and other disciplines, it might be a far healthier response to rethink "the field" of anthropology by reconsidering what our commitment to fieldwork entails.

Such a rethinking of the idea of "the field," coupled with an explicit attentiveness to location, might open the way for both a different kind of anthropological knowledge and a different kind of anthropological subject. We have attempted to demonstrate that the uncritical loyalty to "the field" in anthropology has long authorized a certain positionality, a particular location from which to speak about Others. Without an explicit consideration of the kind of subject and the kind of knowledge that ethnographic work produces—by what method? for whom? about whom? by whom? to what end?—we anthropologists will continue to valorize, in the universalizing language of meritocracy, a very particular social, racial, gendered, and sexual location. Practicing decolonized anthropology in a deterritorialized world means as a first step doing away with the distancing and exoticization of the conventional anthropological "field," and foregrounding the ways in which we anthropologists are historically and socially (not just biographically) linked with the areas we study (E. Gordon 1991). In other words, we have to move beyond well-intentioned place-marking devices such as "Western, white anthropologist," which too often substitute a gesture of expiation for a more historical and structural understanding of location. It also means taking away lingering evolutionist and colonialist ideas of "natives in their natural state," and denying the anthropological hierarchy of field sites that devalues work in so many intellectually and politically crucial areas (homelessness, AIDS, sexuality, the media) that are often deemed insufficiently "anthropological." But a heightened sense of location means most of all a recognition that the topics we study and the methods we employ are inextricably bound up with political practice (Bourgois 1991).

The traditional commitment to "the field" has entailed, we have argued, its own form of political engagement, in terms of both the knowledge it has produced and the kind of disciplinary subject it has created. Our focus on *shifting locations* rather than *bounded fields* is linked to a different political vision, one that sees anthropological knowledge as a form of situated intervention. Rather than viewing ethnographic intervention as a disinterested search for truth in the service of universal humanistic knowledge, we see it as a way of pursuing specific political aims while simultaneously seeking lines

of common political purpose with allies who stand elsewhere—a mode of building what Haraway (1988) has termed "web-like interconnections" between different social and cultural locations. Applied anthropology and especially activist anthropology have long had the virtue of linking ethnographic practice to a specific and explicit political project. Partly for this reason, they have been consistently devalued in the domain of academic anthropology (cf. Ferguson forthcoming). Yet we would emphasize that associating one's research with a political position does not by itself call into question the location of the activist-anthropologist in the way that we have suggested is necessary, since even the most politically engaged "experts" may still conceive of themselves as occupying an external and epistemologically privileged position. Rather than viewing anthropologists as possessing unique knowledge and insights that they can then share with or put to work for various "ordinary people," our approach insists that anthropological knowledge coexists with other forms of knowledge. We see the political task not as "sharing" knowledge with those who lack it, but as forging links between *different* knowledges that are possible from different locations and tracing lines of possible alliance and common purpose between them. In this sense, we view a research area less as a "field" for the collection of data than as a site for strategic intervention.

The idea that anthropology's distinctive trademark might be found not in its commitment to "the local" but in its attentiveness to epistemological and political issues of location surely takes us far from the classical natural history model of fieldwork as "the detailed study of a limited area." It may be objected, in fact, that it takes us *too* far—that such a reformulation of the fieldwork tradition leaves too little that is recognizable of the old Malinowskian archetype on which the discipline has for so long relied for its self-image and legitimation. At a time of rapid and contentious disciplinary change, it might be argued, such a reworking of one of the few apparently solid points of common reference can only exacerbate the confusion. But what such worries ignore is the fact that the classical idea of "the field" is *already* being challenged, undermined, and reworked in countless ways in ethnographic practice, as several of the chapters in this book, along with other works discussed in this chapter (and in chapter 10) illustrate. An unyielding commitment to the virtues of an unreconstructed Malinowskian "field" cannot reverse this transformation, though it can do much to misunderstand it. Indeed, if, as we have suggested, much of the best new work in the discipline challenges existing conventions of "field" and "fieldwork," the refusal to interrogate those conventions seems less likely to prevent disciplinary confusion and discord than to generate it. Like any tradition valued by a community, anthropology's fieldwork tradition will manage to secure its continuity only if it is able to change to accommodate new circumstances. For that to happen, as Malinowski himself pointed out, such a tradition must be aggressively and imaginatively reinterpreted to meet the needs of the present.

NOTES

1. Here and throughout this chapter we use "anthropology" as a shorthand for sociocultural anthropology, leaving to one side the very interesting issues raised by the roles of "field" and "fieldwork" in the other subfields of anthropology: archaeology and biological anthropology.

2. Our focus on what one might call the hegemonic centers of the discipline is deliberate and motivated. Since we are concerned, above all, with the mechanisms through which dominant disciplinary norms and conventions are established, we believe there is good reason for paying special attention to those institutional sites and national contexts that, in practice, enjoy a disproportionate say in setting theoretical and methodological agendas and in defining what will (and will not) count as "real anthropology," not only in the U.S. or U.K. but throughout the anthropological world. This choice of focus is not intended to diminish the importance and vitality of a variety of peripheral, heterodox, or subordinated sites and contexts of anthropological practice, which we discuss briefly in Part IV of this chapter. The point, on the contrary, is to explore how and why such alternative traditions have been marginalized and ignored, and with what consequences.

3. The survey is cited in Stocking 1992a: 14.

4. The observation that peoples and cultures are nowadays less localized is not meant to imply that in the past, groups were somehow naturally bounded, anchored in space, or unaffected by mass migrations or cultural flows. As we will emphasize later, processes of migration and cultural "diffusion" are far from new, and anthropology has a long (if often underappreciated) history of attention to them (cf. Gupta and Ferguson, eds., forthcoming).

5. It can be argued that the inherited division of conventional academic disciplines is part of the problem here, pressing the intellectual practices of the present into the Procrustean bed of outdated conceptual categories. This is certainly the case with respect to anthropology's perennial embarrassment over the issue of the (non)unity of its "subfields." The periodic trumpeting of the virtues of an "integrated," "holistic," "four-field" anthropology cannot disguise the obvious fact that the lumping of social and cultural studies of Third World peoples together in a single discipline with such things as behavioral studies of baboons and archaeological excavations of human fossils can only be understood as a legacy of nineteenth-century evolutionist thought, persisting (as a "survival," one might say) only thanks to the ossified institutional structure of the modern university. Indeed, Boas himself understood the shape of the anthropological discipline as a historical accident originating in the fact that "other sciences occupied part of the ground before the development of modern anthropology" (Stocking, ed., 1974: 269). The "four-field" structure, he predicted, would be dissolved in time, once other sciences such as linguistics and biology matured to the point where they would deal with "the work that we are doing now because no one else cares for it" (Stocking, ed., 1974: 35; cf. Stocking 1988).

It should be noted, however, that the predicament of finding one's disciplinary bounds at odds with current thinking is not unique to anthropologists. As Kuklick (chapter 2) points out, it was institutionalization in universities that gave *all* the disciplines the mixed blessing of stability, "imparting to each field the quasi-natural status that has become increasingly problematic for virtually all of them." But if the *form* of the disciplinary division of labor is, thanks to such institutionalization, fairly fixed, its *content* is not. Because disciplinary traditions and subject matters are continually reworked and reinvented, quite fundamental changes can occur even in the absence of disciplinary reorganization.

6. Again, we concentrate here on the dominant Anglo-American tradition (cf. note 2).

7. Kuklick is discussing the British tradition, which is essential to grasping the roots of the "Malinowskian revolution." The American Boasian orientation to the field was significantly different, however, as will be discussed below.

8. Vincent (1990: 106) has argued that the "fieldwork revolution" preceded Malinowski's self-promoting "discovery" of it, and might more properly be credited to Rivers. On the American side, a key role in the development of "fieldwork" has often been attributed to Boas, while Lewis Henry Morgan's researches among the Seneca provide an even earlier point of reference. A more complete account would also have to include (among many others) figures such as Henry Rowe Schoolcraft, Frank Hamilton Cushing, and Ely Parker. Kuklick (chapter 2) shows more fundamentally that the turn to field observation was not a uniquely anthropological move at all, but part of a general development within *all* of the natural history sciences in the late nineteenth and early twentieth centuries. However, as Malinowski would surely agree, foundation myths need have no necessary relation to actual historical sequences. Since our concern here is more with the received tradition of fieldwork than with its actual genesis, we are content to continue speaking of the "revolution" as Malinowskian (cf. Stocking 1992a: 281).

9. Akhil Gupta wishes to thank Marilyn Ivy for the stimulating conversation within which some of these ideas first arose. "The field" has, of course, other connotations as well; most interestingly, perhaps, the idea of a "field" of interacting forces, as in physics, as Roger Rouse and Emily Martin have both pointed out to us. Yet anthropology's "field," it seems to us, has more often been grasped as a place of terrestrial concreteness than as an abstract space within which invisible forces might meet. Anthropologists going to the field expect to get mud on their boots; like other "field scientists," they have aimed to discover not disembodied fields of force, but a reality repeatedly described by such adjectives as *messy, flesh-and-blood,* and *on-the-ground.*

10. As Thomas notes, the fact that "there is virtually no discussion now of what regions are, [and] of what status they are supposed to have as entities in anthropological talk" (1989: 27) shows not that anthropology no longer relies on culture areas, but that it relies on unacknowledged, untheorized, and taken-for-granted territorializations of cultural difference. The uncritical use of such mappings, Thomas shows, may unwittingly perpetuate evolutionist and racist assumptions inherited from the colonial past. For an attempt to locate an empirical basis for the division of the world into culture areas, see Burton et al. (1996).

11. It seems to be the case that doing fieldwork in Europe is much more acceptable in anthropology when it is a second field site developed later in the career, rather than a dissertation site (see the discussion of fieldwork in the United States, below).

It is also true that southern and eastern Europe seem to be distinctly more "anthropological" than northern and western Europe. Herzfeld (1987) shows that the "anthropological-ness" of Greece, like its "European-ness," is historically variable and subject to contestation and debate.

12. It does not follow from this that Evans-Pritchard therefore worked in the service of colonial rule—that is a different proposition requiring independent demonstration.

13. We realize that these categories are not as neatly opposed as this formulation might seem to imply. Much of the creation of knowledge about Third World nation-states continues to occur in, and through, former colonial centers.

14. We use the term *visa procedures* here as shorthand for the whole complex of mechanisms used to regulate the production of knowledge within and about nation-states.

15. We are reminded of Bellah et al.'s analysis (1985) of the systematic patterns by which people fall in love, each supposing their love to be entirely unique.

16. We borrow the term *archetype* from Stocking, but it should be noted that we develop it in ways that probably depart from his intended meaning.

17. Visweswaran (1994: 95–130) has discussed this constrast.

18. "Even in the absence of a separate autobiographical volume, personal narrative is a conventional component of ethnographies. It turns up almost invariably in introductions or first chapters, where opening narratives commonly recount the writer's arrival at the field site, for instance, the initial reception by the inhabitants, the slow, agonizing process of learning the language and overcoming rejection, the anguish and loss of leaving. Though they exist only on the margins of the formal ethnographic description, these conventional opening narratives are not trivial. They play the crucial role of anchoring that description in the intense and authority-giving personal experience of fieldwork. . . . Always they are responsible for setting up the initial positionings of the subjects of the ethnographic text: the ethnographer, the native, and the reader" (Pratt 1986: 31–32). See the thoughtful discussion of anthropological arrivals in Tsing (1993).

19. The phrase "writing up" is itself suggestive of a hierarchy of texts mapping itself onto a hierarchy of spaces. One "writes up" the disjointed, fragmented, immanent text found in fieldnotes into something more complete and polished. One also "writes up" in a space that is superior, more conducive to reflection and the higher arts of theoretical and mental work.

20. A survey of job ads for sociocultural anthropologists that appeared in the *Anthropology Newsletter* between September 1994 and April 1996 showed that most advertised positions (100 out of 178) specified preferred geographical areas (25 Asia, 37 Latin America, 37 North America, 15 Sub-Saharan Africa, 10 Caribbean, 3 Middle East, 3 Oceania, 2 Europe), while another 11 specified a geographical area negatively (e.g., "non-West" or "non-U.S."). (Note that the figures for the different areas add up to more than the total number of area-based positions, because some jobs mention more than one area.) Of the positions, 65 did not refer to area, and 2 referred to specific diasporic groups.

21. In the survey discussed in note 20, we found that of the 37 ads that included a call for a North America area focus (sometimes as one of several possible areas), 16 specifically called for a specialization on Native Americans. Another 10 requested African American specialists, along with 2 for Asian American specialists, and 1 for Asian American / Chicano. Of the 8 remaining positions, 4 were described in regional terms (e.g., "Southeastern U.S.," "U.S. Southwest"), leaving only 4 jobs that were unqualified by ethnic or regional descriptors. These results are generally consistent with those of another, slightly different employment survey carried out by Judith Goode of the Society for the Anthropology of North America (SANA 1996), which found that out of 730 job listings (all subfields) sampled between 1986 and 1994, 64 were specifically designated as North Americanist positions, of which 45 required specialization on a specific U.S. ethnic group (SANA 1996: 31).

By pointing out such hiring patterns, we do not mean to imply that anthropologists should not focus on Native Americans or minority groups, but only to insist that the casting of the anthropological net to include sites ranging from "Samoa to South Central" (as a recent anthropological video catalogue from Filmmakers Library put it) does not displace the old conventions that locate the subject matter of anthropology in terms of white, Western, middle-class alterity. (The no-doubt-unintended primitivizing effects of such disciplinary definitions are made particularly clear when we open the Filmmakers Library brochure and find the "South Central" film located just opposite the "Primate Social Behavior" section.)

22. The "top ten" departments were taken from the recent National Research Council study of U.S. doctorate programs (Goldberger, Maher, and Flattau 1995: 475) and included: Michigan, Chicago, Berkeley, Harvard, Arizona, Pennsylvania, Stanford, Yale, UCLA, and UCSD. For each department, we counted all social-cultural anthropologists (including linguistic anthropologists) listed in the *AAA Guide to Departments of Anthropology* as "Full-time Faculty"—including joint appointment faculty, but not "Anthropologists in Other Departments" or courtesy (secondary) appointments. We found a total of 189 social-cultural anthropologists, of which 184 stated area specializations. We found that 23 of these anthropologists listed North America or the United States as their primary area (i.e., listed it first, in cases of more than one area focus), of which 15 could be determined to be specialists on Native Americans, leaving 8 primary specialists in nonnative North America. We found an additional 26 anthropologists who listed North America or the United States as a secondary area interest (i.e., listed it, but did not put it first).

23. Some who have ventured to examine the mass media ethnographically are: Heide 1995; Ang 1985 (1982); Morley 1980, 1986; Powdermaker 1950; Seiter et al. 1989; Abu-Lughod 1993; Mankekar 1993a, 1993b; Dickey 1993; Spitulnik 1994. See Spitulnik 1993 for a full review and discussion.

24. The reason for this historical tendency, we suggest, cannot simply be that such supralocal political identifications have developed only recently. For instance, during Robert Redfield's classic 1927 fieldwork (to take only one of many possible examples), the ethnographer witnessed "Bolsheviks" fighting in the streets of Tepoztlan as part of a Zapatista uprising, and he described the local people he knew as "very Zapatista in sentiment." But we know this from his personal papers and his wife's diary; his ethnography painted a very different picture of peaceful villagers living local lives with little interest in national or international politics (Vincent 1990: 206–207).

25. In posing this question, we do not mean to imply that there are not often excellent reasons for choosing to work in villages. Indeed, we have each carried out vil-

lage-level fieldwork in our own studies and appreciate fully the methodological opportunities and advantages often provided by such settings. Our point is only to question the conventional mapping of "field site" onto exotic "local community" that is so economically expressed in the archetypal anthropological image of "the village."

26. Indeed, even much older communications technologies such as telephones remain strikingly underresearched in anthropology, as Orvar Löfgren has pointed out recently (Löfgren 1995).

27. This does not mean, of course, that judgments of excellence cannot or should not be made, but only that (1) such judgments must always be made in terms of standards and principles that are never the only ones possible, and (2) every choice of a set of standards and principles for judgment will have social and political implications; the "grid" will not in this sense be "neutral."

28. The term is Kirin Narayan's (cited in Abu-Lughod 1991).

29. It is also interesting to note how few Africans are involved in the anthropological study of Africa. Jane Guyer (1996: 30) has recently surveyed the percentage of dissertations on Africa written by African-surnamed authors, and found that of eighteen surveyed disciplines, anthropology had by far the lowest percentage of African authors (only 18 percent of anthropology dissertations on Africa were written by authors with African surnames, compared with, e.g., 54 percent in political science, 70 percent in sociology, and 33 percent in history). See also her thoughtful remarks on the fieldwork tradition and its future in African studies (1996: 78–80).

30. Bell, Caplan, and Karim (1993) explore the myriad ways in which supposedly gender-neutral norms of fieldwork clash with highly gendered actual experiences of fieldwork.

31. In our discussion so far, we have not even touched on those micropractices of the academy that screen candidates in the name of "collegiality" and "suitability" for class, race, and sex (see Rabinow 1991).

32. We are grateful to Anna Tsing for pointing out to us the importance of exploring heterodox traditions of "the field."

33. Kuklick argues (chapter 2) that "a neglect of comparative, historical analysis" accompanied the rise of fieldwork not only in anthropology but in all of the field sciences. Anthropologists, she suggests, "might derive some consolation from the knowledge that the turn to the synchronic was not their field's alone."

34. Mead herself, of course, was also a leading figure in the study of "acculturation" and "modernization," especially (but not only) in her later work.

35. The kindred distaste that mainstream anthropology shows for the similarly "impure" field of "development anthropology" is analyzed in Ferguson forthcoming.

36. The period of the 1930s and 1940s saw a good deal of politically engaged work on "social problems" in British anthropology as well. Godfrey Wilson's "Essay on the Economics of Detribalization in Northern Rhodesia" (1941–1942), for instance, was a precocious analysis of labor migration, rural poverty, and what would later be called "underdevelopment" in a colonial setting, which insisted on linking poverty and famine in rural northern Rhodesia both to urban mining development and to a wider world economy. Works such as this certainly challenged the prevailing assumptions of academic anthropology in a number of ways. Since most of the British studies of "culture contact" and "social change" were set in "the colonies," however, they did not call into question the "home"/"field" division in the same way that work on acculturation and poverty in the U.S. did. (Note, however, that some of the work on "culture contact" in South Africa had a similar blurring effect. For white South African anthropologists, the "field" that contained the "pure" natives was safely off in the reserves, but in the study of acculturation, "the field" came much closer to home. See Monica Hunter Wilson's extraordinary monograph in the "culture contact" tradition, *Reaction to Conquest: Effects of Contact with Europeans on the Pondo of South Africa* [1936].)

37. Radin's study was funded by the California State Emergency Relief Administration (SERA), Project 2-F2-98 (3-F2-145), Cultural Anthropology.

38. Radin's lifelong study of the Winnebago Indians left us with what has been called "perhaps the most complete and detailed long-term record in monographs and field notes that we have of a primitive society as seen by a single observer through all the stages of his own intellectual history" (Vidich 1966 [1933]: xiv). Radin chided Margaret Mead for drawing ethnographic conclusions on the basis of less than a year of fieldwork that would properly require "a long and protracted residence and a complete command of the language." A year or two, he suggested, was not nearly long enough for deep cultural understanding: "What one gets within a year, or for that matter within five years . . . is bound to be superficial" (Radin 1966 [1933]: 178–179).

39. The recent work of Rosaldo (1993, 1994) addresses similar questions regarding the political implications of heterodox anthropological methodologies.

40. Mafhoud Bennoune has described coming to the U.S. in the early 1970s to study anthropology from a background as an Algerian revolutionary and finding that his plans to study "the causes and consequences of labor migration" of Algerian workers to France were frustrated by being forced into a "community study" model. Bennoune recounts how the director of the research center with which his dissertation research was affiliated (and the "manager" of his Ford Foundation funds) demanded that he focus on firsthand observations within a community ("Mohammed A. and Mustafa B. and Musa C."), while giving documentary research only "very secondary consideration." Bennoune understood his director to be ordering him "in a very explicit manner to study only a small group of migrant workers in complete isolation from the historical, social, and economic context of colonialism and imperialism" (Bennoune 1985: 362–363).

41. Dipesh Chakrabarty (1992: 2) has pointed out a similar situation in the field of history, where historians of Europe feel no need to refer to non-Western, Third World histories: "'They' produce their work in relative ignorance of non-Western histories, and this does not seem to affect the quality of their work. This is a gesture, however, that 'we' cannot return. We cannot even afford an equality or symmetry of ignorance at this level without taking the risk of appearing 'old-fashioned' or 'outdated.'"

42. The interaction of metropolitan anthropology with the discipline's local representatives in peripheral settings is complex. Local anthropologists may exercise varying degrees of influence on the topics and methods used by Western ethnographers. But intellectual production in many such settings is itself heavily colonized. Discrepancies of funding and resources also endow First World ethnographers with distinct advantages in the space of representation. For example, graduate students funded from U.S. sources and doing fieldwork in India are paid at least twice as much as full professors in Indian universities. Journals in which First World ethnographers publish are not available in most libraries, and are much more expensive to subscribe

to from foreign countries, even poorer ones. In 1991, libraries in New Delhi cut back journal runs because the new fiscal regime imposed by the IMF raised the exchange rate, making journals prohibitively expensive. These circumstances make a mockery of the notion that the space of representation can be a truly dialogic one. Indian anthropologists have complained, for example, that First World ethnographers who pay large sums (by local standards) to "informants" effectively prevent any native ethnographers from working in the vicinity, as certain expectations of payment are set which local scholars are unable to meet.

43. De Lima (1992) has given a vivid account of the encounter between Brazilian and U.S. anthropological norms in the context of his own graduate education. While he does not discuss practices of the field, he has a great deal to say about the way that taken-for-granted and supposedly "neutral" academic forms—from "clear," "well-structured" essays and carefully timed oral presentations read from written texts to formal job searches ("they look for a professor the same way they look for a roommate")—in fact work to enforce American cultural premises.

44. We are convinced, for example, by Tishkov's critical appraisal (1992) of Soviet ethnography, which laments the lack of extended fieldwork among a younger generation of scholars. Yet Tishkov's essay also makes one realize how acutely the hegemony of Anglo-American norms is felt: "In world anthropology, at least a year's fieldwork with a community or group is considered the norm for everyone from the postgraduate student to the leading professional" (1992: 374).

45. Since we have relied entirely on Geertz's description of the encounter between the Harvard team and their Indonesian counterparts, we may have been misled by the dramatic presentation of the episode to overstate the apparent lack of negotiation between the Americans and their hosts.

46. Another important "border" of this kind is one that separates anthropology from journalism. This is explored at some length in chapter 4.

47. For a small sample of the work of these scholars, see:

a. Hurston 1935, 1969 (1942), 1978 (1937); Hernandez 1993; Dorst 1987;
b. Paredes 1958, 1993; Rosaldo 1987;
c. Du Bois 1967 (1899), 1961 (1903), 1964 (1935); see also the excellent special issue of *Critique of Anthropology* (Harrison and Nonini, eds., 1992);
d. James 1963 (1938), 1969, 1983 (1963); Grimshaw and Hart 1991;
e. Drake 1966, 1987, 1990; Drake and Cayton 1993 (1945); Harrison and Nonini, 1992.

48. The status of "insider" is of course a complicated matter, since there are as many ways of being "inside" or "outside" as there are of defining a community (cf. Hurston 1935; Bell, Caplan, and Karim 1993; Narayan 1993).

49. Deborah D'Amico-Samuels (1991: 75) has put it very well: "The real distancing effects of the field are masked in the term 'back from the field.' These words perpetuate the notion that ethnographers and those who provide their data live in worlds that are different and separate, rather than different and unequal in ways which tie the subordination of one to the power of the other."

50. On the politics of location, see Rich (1986), Anzaldúa (1987), Spivak (1988), Pratt (1984), Martin and Mohanty (1986), Reagon (1983), Wallace (1989), Haraway (1988), Lorde (1984), Kaplan (forthcoming), Nicholson (1990).