

## American Geographical Society

---

Returning Home and to the Field

Author(s): Karen E. Till

Source: *Geographical Review*, Vol. 91, No. 1/2, Doing Fieldwork (Jan. - Apr., 2001), pp. 46-56

Published by: American Geographical Society

Stable URL: <http://www.jstor.org/stable/3250804>

Accessed: 14/01/2010 08:32

---

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=ags>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).



American Geographical Society is collaborating with JSTOR to digitize, preserve and extend access to *Geographical Review*.

<http://www.jstor.org>

## RETURNING HOME AND TO THE FIELD\*

KAREN E. TILL

*Last week I “returned from the field.” Even though I am back home I certainly don’t feel at home. I feel disoriented for all sorts of reasons. Because I came back during the sixth week of the semester, I feel like an outsider in my workplace, an empty shell moving through corridors and seminar rooms. (Was that me teaching that class a few months ago?) Where I live now seems foreign, perhaps because I’ve lived longer in Berlin than in any of my American homes during the last ten years. This blurring of home and the field reminds me of an experience last year when two of my Berlin research consultants gave lectures about their work at my “home” university. Due to various conversations and interactions, I unexpectedly had to face some of the moral issues and social relations of the field at home. I felt almost schizophrenic, torn between worlds, cultures, sets of social relations, and selves.*

A researcher cannot easily divide her research and personal selves into separate sites of home and the field. Although many geographers realize this in theory, in practice we often construct emotional, spatial, and temporal boundaries between personal and work lives, a *here* and *there*, a home and field. The dislocations I experienced in returning from the field or when the field invaded my home resulted from my attempts to protect myself from my work. Yet these very dislocations forced me to acknowledge that life in one place influences social relations in another.

Home and the field are unstable categories that are designated by research conventions, the academy, and researchers in particular ways. By moving between ever-changing homes/fields and social relations, the researcher must acknowledge that research spaces are always hybrid; they are complex social spaces of dislocation. Furthermore, our identities are constantly made and remade through repetitive performances—performances that include research (Rose 1997). When we move back and forth between shifting homes and fields, our research agendas, relationships, and even our own understandings of ourselves as researchers will change because we can never know who we will become during the research process. As I describe below through personal vignettes about conducting follow-up research in Berlin, Germany, when we return and share our writings with research consultants, we are forced to challenge previously held assumptions and to negotiate new research relationships. Facing our unease in such settings may be difficult, but it may also lead to new insights and more empathetic geographies and histories.

---

\* This article owes much to a stimulating panel discussion on “Exploring Cultural and Political Terrains of Qualitative Methods,” held during the 2000 annual meeting of the Association of American Geographers. I thank Vincent Del Casino, Dydia DeLyser, Jennifer Hyndman, Suzanne Michel, Richa Nagar, Yamuna Sangarasivam, and the attendees of that session for their insights. Helpful comments on earlier drafts of this essay came from Susan Terrio, Patricia Ehrkamp, Joan Hackling, Steve Pile, Mary Thomas, Dydia DeLyser, Paul Starrs, Heike Alberts, and participants in the 2001 University of Minnesota Advanced Qualitative Methods Seminar. My warmest thanks are also extended to the many individuals in Berlin who have shared their lives with me. Without their generosity, my research would not be possible.

✪ DR. TILL is an assistant professor of geography at the University of Minnesota, Minneapolis, Minnesota 55455.

## CONSTRUCTING HOME AND THE FIELD AS RESEARCH TERRITORIES

To call attention to the power relations involved in the scholarly production of knowledge, anthropologists, geographers, and feminists have argued for more complex understandings of home and the field (PG 1994; Gupta and Ferguson 1997). As Cindi Katz (1994) and members of the Women and Geography Study Group (Madge and others 1997) argue, we cannot dissociate our identities as academics from the places, contexts, and peoples where and with whom we do our research, write, and communicate. From this perspective, the “field” includes: the academy, where research is initiated, where the people we speak with live, and the social contexts and settings in which research is funded and made available to various audiences (Nast 1994). Individual understandings of the field, moreover, are positionally situated and always shifting. As Lila Abu-Lughod points out, every view and act of speaking must come “from somewhere” (1991).

The “somewheres” from where we speak result from ever-shifting personal and scholarly understandings of home and the field. When we conduct research, we often bring our homes with us; sometimes the field becomes a home (Pile, personal correspondence; Oberhauser 1997). In my first research project, for example, I examined newly constructed, master-planned communities in southern Orange County, California. I didn’t distinguish a home from the field because the field was home: I analyzed the rapidly changing landscapes and urban developments that were located between my first adult home, Los Angeles, and my childhood home, northeastern San Diego County. Through research and subsequent writings I claimed a speaking position of native Southern Californian, based on personal experiences and understandings of small towns and cities in the region. I felt constrained growing up in a small, white, working-class town, so my first experiences living in a large city were very positive and even redemptive, thanks to the diversity of lifestyles there and the people I met. Not surprisingly, I was suspicious of how (predominantly white male) experts represented a new, planned community as a historic, democratic, and utopian small town. Such place-making activities, I argued, played on and created people’s fears of the dark city and sprawling suburb and reified exclusionary “geographies of otherness” (Till 1993).

In my dissertation research I sought out a project that was not about my home but located “somewhere else.” I chose to study Berlin because I wanted to move from research about utopian representations of “a good place to live” to examine its other face, the dystopian city. Even though I had never been to Berlin, it was nonetheless familiar to me. Both Los Angeles and Berlin have been represented—albeit in distinct ways—as dark, nightmarish cities whose landscapes reflect maniacal quests for absolute power and racial control. Yet many cultural and political worlds exist in these cities, and it was that critical edge between oppressive power and progressive resistance that I wanted to explore in Berlin. An emotional return was also implicit in my work: By going to Germany I was “returning” to an absent familial past, an imagined ancestral home. My German mother, born in West Prussia in 1929, spoke little about her wartime and postwar experiences, and I had always been curious

about my family history. Subconsciously I may have wanted to go “back home” to fill an absence in my present-day life.

Before I even went to Berlin, then, my understandings of home and the field were complex. Yet these categories were institutionally produced as distinct, albeit admittedly abstract, realms of knowledge about America and Germany. During my doctoral oral preliminary examination in the United States, for example, my committee asked, “Why should an American study German identity and memory?” In Germany, gatekeepers—literally, influential figures who had to approve, at least implicitly, my research and provide physical access to places, peoples, institutions, and information—asked, “Why don’t you study slavery or Native American genocide in your own country?” or stated, “Americans aren’t interested in history.” In response to these experiences, I delineated research territories of “home” and the “field” to justify my project to my dissertation committee, other academics, and research consultants. The assumed research “distance” between home and the field also helped me negotiate passage through gatekeepers in Germany.

Scholars create such research territories at various scales—in ways that we often don’t realize—as part of the research process as well as a process of identity formation. Demarcating home as distinct from the field was one way I tried to manage my research relationships, controlling how knowledge about “the field” would be defined and represented. But I also erected boundaries to protect myself from the emotional, social, and political difficulties of conducting work about post-Holocaust memory in Germany.<sup>1</sup> Conflicts, tensions, differences, and gaps that emerge through the research process should be acknowledged and studied (Rose 1997; DeVault 1999; Nagar and Geiger 1999). Conducting follow-up research is one way to recognize such differences.

#### NEGOTIATING RESEARCH SOCIAL RELATIONS

During the past ten years I have examined the interrelated processes of place making and social memory in a city haunted by its dark pasts (Till 1996, 1999, 2001b). I have explored how various social groups attempt to represent a people and a past by establishing public places of memory, such as national history museums, Holocaust memorials, and museum-memorial educational centers. Because many publics and many pasts can be remembered and represented, the act of place making—notably in Berlin—is controversial. To understand the goals and actions of various groups and individuals, I examined a limited number of case studies using multiple interpretive qualitative methods (Pile 1991), including participant observation, documentary analysis, and interviews. I traced the institutional histories of places of memory to study why these entities were established when and where they were.

I worked closely with what I came to describe as “memory experts” (Figure 1). In Berlin these included museum specialists, educators, memorial directors, academics (in history and art history), artists, members of the Jewish community, and participants in citizen initiatives. Most were well-educated individuals, ranging in age from their early twenties to their late sixties; men held more positions of power



FIG. 1—The author visiting with a research consultant at an international symposium in Germany. (Photograph by Boris Borowski, March 2001)

than did women. I tried to build reciprocal relationships with my consultants and assumed that interviewees knew more than I did about the institutions and places being discussed. Such an approach is advocated by many feminists (England 1994). Because my research consultants were incredibly self-reflexive about their work (Till 2001a), I shared my interpretations and opinions on topics with some of them. Sometimes I challenged statements or sought responses to my preliminary analysis. Of course, not all consultants wanted such a relationship. Many agreed to just one formal interview and kept a strict professional distance.

Early in my research I couldn't anticipate how I would be "seen" by my consultants. For example, I didn't fully understand the extent of U.S. influence in post-World War II, post-Holocaust Germany. My presence—an American in Berlin—was often defined by these historical and political relationships. Because I was interested in identity politics, I had to negotiate not only my Americanness but also my ethnic German heritage, my age, my gender, and my nonexpert (geographer and student) status in different ways with nearly each expert and every citizen group. My relationships with different individuals varied enormously, and relationships with particular individuals continually changed (compare Nagar 1997). I was never clearly an "insider" or an "outsider." Indeed, as Jennifer Pierce suggests, many possible social statuses exist for ethnographers, "shifting positions [that] have consequences for what we can 'know' and see as researchers" (1995, 194).

## RETURNING TO THE FIELD

By the time I began my follow-up research, Berlin had become a home for me. I knew parts of the city intimately, and I looked forward to revisiting favorite districts, locales, and landmarks. I was intrigued to see how rapidly the city was changing, and I wanted to explore new areas. My German improved with each trip, which made basic living easier and conversations more enjoyable. Friends living in Europe visited me in Berlin, and I visited my relatives in Germany. One couple, who used to be my neighbors when I lived in East Berlin, even began jokingly to call me their daughter because I regularly came “home” to visit them.

Before going back, I tried to let my research consultants know that I wanted their input on my dissertation: What did they think of how I represented them, their work, and the places where they lived and worked? Returning for this purpose was reassuring because I no longer had to spend months getting to know people and becoming familiar with their social and cultural networks. I had to make fewer new contacts with institutions and groups, I didn’t have to explain my research project as often (and when I did I had a better sense of how to do so culturally), I had to negotiate with far fewer gatekeepers, I had less trouble setting up interviews, and I knew where to find information about institutions. (I even joked with some consultants about the cultural Americanness of the Human Subjects Internal Review Board consent forms.) But I also naively assumed that the home/field social and research relationships of my dissertation research phase would remain the same. Roughly three years had passed since I had seen my research consultants in person. They knew me as a student, and now I was going back as an assistant professor, which gave me confidence and a new social standing as an “expert” in Germany. I was also returning for different reasons: to refine my previous analyses and to begin new case-study research. Because of my assumptions about returning, I was unprepared for what would transpire.

## OBTAINING INPUT: TWO EXAMPLES

When working with experts from public institutions with limited resources or in politically charged settings, much may be at stake (Terrio 1998). Experts write texts, create exhibitions, conduct research, give tours and seminars, offer public lectures, and try to gain political and financial support for their work. Not surprisingly, when I went back to Berlin people responded in various ways. Some were glad to see me, offering to meet for coffee or a meal. Others remained unsure of my motives and reasons for coming back (a sentiment that actually increased for some in follow-up trips). Still others seemed indifferent. In terms of feedback, most individuals hadn’t read my dissertation, and those who had, had read it very selectively (compare Behar 1993; Ellis 1995). Charles Kurzman similarly noted that most people skip the theoretical and analytical arguments and focus on who is quoted, praised, or criticized, and how often: “The researcher’s goal of analysis simply is not a high priority in the subjects’ lives. . . . [Subjects] denied that my theoretical interests . . . were even applicable to them” (1991, 266).

Two examples may illustrate how my research relationships changed with follow-up research. To assure anonymity, in this essay I refer to all of my consultants as “he,” though not all of them were male. I also have changed some details about my interactions in order to protect consultants’ identities (see Kennedy and Davis 1996). In the following examples, my consultants were of a different age, gender, demeanor, and dress and were distinct in educational training and work experience.

One consultant was happy to see me and met me with smiles. He had always been generous with his time during the dissertation-research phase, and we had had numerous informal conversations. He had helped me write and administer a visitor survey at his institution. I had sat in on his tours and seminars, read his professional writings, and conducted several in-depth interviews. When I went back, he was conducting research for a future permanent exhibition, giving tours, and working on other projects. When we spoke on the telephone before our meeting, he mentioned his pleasure at the way I had attributed information to him (even though I never used his name). He expressed pleasant surprise at my critiques of the existing status quo because I’m American. (I am not sure whether his mention of my nationality indicated that he assumed my politics were conservative or that he perceived me as lacking insight or knowledge about Berlin politics as a nonnative researcher.)

When we met for what I thought would be a follow-up interview, I found that he had invited a woman about my age—whom I didn’t know—to join us for lunch. He proudly lugged along a copy of my dissertation “just in case,” referring to it jokingly as “the Bible.” Although we had often discussed my work over meals in the past, there were enough cues at this point for me to understand that he wanted only a social visit. But I wasn’t sure I had the time to reschedule another interview, so I decided to ask more pointed questions as we ate—which, of course, turned out to be a mistake. At one point I asked him what he thought about my criticisms of his institution. When he didn’t respond, our lunch companion, who wasn’t familiar with my work, asked me to outline my interpretations. As the two of us discussed my research, my consultant became quiet and more reserved.

Subsequent interactions with this consultant were strained. He was often too busy to meet me during later trips, and I didn’t stay in close contact with him through correspondence. Reflecting on this relationship now, I realize that he viewed my dissertation as a finished product and expected us to discuss the positive things I had written about his workplace. I, on the other hand, hoped for a lively discussion about my research. As a result of our lunch, however, our positions may have shifted: The lunch guest may have come to view me, rather than my consultant, as the expert because of my title, critical comments, and Americanness. To this day I feel a sense of personal loss.

In the second case I maintained a professional, if removed, relationship with a research consultant. Our interactions were formal, and we had always met at his institution. During the course of my dissertation research he had worked on temporary exhibitions and had done research for a permanent exhibition. He and other staff members had helped me conduct an open-ended visitor survey about an exhi-

bition at his home institution. I had had access to public-relations files and had been given permission to observe individuals and tours going through exhibitions there. I had conducted a single in-depth personal interview with him.

When I went back, I interacted briefly with him for a tour before our follow-up interview, and he seemed quite friendly. I planned on starting the interview by asking about his recent work projects. But as soon as I started the tape recorder, he articulated his displeasure with my research in a very terse way: "In general I have the feeling that you used [our work and] us to a certain degree, and instrumentalized us. I don't like that." Although he admitted that he hadn't read the entire dissertation, he nonetheless felt that I left out too much information about his work and his institution. As Kurzman describes, "Subjects, by and large, think of themselves as individuals, with more or less special characteristics. To reduce them to a single aspect or limited bundle of attributes is to ignore a large part of who they are, or who they think they are" (1991, 268). This consultant felt that I had simplified his work and the function of his institution to make my arguments stronger. To a degree, he was right. At the time of the follow-up interview, however, I was surprised by his strong negative reactions and found myself defending my work—which was particularly difficult to do because I was upset and couldn't think clearly in another language.

Later I wondered whether my representation of his work and workplace could be used in unintended ways. Scholarly criticisms, particularly from Americans, about how Germans represent the past and their identity are taken seriously. Furthermore, my research topic can be politically explosive, particularly in the context of postunification Berlin. The network of individuals working at the places I studied is small, and word gets around. Might my writing affect his job and status? I also wondered about our future research relationship. Should I have shared my work with him? Would I be able to come back to his institution after such an encounter? I felt better when he sent me a note thanking me for the discussion and acknowledging the validity of some of my arguments. (A few years later, he even asked me to make a scholarly presentation of my work in Berlin.)

Melissa Gilbert has argued against the possibility of being a researcher-as-friend, stating that we are always outsiders when engaged in research (1994). Yet acknowledging that we are "outsiders" is not enough to avoid unintentionally hurting others, experiencing loss, or trying to understand the changes in our relationships through time (Patai 1991; Ellis 1995; Pierce 1995; DeVault 1999). Researchers should recognize each person's goals in the research relationship (compare Mbilinyi 1989). Depending on the nature of the relationship, the context of the research process, and the ethical considerations associated with the research, we must—with our research partners—choose what relationships and behaviors are most appropriate and respectful (Katz 1992; England 1994; Nagar and Geiger 1999).

#### WHAT WRITINGS SHOULD A RESEARCHER SHARE?

In subsequent years I experimented with sharing writings at an earlier stage in the research process, in hopes of reducing the strong emotional response of consult-

ants to finished work and of increasing the sense of cooperation between my consultants and me on the project. Feminists suggest that such a strategy may reduce unequal power relations inherent to research (Mbilinyi 1989; Wolf 1996). Although this may be true for other research projects, I learned that sharing rough drafts and interview transcripts in politically charged settings can evoke anxiety, damage research relationships, and even jeopardize attempts at confidentiality (compare Kurzman 1991; Brettell 1993; Ellis 1995).

In my experience, individuals were surprised or angry when they read interview transcripts. What they read did not coincide with their memories of the conversations and interactions we had had. For example, I shared a set of transcripts with some consultants who were familiar with the university setting and my role as professor and researcher. We always met at their homes over meals, and our discussions were informal, despite the presence of my tape recorder. When I was back in the United States, they e-mailed me and asked to see what I had written about them and their work. At the time I was adjusting to a new job and working on other projects, so I didn't have any good drafts of my work. Because I often discussed my research interpretations with them, I decided to share interview transcripts. When I e-mailed them copies, I mentioned that the interviews were unedited and would seem rough. I also told them that this wasn't my normal practice and that I would be curious to know their reactions.

They were shocked, even appalled. They didn't remember voicing such strong opinions about particular individuals, places, and institutions. They may have grown anxious rereading what they said in print because, taken out of context, their words could easily be misinterpreted. These consultants asked how I would use the transcripts and wanted to know who my other consultants were. I felt uncomfortable with their requests to break confidentiality and replied simply by reminding them what my case studies were. I also assured them that I would edit their comments and try not to reveal their identities (even though they gave me approval to use their real names). Despite these interactions, our research relationships have remained strong, and I still visit and interview these individuals in their homes.

Even if one shares polished writings, research consultants may try to find out information about other individuals and jeopardize attempts at confidentiality. I sent one individual a copy of a formal conference presentation. When we met at a second follow-up interview, he immediately pointed to a lengthy quotation in the paper and wanted to know whom I had quoted. He did so not in an accusative way, but appeared to be having fun guessing. Because he knew directors and staff members at similar institutions so well, he correctly guessed whom I had quoted *by the way the informant spoke!* He also wanted to know how yet another person reacted to a different section of my paper. Although I don't think he intended to harm these individuals, I found myself in a difficult position. I needed to protect the identity of my research consultants—which was almost impossible to do—and I also needed to maintain the research relationship with this consultant. In subsequent visits I em-

phasized my status as professor (a title that is taken more seriously in Germany than in the United States). I switched from using the familiar second-person *Du* to using the formal *Sie* and acted as professionally (in a German cultural research context) as I could. I carefully chose the color of my suit and my hairstyle. I used a briefcase, not a backpack. I was a bit more cool and distant in my mannerisms. It was a strategy that made me, at least, more comfortable; I felt as though I had more control over our research relationship and could better discuss issues such as confidentiality. The next interviews with him went very well.

Sharing writing is an important part of follow-up research (Brettell 1993). Research consultants and the researcher cooperate with one another to understand the processes, social relations, and places being studied. Consultants may feel empowered by pointing out mistakes or reanalyzing the researcher's representations (Mbilinyi 1989; Kennedy and Davis 1996). In situations that are particularly volatile, the researcher may have to protect the identity of an individual by avoiding long quotations, even though such a practice may work against other research goals, such as giving voice to consultants. Despite such difficulties, research consultants deserve to read more finished written works, and they should be encouraged to provide comments so that they aren't misinterpreted (compare Kurzman 1991).<sup>2</sup> Through sharing work and keeping consultants' goals, feelings, and feedback in mind, the researcher will be more responsible in her writing (Wolf 1996).

#### SO WHY CONDUCT FOLLOW-UP RESEARCH?

Returning home and to the field makes us ask, "Why, what, and where is *this* home/field?", "How do I (and the academy) construct these research territories?", and, "What does 'returning' mean?" Returning forces the researcher to acknowledge how shifting social and personal relations affect her understandings of the research project. Interpretations from previous research trips may be scrutinized and challenged by sharing work with consultants. Follow-up research, in other words, enables the researcher to be more objective (Haraway 1991) in her analysis and writings.

Returning is not easy. Because of institutional requirements and constraints in academia, conducting follow-up research can be emotionally difficult, time consuming, costly, and professionally risky (Bogdan and Biklen 1998). Some geography departments and universities reward the publication of articles rather than books, a practice that supports, however indirectly, short-term projects at the expense of more sustained ones. The unfortunate—and often misguided—implication of such promotion and tenure practices is that if a researcher continues to work in the same places, her research is not "new" (Dowler 2001). But there are also many good reasons to be engaged in long-term projects—reasons academic institutions should reward. Through follow-up work, the researcher may gain greater in-depth understandings of the peoples (including the researcher), places, and institutions studied. With sustained research that includes different kinds of returns over extended periods, new analytical perspectives, research possibilities, and speaking positions emerge.

Only after I started to present my research findings from a completed dissertation in interdisciplinary and international settings did I begin to understand my work politically as a project of cultural translation. I want to describe and analyze German geographies, politics, and cultures of post-Holocaust memory for English-speaking audiences. Perhaps Americans and others who have not yet grappled with dark pasts may learn from the German experience. Unexpectedly, I found that some Germans appreciate this goal of my research. Scholars have thanked me for presenting my arguments in academic settings, stating that they cannot say what I can say because they are German. Consultants have asked me to write letters as an American expert to support certain institutions or projects because they know that my voice has more influence than the voice of a local citizen. It is, of course, due to their generosity with my research that I can speak from the position of “cultural translator.” In return, I also translate from other speaking positions, explaining, for example, why a geographical perspective matters for their work. And this is perhaps the largest advantage of conducting follow-up work. If the researcher is lucky, part of returning may also include cross-cultural and interdisciplinary conversations with research consultants, colleagues, and students—all of whom speak “from somewhere” between their selves/worlds at home and in the field.

#### NOTES

1. Looking at historical photographs, documents, and materials exhibited in museums and educational centers about the rise of National Socialism, World War II, and the Holocaust in a foreign language on a regular basis was not easy. Talking to people about their work was also emotionally exhausting. At the end of many research days I was too depressed, tired, or upset to go “home” and write up field notes. Instead, I found myself going to cafés and other social places to work.
2. I decided not share a draft of this article with my research consultants for reasons of confidentiality.

#### REFERENCES

- Abu-Lughod, L. 1991. Writing against Culture. In *Recapturing Anthropology: Working in the Present*, edited by R. G. Fox, 137–162. Santa Fe, N.M.: School of American Research Press.
- Behar, R. 1993. *Translated Woman: Crossing the Border with Esperanza's Story*. Boston: Beacon Press.
- Bogdan, R. C., and S. K. Biklen. 1998. *Qualitative Research for Education: An Introduction to Theory and Methods*. 3d ed. Boston: Allyn and Bacon.
- Brettell, C. B., ed. 1993. *When They Read What We Write: The Politics of Ethnography*. Westport, Conn., and London: Bergin & Garvey.
- DeVault, M. L. 1999. *Liberating Method: Feminism and Social Research*. Philadelphia, Pa.: Temple University Press.
- Dowler, L. 2001. The Four Square Laundry: Participant Observation in a War Zone. *Geographical Review* 91 (1–2): 414–422.
- Ellis, C. 1995. Emotional and Ethical Quagmires in Returning to the Field. *Journal of Contemporary Ethnography* 24 (1): 68–98.
- England, K. V. L. 1994. Getting Personal: Reflexivity, Positionality, and Feminist Research. *Professional Geographer* 46 (1): 80–89.
- Gilbert, M. R. 1994. The Politics of Location: Doing Feminist Research at Home. *Professional Geographer* 46 (1): 90–96.
- Gupta, A., and J. Ferguson, eds. 1997. *Anthropological Locations: Boundaries and Grounds of a Field Science*. Berkeley: University of California Press.
- Haraway, D. J. 1991. *Simians, Cyborgs, and Women: The Reinvention of Nature*. New York and London: Routledge.

- Katz, C. 1992. All the World Is Staged: Intellectuals and the Projects of Ethnography. *Environment and Planning D: Society and Space* 10 (5): 495–510.
- . 1994. Playing the Field: Questions of Fieldwork in Geography. *Professional Geographer* 46 (1): 67–72.
- Kennedy, E. L., and M. Davis. 1996. Constructing an Ethnohistory of the Buffalo Lesbian Community: Reflexivity, Dialogue, and Politics. In *Out in the Field: Reflections of Lesbian and Gay Anthropologists*, edited by E. Lewin and W. L. Leap, 171–199. Urbana and Chicago: University of Illinois Press.
- Kurzman, C. 1991. Convincing Sociologists: Values and Interests in the Sociology of Knowledge. In *Ethnography Unbound: Power and Resistance in the Modern Metropolis*, 250–268. Berkeley: University of California Press.
- Madge, C., P. Raghuran, T. Skelton, K. Willis, and J. Williams. 1997. Methods and Methodologies in Feminist Geographies: Politics, Practice and Power. In *Feminist Geographies: Explorations in Diversity and Difference*, edited by the Women and Geography Study Group of the Royal Geographical Society with the Institute of British Geographers, 86–111. Edinburgh Gate and Harlow, England: Addison Wesley Longman.
- Mbilinyi, M. 1989. “I’d Have Been a Man”: Politics and the Labor Process in Producing Personal Narratives. In *Interpreting Women’s Lives: Feminist Theory and Personal Narratives*, edited by the Personal Narratives Group, 204–227. Bloomington and Indianapolis: Indiana University Press.
- Nagar, R. 1997. Exploring Methodological Borderlands through Oral Narratives. In *Thresholds in Feminist Geography: Difference, Methodology, Representation*, edited by J. P. Jones III, H. J. Nast, and S. M. Roberts, 203–224. Lanham, Md.: Rowman & Littlefield.
- Nagar, R., and S. Geiger. 1999. Reflexivity, Positionality, and Identity in Feminist Fieldwork: Beyond the Impasse. Paper presented at the annual National Women’s Studies Association conference, Albuquerque, N.Mex., June.
- Nast, H. J. 1994. Opening Remarks on “Women in the Field.” *Professional Geographer* 46 (1): 54–66.
- Oberhauser, A. 1997. The Home as “Field”: Households and Homework in Rural Appalachia. In *Thresholds in Feminist Geography: Difference, Methodology, Representation*, edited by J. P. Jones III, H. J. Nast, and S. M. Roberts, 165–182. Lanham, Md.: Rowman & Littlefield.
- Patai, D. 1991. U.S. Academics and Third World Women: Is Ethical Research Possible? In *Women’s Words: The Feminist Practice of Oral History*, edited by S. B. Gluck and D. Patai, 137–153. New York and London: Routledge.
- PG [*Professional Geographer*]. 1994. Special Issue on “Women in the Field: Critical Feminist Methodologies and Theoretical Perspectives.” *Professional Geographer* 46 (1): 54–102.
- Pierce, J. L. 1995. *Gender Trials: Emotional Lives in Contemporary Law Firms*. Berkeley: University of California Press.
- Pile, S. 1991. Practising Interpretive Geography. *Transactions, Institute of British Geographers*, n.s., 16 (4): 458–469.
- Rose, G. 1997. Situating Knowledges: Positionality, Reflexivities and Other Tactics. *Progress in Human Geography* 21 (3): 305–320.
- Terrio, Susan. 1998. Deconstructing Fieldwork in Contemporary Urban France. *Anthropological Quarterly* 71 (1): 18–31.
- Till, K. E. 1993. Neotraditional Towns and Urban Villages: The Cultural Production of a Geography of “Otherness.” *Environment and Planning D: Society and Space* 11 (6): 709–732.
- . 1996. Place and the Politics of Memory: A Geo-Ethnography of Museums and Memorials in Berlin. Ph.D. diss., University of Wisconsin, Madison.
- . 1999. Staging the Past: Landscape Designs, Cultural Identity, and *Erinnerungspolitik* at Berlin’s Neue Wache. *Ecumene* 6 (3): 251–283.
- . 2001a. Fragments. Ruins. Artifacts. Torsos. *Historical Geography* 29: 70–73.
- . 2001b. Reimagining National Identity: “Chapters of Life” at the German Historical Museum in Berlin. In *Textures of Place: Exploring Humanist Geographies*, edited by P. C. Adams, S. Hoelscher, and K. E. Till, 273–299. Minneapolis: University of Minnesota Press.
- Wolf, D. L. 1996. Situating Feminist Dilemmas in Fieldwork. In *Feminist Dilemmas in Fieldwork*, edited by D. L. Wolf, 1–55. Boulder, Colo.: Westview Press.