

Playing the Field: Questions of Fieldwork in Geography

Cindi Katz

City University of New York

Many questions—practical, strategic, political, ethical, personal—are raised by conducting field research. Some of these seem, or are constituted as, separate from the “research itself,” yet are integral to it. In this paper I attempt to cut through the breach that divides the doing of fieldwork and the fieldwork itself by addressing what constitutes the “field,” what constitutes a field researcher, and what constitutes data under contemporary conditions of globalization. Drawing on my work in New York City and Sudan, I argue that by interrogating the multiple positionings of intellectuals and the means by which knowledge is produced and exchanged, field researchers and those with whom they work can find common ground to construct a politics of engagement that does not compartmentalize social actors along solitary axes. **Key Words:** ethnography, feminism, fieldwork, methodology, politics of research.

The questions raised by conducting fieldwork in human geography at once invoke boundaries and blur borders. Where are the boundaries between “the research” and everyday life; between “the fieldwork” and doing fieldwork; between “the field” and not; between “the scholar” and subject? Under contemporary conditions of globalization and post-positivist thought in the social sciences, we are always already in the field—multiply positioned actors, aware of the partiality of all our stories and the artifice of the boundaries drawn in order to tell them.

My argument itself traces a border—between recognition of the artificiality of the distinctions drawn between research and politics, the operations of research and the research itself, the field and the “not field,” the researcher and the participant; and the need to live by these distinctions in order to accomplish something, however partial and incomplete, to avoid paralysis, cynicism, the “waste” of our training, skills, and talents. At this historical moment and in all the geographical sites of research, it is crucial that social scientists inhabit a difficult and inherently unstable *space of betweenness* (cf. Katz 1992; Trinh 1986–87) in order to engage in rhetorical, empirical, and strategic *displacements* that merge our scholarship with a clear politics that works against the forces of oppression (Mascia-Lees et al. 1989).¹ This stance reflects a commitment to a project of critical scholarship and political subjectivity that at once connects me to a community of similarly engaged intellectuals, the political

subjects in communities where I work, and a global cosmopolitan community of historical actors opposed to capitalism, racism, and patriarchy.

But what does this mean in that blurry space of everyday life that I am arguing is also “the field”? I will get at this question in three ways: (1) through looking at *the field* and its constitution as a discursive and spatial practice; (2) through a brief examination of the power that infuses these practices; and (3) through a discussion of my own grappling with these issues and practices in New York and Sudan, particularly around the question of choosing a “field site” and doing the research.²

Constituting “the Field”

According to Clifford Geertz (1979), when we do field research we are engaged in conversations with “natives.” But to have these conversations in a way that is distinct from everyday life, we must have “a field” marked off in space and time. Through this localizing strategy by which a physical space is marked off for a period of time, “we”—ethnographers—define a site of inquiry that is necessarily artificial in its separations from geographical space and the flow of time. In most cases it is the ethnographer who draws the lines, defining in and out. Each focus, of course, excludes as well as includes. What it excludes or why is rarely addressed in ethnographic inquiry. Most often the fieldworker displaces her/himself in order to see—figures and grounds are often difficult

to discern at home, while even the routine practices of "others" have the capacity to intrigue. While the exoticizing impulse may be less strong than in the past, ethnographers still generally rely on at least some displacement from home grounds to elsewhere to distinguish and differentiate the objects of their inquiries.

Of course, there is a double displacement—conversations are first *in* the field and then *to* the field. Each conversation requires/draws on the act of displacement. Ethnographers are displaced persons—first to see, then to speak. One goes to the field as a kind of "stranger," and draws on that status to see difference and ask questions that under other circumstances might seem (even more) intrusive, ignorant, or inane to those who answer them. The answers, and what one makes of them, have currency in other sites of enunciation—journals, classrooms, conference halls—that the ethnographer travels to with the scholarly equivalent of war stories.

But there is also a displacement of the field site itself that is crucial for the kind of engaged research I am advocating. This displacement ensures that no site becomes a vessel for holding cultural attributes. In my own work, for instance, I have refused to look at issues of children's marginalization by the forces of capitalism in a single site. By displacing the field and addressing the issue in rural Sudan and East Harlem, New York—settings that on the surface appear to have little in common—I am able to tell a story not of marginalization alone where "those poor people" might be the key narrative theme, but of the systemic predations of global economic restructuring.

Arjun Appadurai (1988b), in a similar vein, cautions against *places* becoming guardians of cultural features and examines how this affects the study of those features in other settings. His work, for example, addresses how hierarchy has come to be associated with India and how the form of hierarchy characteristic of India can influence how one sees or looks for hierarchy elsewhere—not only homogenizing hierarchical forms but blinding researchers to its other visages. Displacement is literally and figuratively a means to avoid this problem; that is, after one investigates a relationship, a process, a set of relations, or a phenomenon as a thread that winds through the political-eco-

nomie and sociocultural relations of one setting, an investigator would then in some way relocate to build on or call into question this work. The displacement might be, for example, a change in settings or a new "thread" to pull in the same setting. Each phase and the strategy itself is a means to locate crucial conjunctures in the social relations of production and reproduction of the areas under study. The aim is not to bound a site of common culture and turn it into a museum/mausoleum, but to locate and pry apart some of the differences, not just between one site and elsewhere but within it as well (cf. Katz 1992).

In my work I seek to multiply the differences in and between each site, to understand their salience, and in the comparison to identify conjunctures of common cultural concern or practice. But all these markings of place mark spaces of power. As Appadurai (1988a, 20) notes, "The problem of place and voice is ultimately a problem of power."

Fields of Power

The fields of power that infuse these spatial and discursive practices are many. There is the power to define "the field" which imposes me/the fieldworker on the time-space of others. I am an outsider in this context, but once there, of course, am not outside the power dynamics of the space so marked. In Sudan, for instance, I was essentially an autonomous researcher, unaffiliated with any group or project. My autonomy was, of course, tainted—only in "The Wizard of Oz" do women descend on other lands without obvious cultural baggage. I was not playing the "good witch of the north," so I first introduced myself to people in the village on a Khartoum University Geography Department field trip, and then, a year later, was somewhat ceremoniously introduced to a meeting of (male) villagers by the social workers associated with the state-sponsored agricultural project whose impact I was studying. The social workers fobbed me off as part of a package deal with a young "woman guide" intending to reside in the village and teach reading and home science to young women there. While the village representatives accepted my petition for residence with some trepidation, they were quite enthusiastic to take in the young guide Leila. My

association with her eased both the apprehension and the actual burden of my residence (for me as well as my hosts.) We lived in a vacated house belonging to one of the wealthier families in the village who had ties to both the agricultural project and the Sudanese Socialist Party in power at the time. While these dynamics did not impede my project in any apparent way, they had obvious and subtle impacts upon my work that are impossible to fully determine due to the historical and geographical situatedness of field research.

In New York the power dynamics were quite different. While I also had an independent research agenda there, I worked in East Harlem as a member of a project known as CAMEO (Community, Autobiography, Memory, Ethnography, and Organization),³ which was explicitly a participatory endeavor rooted in the common political ground unearthed in numerous meetings between participants based in the community and in the university. The dissimilar power dynamics of the two studies render basic issues, such as defining the fields of inquiry, quite different.

But I operate in another power field as well—an academic field with power to define or legitimate a field of inquiry. Women in general and feminist work in particular historically have been marginalized by the academic mainstream. In geography ethnographic fieldwork has never been central. In the wake of the “quantitative revolution” and in the face of the positivism that still holds sway in much of the field, ethnographic and other forms of qualitative research have been required to conform to standards that are external to their constitution. There is no parity or reciprocity in this realm, although as nonpositivist paradigms—among them Marxism, feminism, realism, and critical humanism—have become ascendent and even dominant in human geography, the demands on nonpositivist scholars have eased. My power/lessness (in the field and in my field) as well as that of the people with whom I work are complexly interwoven in the fieldwork process and the ways I report on my work.

Working the Field

The fields of power that connect the field researcher and participants, the participants to one another, scholars in the field, and research

participants and audiences as historical subjects who confront various but specifiable conditions of oppression, deserve critical scrutiny in the conduct of field research. Such scrutiny raises questions such as “where are one’s fields”; “what are the displacements”; and “how does the work deploy and confront power—whose power, where, and under what conditions?” I will refract these questions through the kaleidoscope of my own ethnographic work over the last 10 years.

The choice of my field site in Sudan was an amalgam of historical circumstance, intellectual criteria, practical specifications, and default. I am responsible to and for the choice. I wanted to study the relations between production and social reproduction under conditions of significant political-economic change. I reasoned that a period of heightened change would sharpen the relief on those cultural practices that maintained (and might transform) a particular social formation, and so chose to undertake my study in an area undergoing dramatic sociocultural and political-economic change. I had long been interested in these questions in the context of Eastern and Southern Africa and decided to conduct the research there in a country that was forging a capitalist approach to development.

A couple of years before I chose a field site, I had traveled through the prospective area. Due to the real life drama and rapidity of the transformations that intrigued me—evidenced in border closings, coups d’etat, and aggressively military dictatorships such as that of Idi Amin in Uganda—I found myself with firsthand experience in only two of the countries where I had thought of working. I found Sudan more compelling due to what I perceived was the relatively light touch of colonialism there. This statement is not intended in any way to minimize over a century of painful colonial exploitation, first by the Ottomans and then by the British through the aegis of the Anglo-Egyptian Condominium, but only to say that relative to a country such as Kenya where there was an enormous European settler population and a widespread plantation economy, most Sudanese had little to do with the colonizers. My sense was that the relative absence of direct contact had left people’s senses of integrity intact, and that my interests were met more by candor and hospitality than by the hostility and

suspicion field workers have (understandably) found elsewhere. In a more careerist vein, I was also aware that Sudan, particularly Northern Sudan, was relatively understudied compared with other parts of Africa, and that added to its cache for my ambitions.

The preceding paragraphs overflow with the arrogance of research (and my calling attention to it here does not obviate it). I speak of choosing, deciding, wanting, traveling, reasoning, finding compelling, and being intrigued. My career in the balance, the object of my study was people's lives, lived in real time and space. But these lives, like "our" own, were not lived in circumstances of their subjects' choosing. Without being too facile in the comparison, there was common ground in struggles with and against these circumstances whether here or there. My research, fueled by feminist and Marxist concerns, was born of a commitment to social change that promised greater political-economic equity, social justice, and cultural freedom as well as widened access to the means of existence and basic rights. I understood (and understand) what I was doing as part of a project that would help move disparate social groups toward these goals in confrontation with sources of oppression and domination that were indeed global. I paid much attention to the specific form these social relations took at the particular geographical and historical juncture that obtained at my field site in Sudan, but the interstices of the global connections were the political ground of my research and justified for me its focus on a different "local" than my own. My ambition was broader than the study of a single-singular site.

While I still agree with this stance, after completing the research I felt somewhat compelled to work where I lived and live where I worked—the research in Sudan had at the very least traces of exoticism and there were similar disjunctures between social reproduction and production closer to home. This intertwining of uneasiness and insight led me to move or "displace" my research to Harlem. The issues and concerns remained largely the same. Indeed the Harlem project was inspired by my findings in Sudan that children were not learning the knowledge they needed for the conditions they were likely to face as adults (Katz 1991). The shifts in social relations, on the one hand, from a subsistence orientation to capital-

ist and, on the other, from Fordist manufacturing to the ravages of deindustrialization, seemed to disrupt everyday life in startlingly similar ways. New York became a field site for the continued study of the production and exchange of knowledge. The common thread of the studies—the de-skilling and displacement of children—would, I thought, be better understood with the insights afforded by the juxtapositions that come from a multilocal ethnography. Moreover, this strategy would make explicit the ties between local effects of global economic restructuring, for that was what I was witnessing in both settings.

Still, Harlem is not my home. I am neither working class nor Latino. I questioned my need to keep this edge of distance in my project—weren't we always already in the field (cf. Koptiuch 1985); couldn't I do this work on my own block? Yes. But here again I was trying to understand the responses to political-economic displacement, and in this working class community the displacements of disinvestment and deindustrialization were stark and hard hitting. Understanding them and developing effective responses seemed more urgent there than in other parts of New York City. In addition, I was part of a larger participatory project (CAMEO) focused on the concerns voiced by the community, and my work fit with and benefited from this broader endeavor.

All of this notwithstanding, I remain uneasy with my choice. I have come to terms with it by yet another displacement—one that connects the concerns of my work to my own subject position as a political actor engaged in struggles against some of the same oppressive processes at work in the lives of those with whom I work. I confront these from a different but not wholly distinct standpoint—there is common ground. If I keep this in sight, I am deterred from the suspect stance that my work has direct benefits for the participants. Such a posture would elide their subjectivity. But if some common grounds are established, there can be mutual learning about the meaningful differences and workable affinities in our positions vis-à-vis the structures of dominance. Then perhaps, we—all participants in the work—can appropriate this knowledge in ways that strengthen us in our encounters with these structures of dominance, and allow us the possibility of connecting across class, race, gender,

or other lines to confront their manifestations in everyday life. I realize this is a tall order (and I do not claim success), but such moves are fundamental to conducting politically committed research that is true to its intent.

In practice this stance may call for different strategies than might be best from a social science or careerist standpoint. For instance, ethnographic work can (inadvertently) expose sensitive practices of subaltern people to those who (might) use this knowledge to oppress them. While virtually all ethnographers protect the anonymity of their participants, there may be times when this is not enough and data must be withheld or reported selectively. M. Milagros Lopez (1992) inspires with her admonition to scholars working with subaltern groups not to render the practices of the oppressed visible to those who dominate, but to make the operations of capitalism and patriarchy more transparent to the oppressed groups.

These issues bear on various projects of which I am a part, and necessarily alter the criteria by which success is measured. In CAMEO, for instance, numerous community-based, and at least partially, community-driven, ethnographies and cultural histories have been recorded on video and speak eloquently of the political, social, and economic struggles of local men and women of different ages. The histories of social and political organizations selected by participants have been documented, at least partially. One of these, the El Barrio Popular Education Program, has enlisted two CAMEO members as occasional teachers. The beginnings of a place history were undertaken for La Marqueta, a longstanding community focal point that until the previous decade was a thriving food and dry goods market. An ethnography of everyday life on one block in East Harlem has sputtered along in fits and starts. A doctoral study of math learning among Latina girls is underway. Some of these activities have arisen from the political mobilization that university based CAMEO members found in the community at the start of the project in 1991, and others have touched off at least sparks of possible mobilization. Nothing has been published in a scholarly journal.

In another Harlem neighborhood, my colleagues from the Children's Environments Research Group at the City University of New York and I have been involved for the last five

years in a participatory project to redesign two schoolyards (Katz and Hart 1990). This project grew out of a community initiative to create early childhood play environments. With funding from the Aaron Diamond Foundation, our group conducted an ethnography of the schoolyards. This study, which was part of the overall redesign effort, differs from many ethnographic forays in that it was directed toward the accomplishment of specific changes called for by participants including the school children, community members, the school district administration, and the school staff. The project from inception through the development of final designs for the two yards received tremendous support from all but the Board of Education bureaucracy which has continued to stonewall efforts to put the designs into practice. The repeated delay tactics and diversions of the Board of Education have mobilized more neighborhood participants than the initial design project did.

In the mid 1980s I conducted intensive field research to inform the development of the social extension program for a large scale reforestation project aimed at Eritrean and Ethiopian refugees in Northeastern Sudan. While this project was driven by the donor, CARE, and the government of Sudan, it was oriented to provide sorely needed resources to refugees and local dwellers alike and was met with wide cooperation. The reforestation efforts, undergirded by an intensive and effective extension program, were highly successful (cf. Katz 1984).

My point in raising these projects is to reflect on their lack of currency in the academy despite their successes on the ground. While I have presented scholarly papers on each project, I have as yet to write on them for academic journals—they seem so “applied” and applied work seems distinctly marginal to the core of debates that swirl in “the field.” We have theories about theory and practice, but practice takes a beating in the high stakes debates of academia. I am enough of a “wannabe” that I have silenced myself on these grounds.

These displacements and discomforts are the issue. I, the social actor/scholar am interpolated in all of these projects, and they—practical, applied, theoretical—figure in my development. I have learned and built a career on each of these undertakings. Their benefits to partici-

pants notwithstanding, these field projects all have probably been more beneficial to me than to them (cf. Stacey 1988). In those cases where "benefits" have accrued to participants in tangible ways, such as in the schoolyards or reforestation projects, the scholarly currency of the work has been ambiguous at best. Yet my argument is always in between these roles. I am always a gendered, historically constituted social and political actor who works as a social scientist and teacher. I am always, everywhere, in "the field." My practice as a politically engaged geographer—feminist, Marxist, anti-racist—requires that I work on many fronts—teaching, writing, and nonacademy based practice—not just to expose power relations but to overcome them (Mascia-Lees et al. 1989, 33). This task requires recognition that as an ethnographer and as a woman my subject position is constituted in spaces of betweenness, what Mascia-Lees and her colleagues (1989, 33) call, "a position that is neither inside nor outside."

From such a standpoint it may be possible to frame questions that are at once of substantive and theoretical interest as well as of practical significance to those with whom we work. By operating within these multiple contexts all the time, we may begin to learn not to displace or separate so as to see and speak, but to see, be seen, speak, listen and be heard in the multiply determined fields that we are everywhere, always in. In this way we can build a politics of engagement and simultaneously practice committed scholarship (cf. Kobayashi 1994). The stakes all around could not be higher. ■

Notes

¹By displacement I mean quite literally a conscious movement from one position or site to another. The implications of the term include notions of uprooting, loosening, disturbing, and dislodging. My argument is that ethnographic research is underwritten by a host of displacements that are rarely addressed by the researcher either in the field setting or in the academy. This piece is intended to problematize the displacements scholars engage in when conducting field research in order to reveal some of the political consequences and potentials of such research.

²I have discussed the questions of representation that these issues raise elsewhere (cf. Katz 1992).

³The CAMEO Project, carried out by members of the Committee for Cultural Studies at the Graduate

School of the City University of New York, was funded by the Aaron Diamond Foundation, Inc.

Literature Cited

- Appadurai, A. 1988a. Introduction: Place and voice in anthropological theory. *Cultural Anthropology* 3:16-20.
- Appadurai, A. 1988b. Putting hierarchy in its place. *Cultural Anthropology* 3:36-49.
- Geertz, C. 1979. From the natives' point of view: On the nature of anthropological understanding. In *Interpretive Social Science: A Reader*, ed. P. Rabinow and W. N. Sullivan, 225-41. Berkeley, CA: University of California Press.
- Katz, C. 1984. *The Social Context of Reforestation: Socio-Economic and Cultural Profile of Project Area with Recommendations for a Program of Forestry Extension*. New York: CARE.
- Katz, C. 1991. Sow what you know: The struggle for social reproduction in rural Sudan. *Annals of the Association of American Geographers* 81:488-514.
- Katz, C. 1992. All the world is staged: Intellectuals and the projects of ethnography. *Environment and Planning D: Society and Space* 10:495-510.
- Katz, C., and R. Hart. 1990. *The Participatory Design of Two Community Elementary Schoolyards in Harlem P.S. 185 and P.S. 208*. New York: Children's Environments Research Group, the City University of New York.
- Lopez, M. M. 1992. The post-work society in Puerto Rico. Lecture presented at Department of Puerto Rican and Hispanic Caribbean Studies, Rutgers University.
- Kobayashi, A. 1994. Coloring the field: Gender, "race," and the politics of fieldwork. *The Professional Geographer* 46:73-80.
- Koptiuch, K. 1985. Fieldwork in the postmodern world: Notes on ethnography in an expanded field. Paper presented at the annual meeting of the American Anthropological Association, Chicago, IL.
- Mascia-Lees, F. E., P. Sharpe, and C. B. Cohen. 1989. The postmodernist turn in anthropology: Cautions from a feminist perspective. *Signs: Journal of Women in Culture and Society* 15:7-33.
- Stacey, J. 1988. Can there be a feminist ethnography? *Women's Studies International Forum* 11:21-27.
- Trinh T. M. 1986-87. Difference: "A special Third World women issue." *Discourse* 8:11-38.
- CINDI KATZ is Assistant Professor in the Environmental Psychology Program at the Graduate School of the City University of New York, New York, NY 10036-8099. Her research interests are nature and society, gender and geography, children and the environment, and cultural studies.

