

FEMINIST DILEMMAS IN FIELDWORK

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The Expeditions of Conjurers: Ethnography, Power, and Pretense

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TALK ABOUT GEOGRAPHICAL FIELD RESEARCH conjures up explorers, chart makers, and expeditions to far-flung lands. The geography of maps, charts, and *terrae incognitae*. When I was looking for dissertation funding in 1980, I was dazzled by the research budget of the National Geographic Foundation. My proposal for less than \$10,000 to do an ethnography of children in rural Sudan was stacked up with (and unfavorably compared to) polar expeditions, space explorations, and ocean-floor mapping projects. I hadn't realized that "real" geography was a million-dollar enterprise. Romantic, expensive. Military.

Talk about ethnographic field research conjures up proper sweaty westerners "going native" in varying degrees of disarray. There was nothing urban, local, or domestic in the mythology of ethnography despite a history lasting several decades. Ethnography itself seemed a colony of anthropology, which continues to wage a losing battle to protect its disciplinary frontiers, in part by defining what proper ethnography entails. When I first went to the field, I felt free to ignore these codifications because of geography's loose affiliation with ethnography as such, and foundations funding ethnography, such as the Wenner-Gren Foundation and the Social Science Research Foundation, felt free to avoid me. My nontraditional approach was Janus-headed—I felt free to do an ethnography of the acquisition and use of environmental knowledge but was perennially insecure about what I was leaving out by not systematically addressing kinship patterns and the like.

Finally, talk of science, even social, even now, and research, even ethnographic, continues to conjure up a masculine presence. Despite a few star women in the field—indeed the name of Margaret Mead was virtually synonymous with anthropology in the lay imagination—it remained difficult to find women in my field who could serve as mentors or role models. And although this is changing, with many recent books celebrating the regendering of science and the academy,

women remain marginalized in science, and our practices in the field (understood here both as discipline and as site of research) seem to require more negotiations than those of our male colleagues.

Talk of conjuring conjures up witches, jugglers, enchantments, and conspiracies. The possible worked out of the improbable. Under "conjurer," the *Oxford English Dictionary* offers the tantalizing, "professor of legerdemain"; for "legerdemain" it suggests "sleight of hand, the performance of tricks, deception, hocus pocus." In the crossroads of improbable possibilities and the performances of deception are some of the dilemmas of doing fieldwork. In this chapter I discuss the tightrope I walk between feminist geographer and "professor of legerdemain"; between the politics of fieldwork and fieldwork as politics.

I take as my starting point that social relations are ordered by the social construction of boundaries—political, economic, social, physical—over time and space; and contemporary social relations of production and reproduction, which are hierarchical and uneven, are oppressive and exploitative to most people in most places. The intersecting trajectories of these oppressions are well known. They include class, race, gender, nation, sexual orientation, and age. My political work, as a scholar and otherwise, is concerned with undoing these relations. Ethnographic work is a means of transgressing boundaries and thereby offers itself as a kind of politics. In this chapter I explore my efforts in this realm with an eye on their contradictory relation to the larger political project of transforming oppressive and exploitative social relations and suggest a direction for field research that offers the possibility of more direct political engagement. I begin by simultaneously tracing the projects in which I have been involved and the practical, theoretical, personal, and political dilemmas they have raised.

A Chronology, Some Confessions

In 1981 I conducted ethnographic field research in a village in central-eastern Sudan that had been included in a state-sponsored agricultural project a decade earlier. My focus in this project was the acquisition and use of environmental knowledge by children. This knowledge—of agriculture, animal husbandry, and the use of local resources—was central to the production and reproduction of everyday life in the village. I was interested in discovering not only what knowledge was passed on and by whom, but also in what ways its production and exchange were altered in the face of imposed socioeconomic and political-ecologic change. I was looking for resistance, possibly—as the song goes—in all the wrong places. The project was supported by a dissertation grant from the National Science Foundation. The grant included funds for my partner to document the children's activities of work and play on Super-8mm film.

The dilemmas of representation, "othering," and intent were raised by this work. These dilemmas were intertwined like the tightly wrapped braid I wore during much of the field research, and like that braid needed frequent unraveling,

combing out, but ultimately rebraiding, in order to keep working. Like most ethnographies, the project was riddled with thorny questions of representation—who were we to go there to tell our stories in their names; how did our self-representations work for and against theirs? The project reiterates the uneven power relations between the United States and Sudan, between colonizer and colonized, that have become commonplace in the critique of ethnography, even as its intent was to undo these relations. As I have noted previously (Katz, 1992), I recognized the uneven power relations involved in doing this research in a rural setting of the so-called Third World, but I thought the meanings and practices of social reproduction would be more visible where they were threatened by the immediate impress of a transforming rural capitalism. As Marx suggested in a discussion of “the colonies” in volume 1 of *Capital*, the contradictions of capitalism may be witnessed more clearly at the margins. More importantly, I reckoned that if capital accumulation was a global process with multiple local forms, it was important to identify the range of oppositional practices to it. By investigating some of these local-global practices I thought that I might establish connections between them and help to build a politics that engaged capitalism in its multiple historical geographies. My sense was (and is) that “we” were already “there,” and our presence created—perhaps even demanded—a space for anti-imperial ethnography.

This awareness and sense of mutual engagement in common problems differently experienced does not obviate the uneven power relations that are summoned and deployed in the enactments of such projects. It was, after all, I who went there and my research agenda that was carried out, and when I was finished I left. Such moves reflect power no matter what their broader intent is and no matter how deep are the feelings engendered in the process.

The politics was intensely personal, and my willingness to be untruthful for strategic reasons concerns and sometimes troubles me. How else can I understand my self-representation and the assumptions it made of the participants in my research? For example, I said I was married to my partner because I assumed our cohabitation would be interpreted as sinful and my work would be undermined. I, of course, was able to make relative moral judgments about their practices, but by my concealment I assumed that they were not capable of the same.¹ In a similar vein, as an anti-Zionist and an atheist Jew, I decided to pass as a Christian so as not to impede my research. I assumed that revealing I was Jewish to the uniformly Muslim and Arab-identified population of the area would create suspicions that I did not want to carry, especially given my deep and long-standing opposition to Zionism. Clearly I unfairly presumed a lack of subtlety on their part, but the strategy was also almost unforgivably lazy. In the United States I am willing to labor over the distinction between Jew and Zionist. Why not there? Again, I explained my choice to myself as strategic—just as I stopped being an always gracious host after a few weeks in the village (and got a kerosene, rather than charcoal, stove for when I was). I felt I did not have time both to engage in deliberations on Zionism and to get my research done. This was disingenuous. I feared that such self-disclosure

might have undermined my work, and I was not willing to take that risk. The choice was surprisingly painful. Not only might I have had interesting political discussions with people in the village but I wasn't very good at being a Christian. I often felt sad because I could not talk about the many practices common to Judaism and Islam (especially when for many, these practices were more important than the political-economic relations I was so eager to discuss). Finally, I was evasive about the uneven economic relations between me and the people with whom I worked. My research grant was small, but was much larger than the average annual household income reported to me in my villagewide survey. There was keen interest in my financial affairs, and I often stumbled as I tried to explain that although I seemed to have a lot of money, I was a poor graduate student. The metric by which this was the case encapsulates the uneven power relations between me and the people in that village. All of these evasions and deceptions were strategic—to ensure the smooth operation of the research. Yet these were the very areas in which I expected people to be honest with me. Although my research did go well on many levels, I wonder how my strategic dishonesty undermined me and my project and whether it really did ease my way and facilitate the work.²

As I unraveled this braid of dilemmas I was determined not to be unraveled by them because of the larger political intent of my project. My intent was to uncover the practical responses to capitalism in the everyday practices through which knowledge was produced, shared, and used. As I have suggested, I saw myself engaged in similar oppositions and in this way connected my own work to those practices that were the focus of my inquiry. This connection seemed to undercut some of the exoticizing aspects of the project by locating me and the people in the village where I worked in an arena of struggle with some common ground. I do not underestimate our different stakes in that ground. But even this discovery raises a set of questions. I was concerned with resistance. I could not abrogate the fact that I defined the terrain of concerns and sought answers for myself in the practices of others. Moreover, while I think my concern with the practices of everyday life made sense to the people with whom I worked, I was caught up with uncovering resistance in the work knowledge parents shared with their children rather than where it might have been more readily located—in their religious teachings and practices. My insistence on finding particular patterns of resistance that might inform my own political practices probably blinded me to some of the more resilient—albeit discomfiting to me—sites of resistance.

In 1983 and 1984 I was back in Sudan working for CARE, a nongovernmental organization that was engaged in a reforestation effort intended to assist Eritrean and Ethiopian refugees. My task was to develop a program of social and agroforestry extension that would ensure the project goals for revegetation and livelihood assistance. I conducted ethnographic field research in the refugee settlements and their surrounds in order to ensure that extension activities addressed potentially conflicting political-ecologic needs and concerns among the major

land use groups in the area, which apart from the refugees included Sudanese pastoralists, peasant farmers, and large-scale cultivators. I organized the program of research and carried it out with research assistants drawn from the refugee communities. The extension program itself was developed by my Sudanese counterpart, a rural sociologist with expertise in agricultural extension, and me, and was supervised over the five-year life of the project by him. My work in this project had ample funding and infrastructural support from CARE, and the experience of eliciting people's participation in the research with the promise of trees felt very different from the abstract promises of my doctoral research. Somehow the tangibility of the trees, which were almost a direct compensation for talking with me and my assistants, made the rhetorical promises of political solidarity that were part of my previous research seem that much more flimsy. The CARE project raised other concerns, and these had more to do with my practices as a scholar and professional. The project was considered a success by both participants and external evaluators. Part of its success was attributable to the intricate and labor-intensive extension program I developed. Yet I have never published anything about the work or other applied projects with which I have been engaged because this work lacks currency in the academy. The split between theory and practice and the privileging of theory are hallmarks of bourgeois and masculinist science. Our silence on such productive engagements in the field, then, is yet another dilemma of fieldwork that lets others define what counts.

When I moved to New York in 1986, I stopped working in Africa and was determined to work where I lived. I had several reasons for this: There were many problems in New York and my political commitments compelled me to address them; I was suspicious of my exoticizing impulse; and I was feeling fragmented by the distances between the multiple locales of my work. In 1987 I began mulling over the relation between the displacements suffered by children in rural Sudan and those experienced by working-class children in New York City. The consequences of capitalist agricultural development on the one hand and deindustrialization on the other appeared to have startlingly similar effects on children—including diminished prospects of meaningful work, dislocations of and depredations on sites of social reproduction, and deskilling leading to disqualification for all but the most marginal jobs. In one way or another this concern for children's displacement in its transnational guises has been at the heart of my work since 1987, and I have addressed it in New York while working on other projects. I have yet to undertake the long-term companion ethnography on children in New York I have promised myself and others I would do. The very dilemmas of fieldwork addressed in this volume have deterred me from completing this work. Without belaboring the point, these dilemmas include the fact that while I have moved the site of my research closer to home, it remains that most of my work in New York has been among working-class populations in central and East Harlem. I am not working class, Latino, or black, while most of those with whom I work are. I simultaneously question my decisions to keep the focus of my inquiries at

given the crises facing these groups and the need to build a broad-based political response to them. This, of course, puts me on the tricky borders attaching research, practice, advocacy, and activism.

In 1988 my colleague in the Children's Environments Research Group at the City University of New York was approached by the schoolyard committee of a school district in upper Manhattan to design an early childhood play environment. In a presentation that showed the kind of play environments our group has advocated and the participatory design strategies we favor, he convinced the committee to try to build a larger schoolyard that would provide an integrated play environment for children from preschool through sixth grade. Our group secured funding from the Aaron Diamond Foundation to develop a participatory design for the community schoolyard. We conducted a fairly spare ethnography of the schoolyard during and after school hours; elicited design ideas from almost all of the children in the school; debated competing design ideas over three-dimensional models of the yard among parents, teachers, administrators, staff, and children—separately and together—and presented the collective design in a kiosk on the street in front of the school so that community members and others could comment upon it. The design program generated through these participatory activities was used for an international design competition open to architecture and landscape architecture students. The winning design—selected by a committee that included the assistant principal of the schools in question and the deputy superintendent of schools in the district—was used as the basis for the design that only now, four years later, is about to be built.

Through the vicissitudes of working with overlapping bureaucracies, competing egos, and a long period of financial retrenchment in New York City we managed to develop a design that reflected the needs and desires of all involved. The research and design processes, together with their many compromises, were painstakingly negotiated. Yet here again I have barely published a word despite the value of this kind of work to the design professions, if not to social science as presently constituted. Apart from the dilemma of "what counts" for whom, this project raised more specifically the problems of advocacy, activism, and commitment in research. Although the schoolyard project was initiated by a school district committee and our approach was always participatory, I do not consider this project exemplary of action research. It was not community driven in its entirety, and its goal—a schoolyard—was predetermined, albeit by a local committee. Although the community voiced its support of schoolyard change as a priority, it is not clear whether if the work had been truly open-ended they would have chosen a schoolyard as the first project.³ In addition, the project has been frustrating for all involved largely because of the relentless stonewalling by the Board of Education, which had to release funds and sign off on the project if the yard was to be built. I have been determined to see this project through to completion and have attended countless meetings and made modest donations of my own funds to that end. But I have often been inattentive in ways that at best slowed things down and at worst caused missed opportunities. Engaging in participatory com-

munity research directed at change is time-consuming and extremely frustrating. There are few rewards for these efforts in the academy and several punishments—years can go by without “results” and time that might be spent publishing is spent “perishing.” The appreciation of my efforts by those at the school and in the community does not ease my conscience. My efforts are uneven and that—no matter what the reason—is inappropriate for committed research engagements.

The same year that the schoolyards project began, a group of cultural studies researchers at the City University of New York developed an alternative field project that became known as CAMEO (Community, Autobiography, Memory, Ethnography, Organization). Beginning in 1991, CAMEO members based at the university worked with community residents in East Harlem (El Barrio) in Manhattan and in Williamsburg in Brooklyn on issues of community concern using autobiography, memory, and ethnography as organizing strategies. We divided into two groups largely and (probably not) accidentally along gender lines. Graduate students and community residents worked together as paid research partners, and research projects were developed collectively in each group. I was part of the El Barrio group in which, among other things, we collected several oral histories that wove together memories of everyday life in East Harlem and their connections to present concerns and practices; began an ethnography of children's everyday lives on a neighborhood block; worked on a place history of La Marqueta, which had been until a few years before a thriving market and focal point for the community and now was a struggling shell; and began to develop a social history of the Center for Popular Education. Some CAMEO participants also mounted a museum exhibit that highlighted local cultural forms and practices for the consumption of wider publics. After two years of sporadic engagement on my part I felt I had developed productive working relations with our research partner, who was a neighborhood community leader, and with some of the children who lived on her block. The graduate students in our group—all native speakers of Spanish—worked in the neighborhood more consistently and had developed closer working relations with more community residents and were also working as occasional teachers at the Center for Popular Education.

I had every opportunity to engage in an extended ethnography of the practices of children's everyday lives within the rubric of CAMEO, but I did not. This was only partly attributable to conflicting time commitments to other work. The reason has been mostly a nagging uncertainty about what was to be gained and for whom. Some of my concerns were raised by the first project in Sudan and do not find any easier resolution. In East Harlem I have addressed the theoretical and practical issues concerning the displacement and deskilling of children by the predations of a restructuring capitalism, but I have not done the detailed ethnography. Most of what would be gained by doing the fieldwork now has everything to do with my career and little to do with the communities where I work. I am skeptical of such productions of knowledge and fearful that they would expose the practices of those with whom I work to those who have an interest in controlling or suppressing them.

My work on this chapter has provided a path out of this dilemma. Rather than conduct the (long forestalled) ethnography of the children's everyday practices, I have concluded that the only viable project would be one of critical environmental education wherein the children's engagement in researching their own neighborhood would enable them to define the problems to be addressed and the means of addressing them. I would offer my skills as a geographer and a teacher to assist them in finding ways to address the questions of concern to them and in these ways would not only discover what issues are important to young people but would engage in a collective process that reworks science itself. My inspiration for this endeavor is drawn in part from the work of the Society for Human Exploration discussed below, which promulgated a notion of research as itself emancipatory.

The Politics of Fieldwork

Each of these endeavors raised a host of dilemmas; only a few of them are raised here, and they are never more than partially resolved. I participate in this critical self-reflection here and elsewhere to show that I am not sounding an anti-intellectual cry for ethnographic business as usual or that I have no guilt. Rather, I want to suggest that the ethnography of ethnographers is reaching an unproductive internal vanishing point. It is time to live and work with and against the contradictions rather than employing self-confession as a barrier against them. Social scientists who engage in field research must find noninnocent (self-reflexive and clearly positioned) ways to work in the world such that we can at once uncover common bonds and recognize differences. If scholars and other practitioners committed to social and economic change promising greater equity and social justice retreat only to speak about themselves or otherwise self-consciously undermine their projects, there will be little hope for the development of new forms of engaged critical scholarship.

This book, and other recent books, surveys critically the politics of fieldwork (Wolf, Chapter 1). My colleagues have addressed many of these issues with great integrity and insight. Here I want to question the politics of fieldwork more closely and explore the possibilities of moving toward a more explicitly politicized research practice. In this task I am multiply positioned as a geographer, feminist, social scientist, and activist. I focus on a small, distinguished, but insufficiently recognized part of the history of geographical field research, the Detroit and Toronto Geographical Expeditions of the late 1960s and early 1970s, because they suggest inspiring, if problematic, strategies to rework committed field research.

Geographic research has long been associated with imperial interests and military conquest (Godlewska and Smith, 1994; Livingstone, 1992). In the United States, if not in Europe, its field practices were traditionally concerned with surveying, charting, and resource assessment more than with social practice. When

the latter was the focus, the trajectory of research was often descriptive, environmental determinist, or, more recently, concerned with how people adjusted to environmental challenges such as way-finding, spatiotemporal constraints, or drought. A fieldwork tradition concerned with the sociospatial practices of people's everyday lives, or with people's productions of space, place, and nature as constitutive of social life, has developed in U.S. geography largely since the 1970s. It remains relatively unusual to engage in ethnographic field research in contemporary geography, although of course it was a staple of the European imperial traditions. Whereas anthropologists engage in relentless autocritiques of the practices of ethnography, ethnographically inclined geographers must still defend these methods within the discipline at the same time as they guard against the naïve appropriation of this problematic methodology from anthropology. As a geographer I not only confront a legacy of field research that I want to stand on its head, but I also engage in peculiar intra- and interdisciplinary politics concerning ethnographic methods and the nature of qualitative research.

In neither task am I alone. In recent years there has been a spate of articles on the problems and questions raised by qualitative research, as well as a number of conference sessions and journal sections devoted to questions raised by feminist research practices (*The Canadian Geographer*, 1993; *The Professional Geographer*, 1994). This work explores such issues as self-reflexivity, intersubjectivity, representation, positionality and epistemic privilege, and the uneven power relations of research (Eyles and Smith, 1988; Pile, 1991; Keith, 1992; McDowell, 1992; Dyck, 1993; Rose, 1993; Gilbert, 1994; Kobayashi, 1994; Nast, 1994). Yet this increasingly lively and sophisticated discussion remains a minor discourse in geography, and those deploying a qualitative analysis and/or ethnographic strategy in their research are often asked to defend or explain their strategies in journals, at conferences, and elsewhere. Thus there is an internal politics of fieldwork within geography as a discipline over critical issues such as voice: who gets to speak and how; epistemology: how knowledge is constituted and by whom; and method itself. Given these stakes, it is hard to call this struggle "purely academic." It has everything to do with power/knowledge, and those engaged understand that their victories and losses shape the contours of the field—the way it looks from the outside and the way it feels from within. Its outcomes determine employment, publication, promotion, funding, and practitioners' attractiveness to students—that is, the tenor, power, and fulfillment of everyday life in the academy. But there are broader issues to which this contest is connected.

In geography there is an interesting, if short, history of work concerned with producing "emancipatory geographies." The Detroit and Toronto Geographical Expeditions, arguably the most notable of these, sought self-consciously to turn the notion (and intent) of "the expedition" on its head. Replete with explorers, survival themes, discovery, and base camps, the two expeditions, carried out intermittently between 1968 and 1975, were anti-imperial, land reclaiming, and folk-empowering projects.

The Geographical Expeditions were the vision of William Bunge, a U.S., white, middle-class, male geographer who had the notion that the skills of geography could be democratized and used by people to alter the oppressive conditions of their lives. In a piece reflecting on the first years of the Detroit Expedition, Bunge (1977) noted that (traditional) explorations were always vital to the societies that undertook them, and that these quests for resources, wealth, and power were commissioned at the highest levels of authority, which were—not coincidentally—their main beneficiaries. By interpreting the social vitality and political-economic consequences of exploration democratically, Bunge and his colleagues offered a radical notion of exploration whose purpose was to help "the human species most directly" (35). They attempted to rid exploration of its exotic and imperial impulses. The point of their Expeditions was that people should explore their home terrains in order to reclaim and restore them. As Bunge suggested, the purpose of exploration would no longer be a search for paradise but a search within the self, a means to develop "a more appropriate base map for our times" (33).

Lacking the panache (and military-industrial rationale) of space explorations and the like (*Field Notes*, 1971; Bunge and Bordessa, 1975), these "subaltern geographies," as we might now call them, and "maps of the human condition" were not funded by the National Geographic Society.⁴ As Bunge noted, the Expeditions were neither "nice" nor "status quo" geographical enterprises. Indeed their essence was an engagement in oppositional geographical practices—the very antithesis of a traditional expeditionary foray. In both Expeditions "explorers" were drawn from the neighborhood as well as the university (Stephenson, 1974). Although the academic geographers made their skills available to particular communities, the definition of problems to be addressed rested with the community as a matter of principle. Stephenson (1974) notes that over half of the research problems addressed by the Toronto Expedition, for instance, were defined by the "community,"⁵ and that throughout the life of the Expedition, these research areas had priority over others originating elsewhere. Expedition participants made much of the fact that they were invited into "the community," and stayed only as long as they were welcome by its members. Full-time participation in the enterprise was essential to the Expedition ethos. Some suggested that this level of commitment distinguished an expedition from (mere) fieldwork (Stephenson, 1974).

Explorers established a "base camp" in the neighborhood where they worked, and lived there at least for the duration of the expeditions—which generally took place over the summer months. The explorers worked in teams to research issues such as children's survival and open space, traffic, highrise construction; hidden spaces and cultural expression; rats and garbage; and "urban nationalism." The overarching structure was an analysis of the spaces of nature, mankind, and machines at five scales, the neighborhood, the metropolis, the nation, the continent, and the planet (Bunge and Bordessa, 1975).

Despite their democratic rhetoric, the Expeditions apparently retained hierarchical tendencies. Stephenson (1974) notes, for example, that Bunge was the

Expedition "theoretician," and he took the problems identified by fieldworkers and produced a "theoretical manual" casting them in a broader structure—complete with hypotheses to be tested in the field. Stephenson called this "the most essential role in the Expedition." Theorizing not only expanded the scope of the work, but gave it a coherence that prevented it from "degenerat[ing] into a series of interesting studies" (1974: 99–100). Drawing on the research manuals, the team leaders, explorers, and part-time explorers collected data in the neighborhood, analyzed it in relation to the hypotheses, and developed proposals for improving the conditions they addressed. Hypotheses were proven or disproven in action through monitoring changes in the community. There seems to have been no internal critique of this highly positivist procedure, although Expedition members occasionally point to the hierarchical presumptions of those outside (e.g., Warren, 1971). My retrospective sense, however, is that this resilient hierarchialization of work must have been inimical to the radical democratic intent of the Expeditions themselves.

The Geographical Expeditions were deeply embedded in their own history and geography. With the U.S. civil rights and antiwar movements came a flowering of socially committed research and radical experiments with action research strategies. These strategies were given greater urgency following the uprisings in many U.S. cities in the late 1960s. The unprecedented withdrawal of capital from Detroit and the riots that followed ravaged the city and were an extraordinary impetus to Bunge, other radical scholars, and the Expeditions.

But the Expeditions also have roots in the geographical practices or inspirations of an earlier time. Among their antecedents was the nineteenth-century Russian anarchist geographer Kropotkin's work on environmental learning. Kropotkin emphasized a kind of environmental learning that fostered people's abilities "to organize their lives cooperatively, reject externally imposed designs for living, and become active agents for change" (Breitbart, 1992: 80). As the feminist geographer Myrna Breitbart notes, one of the keystones of Kropotkin's alternative pedagogy entailed community study as "a critical and emancipatory process," whereby children and others might examine the social relations embedded and perpetuated in the physical environment as a means of understanding and confronting the forces of dominance in their lives. Kropotkin promulgated the notion of "direct community and workplace exploration" that drew on the expertise of people who lived and worked in these environments, and saw this as a strategy of social and environmental change (Breitbart, 1992: 80; Breitbart, 1981). The similarities between Kropotkin and the work of the Geographical Expeditions are clear. Not only was the Society for Human Exploration committed to precisely these sorts of critical explorations of the community, but much of its emphasis was upon education, especially sharing the skills of academic geography with community members and students of all ages who could use them to design strategies for change. They seem to have enacted an inspiring form of praxis that encompassed teaching, learning, researching, and work toward change.

The Society for Human Exploration remained active from 1968 to 1975. During that time they launched major Expeditions in Detroit and Toronto that produced a wealth of data relevant to those engaged in emancipatory projects of urban social and geographical change. Their maps, charts, and diagrams are still provocative, and the problems they examined remain, tragically, on the agenda. Most of these problems, such as children's access to the outdoor environment, children's exposure to environmental hazards, urban disinvestment, and the conflicts between cities and their suburbs, have only gotten worse. The Expeditions were exemplary in engaging community residents in projects that focused their anger and empowered them to change conditions that were oppressive. Among the projects of the Detroit Expedition, for instance, was an atlas of human needs that identified areas that lacked health services and included maps of children's injuries from traffic accidents and maps of people who lived alone. Children produced neighborhood maps that showed, among other things, the location of trees (living or dead), rubbish, and streetlights. Some of the documents were written by community members, who often attacked academic elitism as well as racial and other social structures of dominance. The axis of power/knowledge they expressed and revealed remains extraordinary and makes the withering of the Expeditions all the more regrettable.

There were problems. As my references to species, humanity, and the planet suggest, the Expeditions tended to naturalize and essentialize social difference and experience. Their language and actions were often tainted with traces of racism, sexism, and nationalism: the responsibility of "thinking and telling," Bunge says at one point, "is dangerous work, manly work, like sand hogging or coal mining." They were often macho, even militaristic, and simultaneously self-aggrandizing: "If there are any dirty or dangerous or doubtful experiences to be faced, the geographers go first, and the leader of the Expedition goes very first" (Bunge, 1977). And the heterosexism of the Expeditions was completely unquestioned. Their rhetoric was explicitly survivalist and its Darwinian overtones sometimes led to naturalized rather than socially wrought strategies for change.

The Expeditions would invert as much as undo the binary relations of power that structure social life under capitalism and patriarchy, leading to a predictable paternalism. For example, the voices of black residents were privileged consistently by the (white, male, bourgeois) leadership, not only because they experienced the most oppressive and dominating social relations in Detroit or Toronto, but *because* they were black. Women were praised and privileged as mothers, with little sense that children and mothers were anything but biologically given units. However salutary and liberating they were in the early days of a political movement (McDowell, 1992), such essential inversions tend to harden the positions they oppose. An overall lack of reflexivity characterized the Expeditions' writings.

For all its problems, the Society for Human Exploration and its Expeditions were largely community driven and activist oriented and sought a radical socialization of science. Without much theoretical fanfare their work bored right into

the heart of what is now framed as power/knowledge and reworked detached notions of science to produce a scientific practice that was as much about organization as about knowledge. My criticisms (with the benefit of hindsight) trace a range of dilemmas that recur in field research, and like my own dilemmas, are only provisionally resolved. Among the lessons of feminist, antiracist, and post-colonial scholarship in recent years have been a clear understanding of the constructed and mobile nature of social categories and a critical awareness of how constructions of difference serve uneven ends. As Audrey Kobayashi (1994: 78) suggests, echoing a central dilemma facing ethnography, this politics of difference highlights the problem of "speaking for" others, and this in turn raises how difficult it is "to move unheralded into just any field situation and become an effective part of its struggle for change just because we believe in its political ends." That kind of engagement stakes out the borders and negotiates difference, identity, and change in an ongoing manner. In reading the texts of the Society for Human Exploration we see that the Expeditions foundered precisely on this point. Like most explorations, they were more concerned to stake out the boundaries and examine the goings-on within them than to live on the borders and understand their multiple positionings around them. A reinvigorated geographical expedition would need to constantly rework this delicate task.

When I reflect, not on the dilemmas of fieldwork alone, but on the serious dilemmas of social change and the relation of my work to them, I am drawn to the vigor and commitment of the Expeditions, despite their considerable blemishes. They offer a welcome corrective to the often paralytic and self-serving musings that forget the larger dilemmas of social change to focus on the blemishes alone. The urgency of these concerns requires that as variously positioned feminist scholars, we keep conjuring up ways—however problematic—to transgress and renegotiate the boundaries that stop us from working toward meaningful social change. A little enchantment might help, but it will take more than legerdemain.

NOTES

1. See Wolf, Chapter 1. Such "white lies" (a revealing term) are not made in a vacuum. While in the field we planned to get married, after years of living together and telling similar lies to landlords. My dissertation adviser had the astute insight that having lied to strangers at the start, we felt that we had been dishonest with family by the end. In some way we felt compelled to get married to make good on the lie. I still have not told that branch of "my family" that we are divorced.

2. By focusing on my own deceptive self-representations, I do not mean to suggest that I think the people who participated in my research were transparently honest. They were, of course, engaged in representation of their own. I have discussed these questions of representation elsewhere (Katz, 1992).

3. This, of course, begs the question of what is meant by "community." There were multiple communities within the neighborhood. These groups often had competing interests despite the fact that they had overlapping constituencies.

4. The work of the Detroit Geographical Expedition and Institute was given modest support by the Association of American Geographers, who in 1969 noted the significance of the DGEI in drawing students from outside of academia and providing them with the research skills and tools to meet their needs, and in pioneering the use of "the geographic method" with a citizen action group (Field Notes 3). Reading the "Field Notes" and the AAG Committee's report on the DGEI made the 1960s seem like another planet, one worthy of exploration.

5. As is often the case with such work, "the community" was not well-defined. As I suggested above, the term remains a kind of catchall that suggests but does not guarantee democratic participation.

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10

Situating Locations: The Politics of Self, Identity, and "Other" in Living and Writing the Text

J A Y A T I L A L

[As] Ethnography is moving into areas long occupied by sociology, . . . It has become clear that every version of an "other," wherever found, is also the construction of a "self," and the making of ethnographic texts . . . is the constant reconstitution of selves and others through specific exclusions, conventions, and discursive practices.

—James Clifford, *Partial Truths*

THE VERY NOTION of what it means to do research on gender and development in the contemporary historical arena has been urgently called into question by recent critical discourses on anticolonialism. For instance, debates postcoloniality have interrogated the excavation of the Third World as a resource for Western theory. Additionally, feminist discourses on difference and universalism have challenged the construction of the "Third World woman" as an essentialized Other. And finally, methodological writings in sociology and anthropology have also articulated a deep skepticism about methodological vantage points that colonize, or objectify, the subjects of one's research. The common theme that derails these strands of questioning is their collective interrogation of the foundationalism that is embedded in much extant social science research, which rests on an essential division between "Self" and "Other," or between the knowing subject (the researcher) and the known, or soon-to-be-known, object (the research subject).

In this chapter, I look at the politics of representation and the epistemological locations in attempting to articulate a nonuniversalizing feminist methodology that goes beyond colonialist representations of "Third World women." In so