

CHAPTER 15

Multi-sited Transnational Ethnography and the Shifting Construction of Fieldwork

In this essay, I explore issues of anthropological practice in the increasingly deterritorialized social reality of the late-capitalist world, in which a fieldworker must negotiate her way between the limits of ethnographic method and transnational spaces. As Akhil Gupta and James Ferguson (1997b) outline, the disciplinary origin of anthropology as a "field science" has led to much of its theoretical and methodological development being centered around the assumption of a well-defined physical site as the appropriate focus for ethnographic fieldwork. Rapid globalization has disrupted this and other associated fundamental assumptions. Increased mobility in the late twentieth century made it difficult to find what were traditionally thought of as suitable locations for ethnographic fieldwork. Current social conditions also require anthropologists to view geographic mobility, transcultural contact, and fluid identities as an integral part of human experience, and to question any naive notions of cultural authenticity and static identity (Appadurai 1990; Hannerz 1996; Clifford 1997a; Ong 1997).

My research on the homemaking practices of Japanese housewives in the United States involves an attempt to look at domestic space as the intersection of the global and the local, and also to examine the implications of corporate-driven transnational mobility on the wives of Japanese businessmen. *Kaigai chuuzai*, an assignment, or the state of being assigned, to a remote, overseas post, has become a significant experience for employees at all levels of Japanese corporations with international business interests (Hamada 1992). As these corporations steadily increased their direct investment in the United States between the late 1970s and the 1990s, the *kaigai chuuzai* of experienced Japanese workers and managers facilitated the transplanting of production know-how and ensured the high efficiency of their U.S. operations. The transnational

migration of corporate Japanese is thus an integral part of the globalizing Japanese economy and its progressively decentralized mode of operation in the global market. At the same time, the ideology of Japaneseness and corporate solidarity may be threatened by the direct encounter with foreign "others." Therefore, transnational Japanese corporations face a dilemma: they need workers who are international enough to function well in a foreign environment, but not so cosmopolitan that they lose their steadfast loyalty to their companies and their country of origin.

Japanese women's domestic labor becomes a key to managing the contradictory requirements of a globalizing economy and the maintenance of Japanese cultural identity. Outside the physical boundary of the nation and away from well-established institutions of corporate socialization, "Japanese" domestic spaces become the primary location of identity-maintenance for expatriate Japanese workers. Middle-class Japanese women are responsible, as wives and mothers, for the construction of "home" and for cultural reproduction within this domestic space (e.g., Lebra 1984; Imamura 1987; Iwao 1993). Transnational Japanese corporations take advantage of this ideology of feminine domesticity as a means of ensuring the productivity of their expatriate workers. In fact, many Japanese employers require that the wives of their expatriate workers accompany their husbands to their foreign stations, and urge them to fulfill their "female duties" by creating a restful "Japanese" home abroad. At the same time, my research has shown that women also reflect on and renegotiate their own femaleness and Japaneseness as a result of their transnational experience.

In his discussion of multi-sited ethnography, George Marcus (1995) analyzes the productivity of multi-sited fieldwork, and encourages anthropologists to test the limits of ethnographic method by moving beyond the reified notions of the "global" versus the "local" and producing an ethnography "in and of the world system," or an ethnography of a cultural formation in the world system that also leads to the understanding of the system itself. He suggests several ways of structuring multisited fieldwork that involve the following: 1) the movement of people; 2) the circulation of material objects—commodities, gifts, money, works of art, and intellectual property; 3) the circulation of signs, symbols, and metaphors; 4) the plot, story, or allegory; 5) the life history or biography of a specific person or a group of people; 6) the conflict; and 7) the strategically situated (single-site) ethnography that focuses on the interaction between the local subjects and the world system (1995:106–111).

All these research strategies have two things in common. First, they follow the transnational flows of people, things, activities, and events that

are configured by the forces of global capitalism to reveal something critical about the workings of the system itself. Second, they are based on the awareness that the workings of the system are visible "in the everyday consciousness and actions of subjects' lives," and reject the assumption that "local knowledge" is constructed merely in a single, geographically bounded site (Marcus 1995:111–112).

My ethnographic project, best characterized as a string of strategically situated single-site ethnographies that loosely follow the movement of Japanese capital in the United States and focus on a particular occupational class, takes Marcus' lead by assuming that local knowledge is constructed in multiple sites. Japan's transnational capitalism has led to varied interests in the United States over time, and I anticipated that the associated demand for women's domestic management may have undergone parallel changes. In order to test this hypothesis I found it necessary to conduct field research in more than one expatriate Japanese community. As a result, I selected three U.S. cities with distinctive expatriate communities: a Midwestern city that I call Centerville (a pseudonym), the greater New York area, and North Carolina "Research Triangle," the area between Raleigh, Durham, and Chapel Hill that is targeted by the state for technology-intensive economic development. My primary informants were married Japanese women who accompanied their husbands on their corporate job assignments in these cities. I interviewed more than one hundred of these Japanese women, and worked closely with several key informants and their circles of friends in three research sites.

Despite rising interest in alternative ethnographic methods that respond to the changing circumstances of our fieldwork, many ethnographers who undertake research in nonconventional sites continue to experience the tension that comes with disregarding deeply internalized disciplinary norms. As James Clifford (1997a: 58) notes, for example, anthropological fieldwork continues to be "seen as a spatial practice of intensive dwelling"; the unconventional ethnographer may also feel driven to create the alibi of exoticism and distance in her field study in order to conform to older conventions. My multisited ethnographic fieldwork in the United States defies the anthropological construction of "places" as a site of academic investigation. It required methodological modifications, which in turn had consequences for the kind of data that I was able to gather. The proximity of my own cultural background to that of my informants also made it necessary for us to negotiate our working relationships in different ways to the dominant trope of anthropologist–other. Finally, questions also arose regarding my own ambivalence toward the "nontraditional" aspects of my fieldwork and my desire for professional credibility.

Pragmatic Concerns

Whether we follow the flow of people, goods, information, or capital, multisited research multiplies the logistical difficulties of any ethnographic fieldwork. As all experienced fieldworkers know, setting oneself up in a new research location costs much money, time, and energy. When an ethnographer starts fieldwork, she has to have a place to live and work, a means of transportation, and enough knowledge to get around. She also has to get to know key people in the social groups of interest and develop rapport with them. In addition, some fieldwork situations may require extensive negotiations with local authorities to obtain research permission. Sometimes navigating through complex political divisions or learning multiple languages or dialects is essential for fieldwork. With much multisited fieldwork, all this has to be done several times over.

Multisited fieldwork can be extremely expensive, and financing it, particularly if one has to maintain two residences (one in the "field" and one "at home"), is potentially very difficult. The fact that I needed a substantial amount of funding to do my multisited research in the United States immediately cut down the number of available grants. To make matters worse, I found few funding sources for which I was eligible to apply. Many funding sources for anthropologists were not available to me, partly because my research location was in the United States and partly because I was not a U.S. citizen. I found that many research grants were intended for "area studies" and required a geographical "fix" (e.g., studying the "Japanese" in Japan) that effectively excluded transnational research like my own. In the end, I found only two funding sources that would even consider funding a non-U.S. citizen to undertake multisited fieldwork for a doctorate in a U.S. institution.

The familiarity of my fieldsites comes first among the reasons why my research diverged from more conventional definitions of the "field." I wanted to work in middle-class American suburbs, on the streets of New York City, and in corporate offices, all of which lack the exoticism of locations more "typical" for anthropological studies. My own anxiety about doing fieldwork in familiar middle-class America, long excluded as an appropriate site for anthropological research, made the start-up phases of multisited fieldwork particularly painful. In any field situation, we struggle in the first few months to get the project going in an unfamiliar place, without knowing where we are going, whom we are meeting, what we are getting into. In a distant, exotic place, though, we have the assurance that everything we do—from asking for a direction to the village chief's house, to negotiating prices at a local market—is a legitimate part of fieldwork.

In my own case, it seemed that all I did in the first few weeks in two field-sites was to make phone calls and to go shopping (to set up my field office and residence)! The problem of this "telephone and shop" phase was that none of the things I was doing appeared to be proper field activities. I also felt an immense psychological pressure to get my supposedly "easy" field project in American suburbia going in the shortest possible time.

While the physical location of my field research was middle-class America, the groups of people with whom I worked were not of those places. Rather, my respondents were expatriate Japanese who resided temporarily in the United States and whose social and cultural lives crossed borders daily. This "misalignment" between the people with whom I worked and the places in which I worked appears to have had a surprisingly jarring effect on the traditional construction of the anthropological field (Gupta & Ferguson 1997a). I continue to feel this sense of misalignment whenever I am asked to identify the geographical "area" of my research. Although I worked in U.S. cities, my research was centered in a particular ethnoscape (Appadurai 1991) within those cities. It was not designed to produce knowledge about "American" cities from the point of view of local players. My expatriate Japanese informants are part of a "tribe" that is outside its natural habitat, a situation that challenges several taken-for-granted anthropological assumptions. One anonymous reviewer of an article I submitted for publication went so far as to state that to document Japanese behavior in an unfamiliar place "will not be a good way to study Japanese culture." While this critique entirely misses the point of conducting transnational multisited research, it also demonstrates the anthropological tendency to look for a perfect match between geography and culture.

Fluid Communities

Expatriate Japanese all over the United States form local social groups of a sort, with tight social networks, certain shared beliefs, and a degree of collective consciousness. People temporarily join these communities of transnational displacement until they return to Japan or move on to another foreign assignment. Thus, they are communities "in transit," lacking both the stability over time and the definable physical and social boundaries of a village community (see Malkki 1997; Passaro 1997). Expatriate Japanese communities in the United States are elusive communities without a "center"—either in the form of a "village plaza," where one can go to meet everybody, or in the form of a visible community

organization. The closest things to community centers are the Japanese grocery stores, where Japanese wives shop for specialty food items and rent Japanese videos almost every day.

The expatriate corporate workers I met, and their families, often seemed to be less interested in building relationships in their temporary homes abroad than in maintaining their social ties back home in Japan. It is perhaps related to this "transient" mentality that, as some of my informants pointed out, relatively little community-building took place beyond a circle of friends and colleagues with whom they form a series of closed cliques. Expatriate community organizations do exist, yet these are either too exclusive (e.g., members-only social clubs, or the parents' association, which expatriates without school-age children could not join) or too business-oriented (aimed at male corporate employees) to become gravitational centers for the expatriate community at large. Religious or political institutions, which might have been important "centers" in other diasporic and immigrant communities, had very small memberships among expatriate corporate Japanese, and thus played only very limited roles.

Expatriate Japanese communities also lacked designated "village elders" to whom an anthropologist could attach herself. Each community had "important people," such as high-ranking executives and their wives or long-term residents whose social status "officially" allowed them a certain amount of influence over a segment of expatriate communities, but in reality, the hierarchical relationships between these individuals and other members of the expatriate community were ambiguous and subject to contestation. The constant movement of people in and out of any given expatriate community also made it difficult for key individuals to develop or exercise control over its social relations across an extended period of time.

My "field" was placeless, also because the social networks of Japanese corporate wives were usually not geographically determined. Expatriate Japanese in the United States tend to live in upper-middle-class suburbs around large cities, and although there were identifiable areas where these families were concentrated in the cities I worked in, the density of households was too low to create a sense of community in most neighborhoods. The car-centered nature of American suburban life also contributed to a lack of neighborliness, as people drove in and out of their garages without many opportunities to casually interact with their neighbors. In addition, Japanese wives tended to build their friendship networks on more personal criteria than physical proximity: in their minds, the match between two people's ages, family compositions, interests, tastes, and lifestyles was of utmost importance in selecting friends (cf. Motoyama 1995). When I asked my informants how they met other Japanese wives,

a few common answers emerged: at the English classes or hobby practices, through common friends, or at their children's school. Even when expatriate wives became friendly with their neighbors, their initial encounter tended to occur away from their homes. Once I managed to "locate" my potential informants, and tap into their social networks, I had to come to terms with the spatial and temporal ephemerality of my field.

At the homes of my research participants, surrounded by ceaseless Japanese chattering and the imported Japanese videos running in the background, I used to get caught in a strange sensation of not quite knowing where I was. The place—an indistinct-looking house in a middle-class American suburb—and the scene—an afternoon gathering of Japanese mothers and assorted children—clashed and collided. The disparity between the place and the space became even more visible when I went out with my informants to public places, like a shopping mall. It was as though we were moving around, contained in a bubble of our own atmosphere in a deep sea, with strange creatures swimming around us, completely incompatible and incommunicative with our kind.

Temporality

It was not only space but also time which structured and punctuated my informants' lives: my "field" vanished daily at 6 P.M. sharp. The house where we were gathering had to be turned over to another function, for the tired husband of our hostess would soon return home, expecting a hot meal and a restful evening. It was like one of those dramatic scene changes in *kabuki* theater, in which a whole set turns upside down and disappears into the basement, to make way for the new set that emerges out of the bottom of the stage. In fifteen minutes or less, all the traces of the daytime gathering were gone, children's toys and books put away, dirty coffee cups washed, the cost of lunch and afternoon sweets squared away, the mothers and children in their cars and on their way home. Until the next lunch date, or another trip to the mall, my "field" dissipated into thin air. I thought often of a Japanese proverb, *youchi asagake*, or "strike by night, run in the morning," or in this case, *hiruuchi yuugake* (strike by noon, run in the evening).

I wondered whether these "hit-and-run" encounters constituted "real fieldwork." As a soon-to-be professional academic, I also feared that I would gain less academic capital without the alibi of coresidence and cultural immersion (Gupta & Ferguson 1997a; Marcus 1998). Being a transnational multisited fieldworker thus meant for me the constant

questioning of my own fieldwork choice, anxiety over the question of authority (Clifford 1983), and self-doubt about the validity of the knowledge that this form of fieldwork was going to produce.

Networking

Japanese expatriate communities around the United States are close-knit, meaning that "everyone knows everyone else's business," as many of my informants would put it. Members of these communities are very cautious, even skeptical, of outsiders, and entry usually requires some sort of introduction by an already-recognized member (not unlike gaining membership to a prestigious country club). The study populations in my three research locations were independent of one another, which meant that I had to develop new research networks in each location through a lengthy and often stressful process.

New York was the hardest place to get my fieldwork going. I had no local lead to begin with, and when I arrived in my fieldsite and began to call every expatriate Japanese organization I could think of, I found them to be extremely challenging to work with. Many of them had an official front person—a public relations manager, for example—whose job is to screen and guard against unwanted outsiders penetrating their organization. I was often politely but decisively brushed off, referred to other organizations, denied information, and even discouraged from doing my research altogether. It was partly the "big city" mentality that got in my way, but it also reminded me of living in Japan, where personal introduction is everything.

The only organization that was open to my research was *Nihonjinkai*, a grass-roots group of Japanese Americans in and around New York that also included some expatriate Japanese members. Although I met a few Japanese corporate wives through *Nihonjinkai*, I was still unable to enter this exclusive expatriate Japanese community. This in itself is an interesting commentary on the relationship between the expatriate corporate Japanese and Japanese-American community. There was marked tension, for example, in and around *Nihonjinkai*, which was started by Japanese immigrants who settled around New York before World War II and had been organized by their descendants and more recently arrived immigrants. However, since the 1980s, the growing number and public status of expatriate corporate Japanese began to overshadow the Japanese-American community so that some members of *Nihonjinkai* expressed

concern that the organization would be "taken over" by the corporate Japanese. This tension also reveals different attitudes among expatriate corporate Japanese and Japanese Americans toward "outsiders." To expatriate corporate Japanese, I, a graduate student from a U.S. university with no known ties to the Japanese corporate world, was a suspicious outsider, while the community of Japanese Americans and more recent immigrants accepted me with little resistance.

In the end, the answer was back home. Realizing that the great majority of expatriate Japanese in New York came from the Tokyo headquarters of their respective companies, I decided that I needed to work my way in from the other side of the Pacific Ocean. My only "business" contact in Tokyo was, unfortunately, my father, who had worked in the corporate world for more than forty years and had an extensive professional network. I say "unfortunately" not simply because of the "professional-personal" boundary issues: I was aware that accepting his assistance would draw me into a web of obligation-driven relationships, worst of all, with the very man whose obliging and imposing ways I had, for a long time, shied away from. My anxieties were later realized, when my father gave me a number of tasks on behalf of those individuals who had helped me get connected in New York. These included attending a dinner my father set up with some of those contacts, gathering and translating college application material for one of their daughters, and offering to be a tour guide. As the Japanese saying goes, "there is nothing more expensive than a free gift."

Aside from my ambivalence about incurring social obligation, this strategy was amazingly effective. A few days after I talked with my father about finding contacts in the New York area, he faxed me a list of five individuals—all expatriate managers of major Japanese corporations—who were "more than happy" to help me with my research "in any way possible." Two of them called me before I even had the chance to call them, offering their assistance and asking for the details of my research needs. This incident also gave me an insight into the "Tokyo-centered" worldview of expatriate Japanese workers, who were physically here in the United States, but psychologically "always facing Tokyo," as one of my informants put it. Some of the informants I got to know through this network were high-ranking managers of prominent U.S. subsidiaries who were unlikely to give time to research that did not promise any immediate benefits to their businesses. Although I eventually developed my own local network, I would not have been able to gain access to these informants without the "Tokyo" connection.

Partial Knowledge?

Field research among mobile and fluid transnational populations is an operation in "scenes" and "spaces"—rather than "sites" or "places"—that exist only as they are "practiced by people's active occupation, their movements through and around [them]" (Clifford 1997a:54). When one's informants themselves are constantly negotiating their social spaces and traversing multiple scenes, the more traditional method of intensive coresidence is often impossible or ineffective. In this field of operation, the ethnographer's mode of existence also becomes transient, as she travels around and slips in and out of her "field" along with her informants. In response to such changed conditions Clifford (1997a:55–58) offers "deep hanging out" as an alternative method to traditional "dwelling." This approach enables the ethnographer to share certain aspects, segments, or scenes of her informants' hybrid lives in socially defined time and space. While ethnographic knowledge has always been "partial," the question still remains as to whether this alternative method produces knowledge of people's lives that is as thick and as intimate as what comes from traditional participant observation.

Recognizing the worldliness of contemporary Japanese, their global consciousness, and their participation in transnational capitalism, I knew that it would be duplicitous to view or represent them as some kind of culturally unique, homogeneous group. Living with an expatriate Japanese family was also not a feasible option. Nuclear families, the living arrangement of most of my expatriate informants, are private sanctuaries in Japanese culture, closed to intrusions by "outsiders," except on some special occasions. Housing a stranger without appropriate social or professional connection was almost unheard of in contemporary middle-class households (see, for example, Kondo 1990; Rosenberger 1994). There was no financial incentive to expatriate Japanese families in subletting out a room, either, since their housing was subsidized by their employers.

It struck me one day that, in fact, the way I "knew" my informants was no different than the way they "knew" each other. The thought was prompted by a comment that I overheard one day in Kawagoe-san's living room. One of her friends remarked to another that she had never seen the other woman's home, although they had grown very friendly with each other at Kawagoe-san's. The members of this group of Japanese women were good friends and shared a lot of time together during the week. But at the end of the day they each went home, their associations not always reaching into their individual domiciles. If the relationships among my research subjects were themselves fluid and temporary, then it

would be artificial and arbitrary to create a coresidency situation for the sake of anthropological tradition. Although the nature of ethnographic data gathered under these fleeting situations is in some ways different from what I might have been able to learn in a more conventional fieldwork setting, it is representative of the domain I was working in.

No fieldworker is able to "know" her informants' lives in their totality. Even if we manage to move in with an informant's family and follow them around day in and day out, we are, to paraphrase Clifford Geertz, always listening in on a conversation that started before we arrived and that will continue after we have left. I am aware that, by not spending twenty-four hours a day with my informants (if such an approach is, or ever has been, possible), I missed the opportunity to directly observe their interactions at home and to gain knowledge of private family matters that would not be discussed outside the home. My data on expatriate Japanese family life primarily came from the verbal accounts of Japanese wives, particularly from a dozen key informants. The few occasions where I observed family interaction were in public places, on social occasions, or in prearranged visits to their homes. This would have been problematic if my ultimate research goal was to generate a composite picture of expatriate family life, but, given my research questions, which were centered on the subjective experiences and views of corporate wives and the relationships among the women, the nature of my interactions proved most appropriate.

I felt that other sources of information, such as interviews and casual conversations with husbands, comments from mental health professionals, published materials, and the few direct observations I was able to make, provided sufficient points of cross-reference. Perhaps my Japanese origin had more advantages in this particular fieldwork setting than in some others, because my data collection depended so much on "talking" with my informants, on understanding subtle verbal nuance and nonverbal signals. And, although I do not wish to pose as an all-knowing native anthropologist, I often sensed that my female gender and Japaneseness had something to do with the sincerity and candidness of my Japanese informants. Many female informants seemed to find it more comfortable, for example, to speak to another Japanese woman than to a foreign and/or male investigator. Middle-aged informants with grown-up children often took fatherly/motherly interests in my success in the United States. In fact, I was often overwhelmed by the weight of stories I heard, as my informants expressed profound feelings and poured out memories of very significant events in their lives.

Being a "free agent," without a residential affiliation to any particular family, also had its advantages. As in any fieldwork situation, the compli-

cated *ningen kankei* (human relationships) of expatriate communities may have meant that having a strong affiliation with a certain household could have limited my social mobility as a researcher. People would be more cautious and selective about what they could or could not tell me; some people may have even declined to talk to me at all, if they did not get along with my hosts. At the same time, I would have been obligated to respect my host family's social status, and would have had to refrain from contacting people with whom they did not feel comfortable. My neutrality was also important to those informants who had strong concerns regarding confidentiality. They knew full well that once a story got into an expatriate rumor mill, there was no knowing where it might end up (see Mori & Saike 1997). For this reason, my informants often found it easier to speak candidly to an unattached anthropologist than to their friends within the expatriate community.

The connection that I managed to establish and maintain with many of my female informants was not the natural outcome of our affinity as "Japanese women," but the result of constant negotiation and reflection. It was certainly convenient that I spoke Japanese fluently and was familiar with cultural idioms, and being a woman did make it easier for me to "fit in" to what were primarily homosocial situations. However, my Japaneseness and femaleness were less than unambiguous, as my informants and I found out, and we had to work out our differences and similarities through the course of my fieldwork. When a female "Japanese" anthropologist is beginning fieldwork among Japanese housewives, she has as much work to do as any other anthropologist, in order to negotiate and establish good working relationships with her informants (Narayan 1993). In some ways, the process may sound easier for the reasons I have just discussed; but in other ways it was also quite complicated, often for the very same reasons.

Locating Myself

When I started my fieldwork in Centerville, I had the hardest time making my potential informants understand that it was an important part of my fieldwork to "play" (or *asobu*) with them. I used to hang out at Kawagoe-san's house, spending long afternoons "playing" with Japanese wives and their children. Kawagoe-san and her friends understood that I was a graduate student, and that I was staying in Centerville for several months for my field research. They also understood that I was interested in them as "study subjects," and that I wanted eventually to interview

them about their experiences in the United States. But the concept of "participant observation" or "informal interaction" seemed to completely escape them. In the beginning, when I showed up at Kawagoe-san's place for an afternoon of sitting around and talking, they'd ask politely, "How is your study going, Sawa-san? You are getting away from your work to join us this afternoon?" I tried, in vain, to explain that it was part of my "work" to be there. But in my informants' minds, Kawagoe-san's home was a realm of "play," so it did not make much sense to say that I was there to "work."

A couple of months after I started visiting Kawagoe-san and her friends, they began to talk about how funny it was that I could actually be "working," while I was doing nothing but "playing" with them. Then, one of the women exclaimed, "Ah, I understand, Sawa-san is an *asobinin*!" An *asobinin* means literally a person who plays, or a loafer without recognizable and honest means of making a living. They may be gamblers, "wise guys," or someone rich enough to not need a job. The livelihood of an *asobinin* is play, instead of work. *Asobinin*'s marginality also serves as a symbol of resistance against social norms and restrictions, slipping in and out of two social milieus. Thus, the reference to *asobinin* made two moves of signification in one stroke, identifying me as someone who played professionally, and also as someone who defied Japanese social conventions in her professional play.

The *asobinin* discussion summarized the differences these corporate wives perceived between myself, "playing" as I worked toward my professional qualification, and their own lives, which are closely tied to domestic concerns, even in their play. These differences had to be verbalized, in part, because of our similarities, because I was simultaneously like them and unlike them. My physical appearance was one example. For anthropologists who conduct field research among phenotypically distant others, "going native" may be an alluring prospect, a wild fantasy of fieldwork. Similarity in physical appearance may, in contrast, prove threatening to the anthropologist's self. Dorinne Kondo, a Japanese-American anthropologist who is physically indistinguishable from the Japanese among whom she worked, struggled with the sense of blurred self-other boundaries during her fieldwork in Tokyo (Kondo 1990). My field situation has some similarities with Kondo's experience, in the sense that I, too, could look like a "Japanese wife." However, the similarities between myself and my informants went a little deeper than our skin color.

My habitus was also very similar to that of the Japanese corporate wives I worked with. Having grown up in the 1960s through 1980s in upwardly mobile middle-class Japanese families, many of us were given

similar choices, and accepted the same definitions of happiness and accomplishment. In other words, we did share something close to Geertz's notion (1973) of "culture" as a system of meanings and worldviews. Indeed, while growing up in Japan I firmly believed that I was going to be a housewife and a mother. I carried a vague image of a husband who worked for a corporation or a government office, and understood that my chances of living overseas would come through the man I would eventually marry. The divergences between my life trajectory and those of my informants occurred only in young adulthood. When our paths crossed in a foreign city, and chance occurrences put us in the same room at the same time, we found our similarities and differences hard to reconcile. My concern was never about becoming one of them but of being reminded that I had no place in their world, because I had turned away from it by moving to America and choosing an academic career.

My "loafer" identity certainly had a pragmatic advantage in my fieldwork situation. It signified that I had a lot of free time and nothing important to do—which was essentially how these women perceived their own lives. Eventually my corporate-wife informants became convinced that it was no imposition to call me up at any time of the day and night, to gossip, to ask me out, or to ask favors of me, all of which dramatically increased my opportunities to "hang out" with them in different settings. More important, the negotiated identity of ethnographer as "loafer" led to a mutually eye-opening relationship, contesting and rewriting the formula of the anthropologist and her cultural "other." In subsequent encounters with these women our conversation often centered on our differences and our reflections on how those differences were produced. We were able to be playful with our differences. For instance, they would tell newcomers to the group that I claimed to "study them," yet what I really did was just hang out with them; or, in reverse, I seemed to be "playing," but actually I was cunningly studying their behavior. Such exchanges resulted in moments of profound reflection, unspoken yearning, and recognition of the possibilities for transformation.

Conclusion

I have outlined above the process of doing multisited ethnographic fieldwork with transnationally mobile groups of middle-class Japanese. In choosing my fieldsite, I consciously selected a particular social, rather than geographic, space located at the intersection of transnational capitalism and cultural identity, a space in which the "global" is localized and

the "local" is globalized. In the process, I discovered that anthropology's traditional preoccupation with fieldwork in faraway places continues to have a powerful grip on how fieldwork practices are framed and on how professional credibility is attributed. The lack of resources available for multisited fieldwork in the United States, unexpected logistical complications, and questions about the validity of relatively novel ethnographic practices all combined to make my fieldsite an "awkward space."

However, this awkward space ultimately turned out to be an incredibly rich source of interaction, in which my informants and I negotiated our understandings of transient lives in an increasingly globalized world. George Marcus contends that multisited ethnography "in and of the world system" requires the ethnographer to situate herself within that system, instead of speaking "from above or 'nowhere'" (Marcus 1995:112). It certainly felt awkward to work against the traditional construction of the field. But, at the same time, I managed to locate myself in the most productive place for my work and my self to become part of my informants' knowledge of the world, while I learned from theirs.