

Chapter Two

Epistemology, Fieldwork, and Situated Knowledge

Every judgment in science stands on the edge of error, and is personal. Science is a tribute to what we can know although we are fallible.

Jacob Bronowski (1973:374)

Feminist objectivity means quite simply *situated knowledges*...The goal is better accounts of the world, that is, “science.”

Donna Haraway (1988:581, 590)

It is conventional in social science to account for research methods, explaining how one came to the knowledge presented, establishing both the basis and limits of this knowledge. While that is its purpose, this chapter moves beyond matter-of-fact description of the study and methods of data collection to produce a different sort of account. In it I seek to: (1) describe my field research and methods; (2) explain the epistemological and disciplinary grounds from which my work proceeds; and (3) address questions of my positionality as the ethnographer of a community I helped create, lived in as a member for many years, and in which I remain bound. Put simply, the goals are to outline my research, situate my work in anthropology, and situate myself in the work.

Central to these objectives—and implicit in the research project itself—is an understanding of knowledge as socially constructed, partial (Geertz 1973, 1983; Clifford and Marcus 1986), and situated (Abu-Lughod 1991; Clifford 1988; Haraway 1988; Rosaldo 1989), rather than universal and transcendent; and of ethnographic

fieldwork as an intersubjective process (Briggs 1970; Geertz 1971; Rabinow 1977) that involves “the whole self” (Kondo 1986). I refer to this perspective as *situated knowledge*. While the first section of this chapter presents a fairly conventional description of my research project, fieldwork methods, and data collected, I turn in the second section to explain what I mean by situated knowledge and identify the theories and practices of anthropology in which it is grounded. Not simply background, this context defines the basis for my claims to knowledge, guiding the conception of my Cyborganic research; shaping my practice in the field; and informing the representations and analysis in this ethnography. Drawing on this context, my third objective is to put situated knowledge into practice by addressing questions of objectivity, subjectivity, and positionality, in my work as the ethnographer of a community I helped build and have been part of for going on fifteen years. In a sense, the third section of the chapter, applies the theory described in the second section to the practice described in the first.

Finding the Field

On August 15, 1993, three days after handing in my Masters thesis, I packed a moving van with my worldly goods and drove from Los Angeles to San Francisco to become a participant-observer of the scene that was flourishing there around techno music, raves, multimedia, and networked computing. I felt an urgency to get into the field to investigate the groundswell of excitement and activity around new media technologies in the Bay Area. My awareness of this scene—and its palpable

Zeitgeist of enthusiasm for the expressive and socially transformative power of computer media—had been growing all year.

That January, I began collaborating with my college friend Jonathan Steuer on a grant proposal to conduct video research of computer use at Apple Computer's Vivarium magnet school in Los Angeles. Steuer, then a doctoral student in Communication at Stanford, lived in San Francisco in an apartment on Ramona Avenue that he shared with two roommates. In the course of working on the grant proposal, I made several visits to San Francisco and stayed at this apartment each time. I slept on the couch, ate with Steuer and his roommates, and joined them in the social activities of their everyday lives. The group household and “techno scene” in which its members participated impressed me. All in their twenties, Steuer and his housemates were a close group, sharing grocery and household expenses; coordinating tasks; attending raves, parties, and other multimedia events; and cooking and eating together along with other members of their growing community. I was particularly fascinated by this social life because at the time I was writing a Masters thesis on suburban homeowners and the subject of community was on my mind.

What struck me was the stark contrast between the lives of my suburban informants and the vibrant, urban life I saw in San Francisco. While the homeowners I studied, pink collar and middle-aged, were isolated via long commutes, bedroom communities, divorce, and the wider destabilizations of U.S. family and civic life;

the young “net ravers” I met in San Francisco were working to build lives against such isolation. They were using new technologies to pursue the same sort of lifeworld (Habermas 1987) projects my suburban informants pursued through homeownership; and were talking deliberately, and rather evangelically, about building community through “the Net.” My brief experiences of some of the communities that they were part of affected me profoundly. Cognitively and professionally, I saw a rich research subject; affectively and personally, I saw the kind of collaborative community to which I aspired. Thus, my visits to San Francisco in the spring of 1993 were my entry point into the San Francisco communities I would set off to study that fall.

A second, equally important, entrée also came about through collaboration on the Vivarium grant proposal. In order to facilitate working together at a distance, Steuer gave me an account on his department’s host computer, *casa.stanford.edu*. This was my first Internet account and facilitating work was only one of the things it was good for. It helped me maintain connections made during my visits to San Francisco after I returned to Los Angeles to finish my Masters thesis. It gave me access to the many wonders of the pre-Web Internet (mailing lists, Usenet, FTP and Gopher servers); and—later in 1993, after the release of Mosaic 1.0, the first popular Web browser—to the World Wide Web. The account opened a whole new world to me. Yet, rich and strange as it was, it was also a world populated, at least in certain neighborhoods, by welcoming, familiar faces: people I’d met in San Francisco, or

heard of, friends of friends, and fellow travelers. With online friends to demonstrate how and why they used the Internet and, most importantly, to use it with me in practice, I began to understand their enthusiasm. I also began to appreciate the power and value of combining face-to-face and online interaction. In this way, my visits to San Francisco in early 1993 and my first Internet account served as key points of entry into the research questions and fieldwork of this dissertation.

In addition to the sense that something anthropologically significant was taking place online and in San Francisco, my decision to go into the field to study the techno-communitarian practices and discourses I encountered there had other personal and practical inputs. One was a response to Donna Haraway's "A Manifesto for Cyborgs" (1985) that urges all who would oppose domination (in its myriad forms) not to let technical knowledge remain the province of the "high-tech boys." I read Haraway's appeal as a personal call to take up ethnographic study of high-tech culture. Thus, when my friend Steuer asked in mid-1993 if I would be interested in collaborating with him on the Cyborganic project, I saw my opportunity. By the end of the summer, I had packed up my life in Los Angeles, and set off for the Bay Area to begin the fieldwork on which this dissertation is based.

The Research

My Cyborganic study followed the ethnographic tradition of participant-observation in which the anthropologist lives among those he is studying during an extended period of fieldwork. From August 1993 to June 1999, I lived and worked in

San Francisco among members of the Cyborganic community. As a participant, I resided in a group household in the neighborhood Cyborganic came to occupy; worked at Netscape Communications and a number of other Internet start-ups, including the Cyborganic Corporation; and took part in a variety of cultural activities and non-work communities, such as SFRaves, Anon Salon, and Burning Man. As an observer, I researched telecommunities for the Institute for the Future (IFTF); and pursued my own ethnographic work conducting interviews, writing field notes, studying and archiving the media generated by my subjects (Appendix A). Though I was a founding member of the Cyborganic project, fellow members knew me as a social anthropologist who had come to San Francisco to study networked communities and the burgeoning cyberculture.

Two IFTF research projects were particularly valuable to me in situating Cyborganic within the wider context of Silicon Valley culture and industry. The first was a study that examined the intersection of corporate intranets and online communities through case studies of groups at Hewlett-Packard, Silicon Graphics, and Cyborganic (IFTF 1997a). I developed the interview questions for the study (Appendix B) and, with three other researchers, conducted ethnographic interviews with group members. The interviews with Cyborganic members provided far more material than was taken up in the IFTF study and they have proven a valuable source for this ethnography.

In the course of this IFTF study, our research team also elicited a group history of Cyborganic at an open meeting held at Cyborganic's offices on December 19, 1996 and attended by fifty-three community members. Two IFTF researchers (Andrea Saveri and Tomi Nagai-Rothe) facilitated the meeting using techniques honed in previous sessions with stakeholders in online communities. The main elicitation device was a huge sheet of paper (approximately 4 feet by 30 feet) affixed to one wall of the large, open space. The paper was blank except for a timeline with years marked out across the top (x-axis), and a column down the left side (y-axis) identifying the categories of information we wanted to collect. The categories were: "big ideas and themes;" "reflections and learnings;" "trends;" "stories;" "events;" and "challenges." Cutout paper figures were provided and people were asked to write their names on these and attach them to the map at whatever point they became part of Cyborganic. The facilitators then asked group members to share reflections and recollections of Cyborganic, recording these on the map as they went along, pausing periodically to ask questions or moderate discussions that broke out. The group history gathered in this session was used to produce an illustrated timeline of "Cyborganic History and Evolution" (Appendix C) that has served to guide the chronicle and ethnography presented in chapters 4, 5, and 6.

In 1997 I participated in a second IFTF project as a visual ethnographer collecting images and conducting interviews for the workshop "Images and Stories of a New Silicon Valley: Transformation of Consumers, Communities, and Public

Space through New Media Technologies” (IFTF 1997b). The workshop looked at San Francisco’s South of Market Street area (SOMA), as a prime example of key shifts in technological innovation and development, including the growing role of consumers and users in driving innovation, and the phenomenon of “outposting” whereby businesses in established regions (such as Silicon Valley) spawn nearby outposts in a process of “short-distance decentralization” (Castells and Hall:235). Just as my earlier work for IFTF, this project provided valuable comparative data for my study of Cyborganic, pointing up those aspects of the group that were part of broader social trends.

The Grounded Theory Method

Essentially, my research has proceeded according to the grounded theory method of qualitative research. This method “stresses discovery and theory development” rather than reasoning from “prior theoretical frameworks” (Charmaz 1983:110). I began researching the techno-communitarian practices and discourses of San Francisco geeks because they appeared to represent an innovative response to the problem of making meaningful social connection in the highly individualistic and mobile society of late twentieth century America. I wanted to study this response anthropologically, examining the practices and social imaginaries of community that it entailed. In my initial period of field research I did not draw directly on any theoretical frame. Yet, in grounded theory, data collection and analysis proceed simultaneously: as my interests developed and specific themes and categories began

to take shape, I started to examine the pertinent literature. Ultimately, in writing up my research, I worked to situate my own interpretations in relation to this scholarship.

Situated Knowledge: Objectivity as the View from Somewhere

As the epigrams at its head suggest, the basic premise of this chapter is that scientific knowledge is partial, personal, and situated; and even so, the production of better accounts of the world¹ remains its goal. Neither universal, nor transcendent, its claims to knowledge do not rest on the separation of knowing subject from known object. This understanding draws on Donna Haraway's critical theorizing of a feminist objectivity (1988) and also on the reconfigurations of thought and practice with which anthropologists have responded to critiques of the discipline's colonialist and positivist legacies. As this discussion will show, both address the problem of what can count as knowledge after the collapse of the Enlightenment's grand narratives.

In "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective" (1988), Haraway proposes a feminist objectivity that charts a course between a radical constructivism in which science is merely "a contestable text," and a critical empiricism anchored in the modern projects of humanist rationality. She argues that feminists cannot be content with critiques of

¹ By this I mean, to use Haraway's words, "enforceable, reliable accounts of things not reducible to power moves and agonistic, high-status games of rhetoric or to scientific, positivist arrogance" (1988:580).

positivist and humanist objectivity; and that they need their own doctrines of objectivity—their own ways to talk about reality, and better or worse accounts of it.

The problem, as Haraway sees it,

is how to have *simultaneously* an account of radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own “semiotic technologies” for making meanings, *and* a no-nonsense commitment to faithful accounts of a “real” world. (Haraway 1988:579)

The solution Haraway proposes for this combination of knowledge practices is “a usable, but not an innocent, doctrine of objectivity” that she refers to as “situated knowledge,” “positioned rationality,” and “views from somewhere.” This situated objectivity differs fundamentally from Enlightenment doctrines of detached scientific objectivity. First, “partiality and not universality is the condition of being heard to make rational knowledge claims” (Haraway 1988:589). As Haraway writes, “only partial perspective promises objective vision. ...Feminist objectivity is about limited location and situated knowledge, not about transcendence and splitting of subject and object” (1988:583). Second—while Enlightenment objectivity rests on distance between subject and object, and on knowledge as the view from nowhere or above (a disembodied, unlocated, unaccountable subject position)—situated objectivity asserts the connection of subject and object, and posits rational knowledge as embodied “views from somewhere” (Haraway 1988:590).

One of the most significant consequences of this understanding of knowledge is a radically altered view of subject-object relations. The subject-object split of

positivist objectivity is not only refused, it is replaced with a vision of objects as actors and of scientific inquiry as “power-sensitive conversation.”

Situated knowledges require that the object of knowledge be pictured as an actor and agent, not as a screen or a ground or a resource, never finally as slave to the master that closes off the dialectic in his unique agency and his authorship of “objective” knowledge. The point is paradigmatically clear in critical approaches to the social and human sciences, where the agency of people studied itself transforms the entire project of producing social theory. Indeed, coming to terms with the agency of the “objects” studied is the only way to avoid gross error and false knowledge of many kinds in these sciences. But the same point must apply to the other knowledge projects called sciences. A corollary of the insistence that ethics and politics covertly or overtly provide the bases for objectivity in the sciences as a heterogeneous whole, and not just in the social sciences, is granting the status of agent/actor to the “objects” of the world. Actors come in many and wonderful forms. Accounts of a “real” world do not, then, depend on a logic of “discovery” but on a power-charged social relation of “conversation.” The world neither speaks itself nor disappears in favor of a master decoder. ...In some critical sense that is crudely hinted at by the clumsy category of the social or of agency, the world encountered in knowledge projects is an active entity. (Haraway 1988:593)

Haraway is explicit that the view of “objects as actors” pertains to all sciences. She explains that she has “refused to resolve the ambiguities built into referring to science without differentiating its extraordinary range of contexts” in order to foreground precisely this “field of commonalities binding exact, physical, natural, social, political, biological, and human sciences” (Haraway 1988:591).

The paradigm shift towards more active and interactive conceptions of objects of knowledge is one Haraway identifies, not only with late-twentieth century feminism and social theory, but also with the biological sciences, in particular with

the radical re-conception of genes in terms of coding and decoding, and of genetics as communication. Haraway's situated objectivity joins the view of *knowledge as constructed* to the everyday practice of science. For example, she writes that by insisting on "a better account of the world," feminists find themselves "conjoined with the discourse of many practicing scientists, who, when all is said and done, mostly believe they are describing and discovering things by *means of* all their constructing and arguing" (1988:579).

The understanding of situated knowledge with which my Cyborganic research proceeded draws from Haraway directly, as well as indirectly, through her influence on anthropology (e.g., Gupta and Ferguson 1997c; Downey and Dumit 1997). But it also draws on the reconfigurations of thought and practice that have taken place in anthropology since the 1960s. Fundamental transformations in the discipline's traditional object, core concepts, and central practices created the conditions that made my Cyborganic project realizable *as anthropology*. By turning briefly to discuss these transformations I seek to explain their formative role in my research and the epistemology of situated knowledge that grounds it.

Anthropology Reconfigured: Ethnography without the Ethnos

My study of Cyborganic bears little resemblance to the traditional image of anthropology as the study of so-called primitive society, and ethnography as the description of "a culture." Of course, that image of anthropology, with its "peoples

and cultures” approach, has been outdated in practice for quite some time. As Akhil Gupta and James Ferguson observed over a decade ago,

Ethnographically, much of the best work today no longer fits within the model of a study of “a culture,” while the most challenging contemporary fieldwork cannot be contained within the stereotypical “among the so-and-so” mold. What would once have appeared a logical impossibility—ethnography without the ethnos—has come to appear, to many, perfectly sensible, even necessary (Appadurai 1991). Theoretically, too, a move away from the “peoples and cultures” vision of the world, always a live concern for a small section of anthropologists, appears to have become a leading position within the discipline. (Gupta and Ferguson 1997b:2)

This shift represents a critical reconfiguration of concepts and practices that once defined the discipline. The view of “cultures” as bounded wholes and of “a culture” as “an integrated totality,” “a universe of shared meaning,” has given way to new theorizations and practices of anthropology. The sources of these shifts are many and varied. Some, such as post-colonialism, post-industrialism, and transmigration, are phenomenal². Others are intellectual currents that have been active in anthropological theory since the 1960s (Ortner 1984). Scholars working in political economy, for example, had long argued against the view of isolated cultures in favor of a world system connected through historical processes (Gunder Frank 1967; Wallerstein 1974; Wolf 1982; Mintz 1985). And the theoretical orientation to practice that Sherry

² David Harvey has written of postmodernity as an historical condition—“a sea-change in cultural as well as in political-economic practices since around 1972” (1989: vii)—and anthropologists have similarly recognized that the fragmented, decentered, compressed, flexible, and refractive terms used to describe the contemporary world reflect material and phenomenal shifts, not simply a postmodernist ideology (Hannerz 1987, 1989; Appadurai 1990; Abu-Lughod 1991; Fox 1991).

Ortner identified as “the key symbol of eighties anthropology” (1984:158) emphasized agency; the relations of human action and “the system;” and the negotiated, conflictual nature of social life and reality. Both of these approaches have contributed to my Cyborganic research and view of situated knowledge.

However, rather than chart the sources of my methods and epistemology comprehensively, I focus here on the response of anthropologists to the “crisis of representation” precipitated by critiques of the discipline’s traditional practices of knowledge making (Marcus and Fischer 1986). These have been a major impetus for the reconfigurations of anthropological thought and practice that enabled my Cyborganic research. Moreover, though not discussed directly, I would argue that the influence of political economy and the practice perspective is implicit in this focus: both in the critiques that brought about the crisis; and in the theory and practice of anthropologists who responded to it. Most significantly, responses to “the crisis” have served as sources for my understanding of situated knowledge, addressing to ethnography the very questions of scientific objectivity Haraway raised. Thus, by turning briefly to discuss anthropology’s crisis of representation, I seek to give a more specific, more anthropological, explanation of situated knowledge and its application to my Cyborganic work.

After the “Crisis,” an “Experimental Moment”

The “crisis of representation” refers to the postmodern, postcolonial, postfeminist erosion of paradigmatic authority that beset anthropology and other

fields in the human sciences during the 1980s. This crisis posed significant challenges to anthropology—a discipline grounded in the Enlightenment project of rationality and objectivity and intimately bound up in the history of Western imperialism. Beginning with the critiques of Dell Hymes, Edward Said, Johannes Fabian, Clifford Geertz, James Clifford, and George Marcus and Michael Fischer, anthropologists were criticized for their unself-conscious production of cultural representations.³ Descriptions and analyses written from observations and fieldnotes—the very heart of ethnography—were called in to serious question, epistemologically, as well as politically (Lutkehaus and Cool 1999). Anthropological knowledge was shown as socially constructed, its authority and objects (“natives,” “cultures,” “traditional society”) narrative effects, rather than natural facts (Clifford 1988:10). The so-called primitive Other—isolated spatially in a faraway place and stranded temporally in the ethnographic present—was revealed as an invention of the discipline (Fabian 1983; Kuper 1988; Appadurai 1988).

Responding to the critiques, many anthropologists explored new strategies for representing “an emergent postmodern world” and worked to “make ethnographic writing more sensitive to its broader political, historical, and philosophical

³ Hymes, *Reinventing Anthropology* (1974); Said, *Orientalism* (1978), “Representing the Colonized: Anthropology’s Interlocutors,” (1989); Fabian, *Time and the Other: How Anthropology Makes Its Object* (1983); Geertz, *Works and Lives: The Anthropologist as Author* (1988); Clifford, *The Predicament of Culture: Twentieth Century Ethnography, Literature, and Art* (1988); Clifford and Marcus, eds., *Writing Culture: The Poetics and Politics of Ethnography* (1986); Marcus and Fischer, *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (1986).

implications” in what came to be called the “new ethnography” (Marcus and Fischer 1986:vii). These new ethnographies took a variety of forms, but shared in a self-conscious effort to portray the socially constructed nature of anthropological knowledge. Some focused on the dialogical nature of ethnographic inquiry (Crapanzano 1980; Dwyer 1982; Price 1983), others on revealing fieldwork as an intersubjective process (Briggs 1970; Geertz 1971; Rabinow 1977). Reflexivity, the practice of representing the ethnographer as a particular individual in the work—rather than an all-seeing, but unseen, authorial voice—was a common feature of these ethnographies; and some researchers began to practice a “reflexive anthropology,” critically considering their own cultural biases and foregrounding questions of identity, authority, and positionality (Hymes 1974; Myerhoff and Ruby 1982). The new ethnography also turned away from the exotic “other” to new subjects of ethnographic investigation, with anthropologists working increasingly *in* and *on* their own societies, including the contemporary West itself (Martin 1987; Traweek 1988; Luhrmann 1989; Ginsburg 1989). Integral to this turn, as Marcus and Fischer have emphasized, was a “repatriation of anthropology as cultural critique.”

The experiments in ethnographic writing have stimulated a search for creative ways to apply both the substantive results and the epistemological lessons learned from ethnography abroad to a renewal of the critical function of anthropology as it is pursued in ethnographic projects at home. (Marcus and Fischer 1986:112)

In these features, the new ethnography and theory that chronicled it opened the way, not only for new forms of ethnographic writing, but for new objects and practices of

anthropological knowledge more broadly. The significance of this work is not that it settled the crisis of representation with anthropologists turning *en masse* to experimental writing. Indeed, proponents of the new ethnography were criticized for a literary focus that “too readily collapsed the politics of ethnography into its poetics” (Abu-Lughod 1991:149). Rather, its significance lies in the broader changes it stimulated as the epistemological lessons of the crisis were brought to bear on fundamental disciplinary assumptions about “culture,” “the field,” and difference. Even those critical of its literary focus for skirting fundamental issues of domination (e.g., Abu-Lughod 1991), drew on the insights of this “experimental moment” and the possibilities it opened for re-imagining the practice of anthropology.

The works of this “experimental moment” influenced my Cyborganic research in several ways. Most visible is the choice to pursue ethnographic fieldwork in the United States, among people much like myself, in a group not bound by ethnos or received solidarities. Moreover, the renewal of anthropology’s critical function is evident in the critique of the narrative of social revolution that comprises the third objective of this dissertation. In addition to legitimating the object and critical approach of my research *as anthropology*, the new ethnography contributed to my understanding of anthropological knowledge as socially constructed and therefore: partial, intersubjective, and situated. Both the turn to reflexivity and to the wider political-historical contexts of ethnography address questions of location, or situatedness. While the former seeks to locate the ethnographer in relation to the

object of ethnographic knowledge, the latter seeks to locate both within the broader context of the world political economy. This idea of location is central to my understanding of situated knowledge, yet it draws more directly on anthropological work that followed on the “experimental moment,” than on the new ethnography itself. By turning briefly to discuss this work, I seek to round out my explanation of situated knowledge and the disciplinary context of my research.

Anthropological Locations and the Ethnography of the Everyday

The concept of location is central to situated knowledge in multiple senses, scales, and dimensions—from geographic locale and historical context, to the inner territories of identity and ontology. In this thinking, I draw on geographer Edward Soja’s concept of the “ontological nexus of space-time-being.” Soja argues that, just as the physical world is delimited in space, time, and matter, the abstract dimensions of spatiality, temporality, and social being “together comprise all facets of human existence” (1989:25). Thus, I understand situated knowledge as a process that entails locating ethnographic inquiry in each of these social dimensions, working to situate both knower and known within the ontological nexus through which they are constituted. While I have noted general sources for this approach in Haraway’s feminist objectivity and anthropology’s “experimental moment,” my understanding of what this process looks like in practice draws on Lila Abu-Lughod’s prescription for “writing against culture” (1991) and Akhil Gupta and James Ferguson’s theorization of “anthropological locations” (1997b, 1997c). These works present

examples and strategies for undertaking ethnographic research in the contemporary world that have guided me in putting situated knowledge into practice. Though their topical frames are quite different, both address questions of location in anthropology and build on their own and others' critiques of the discipline to offer new perspectives on culture, the field, and difference.

In the last twenty years, anthropologists have problematized conventional notions of culture and the field by examining difference at home (Weston 1997); working from positions that blur the self/other dichotomy, such as native, feminist and “halfie” anthropologists (Abu-Lughod 1991); studying groups not rooted in stable territories, such as homeless (Passaro 1997), migrant worker (Leonard 1997) and refugee populations (Malkki 1997, 1995a, 1995b), and investigating social and cultural processes that are not spatially localized (Martin 1995). Within this flourishing of reconceived perspectives, I single out Abu-Lughod and Gupta and Ferguson for two reasons. First, they, too, draw explicitly on feminist epistemologies and argue for increased attention to the situatedness of anthropological knowledge. Second, they describe and theorize wider trends and bodies of work in the discipline, offering examples and strategies for doing anthropology in the present.

In her essay “Writing Against Culture” (1991), Abu-Lughod maintains that the concept of culture, “especially as it functions to distinguish ‘cultures’,” has outlived its usefulness and “become something anthropologists would want to work against in their theories, their ethnographic practice, and their ethnographic writing”

(1991:138). Like many in the wake of the crisis of representation, she criticizes the idea of cultures as homogenous, coherent, bounded, and timeless. However, her approach is distinct in focusing on the problematic opposition between self and other that has been “central to the paradigm of anthropology.” Defining “halfies” as “people whose national or cultural identity is mixed by virtue of migration, overseas education, or parentage,” Abu-Lughod argues that the dilemmas shared by feminist and “halfie” anthropologists clarify the problems of the self/other dichotomy and, ultimately, call in to question “the value of the concept of culture on which it depends” (1991:137-38). In the course of her argument, she draws out the shared elements of feminist and “halfie” anthropology and explains what insight they offer the discipline as a whole; she urges anthropologists to “consider strategies for writing against culture,” and discusses three she sees as “promising” (1991:147).

Abu-Lughod begins by reflecting on what anthropology can learn from feminist theory, “an academic practice that also traffics in selves and others” (1991:139). Before anthropology faced the challenge of “ethnography without the ethnos,” feminists faced the challenge of feminism without “the woman.” That is, though “woman” was their subject, the differences encompassed in that category produced in feminist theory a crisis of subjecthood.

The crisis in feminist theory...was the problem of “difference.” For whom did feminists speak? Within the women’s movement, the objections of lesbians, African-American women, and other “women of color” that their experiences as women were different from those of white, middle-class, heterosexual women problematized the identity of women as selves. (Abu-Lughod 1991:140)

“From its experience with this crisis,” Abu-Lughod writes, “feminist theory can offer anthropology two useful reminders:” (1) “the self is always a construction, never a natural or found entity, even if it has that appearance;” and (2) “the process of creating self in opposition to an other always involves the repression or omission of other forms of difference” (1991:140). This crisis in feminism, she argues, should caution anthropologists against the dangers of assuming self and other as givens.

Both feminism and anthropology are constructed through opposition to “others.” While feminists “discover the self by becoming conscious of oppression from the Other,” anthropologists “constitute their selves in relation to an other, but do not view this other as ‘under attack’”(1991:138). Abu-Lughod criticizes those who characterize “the relationship between anthropological self and other as nonadversarial,” pointing out that the discipline is

built on the historically constructed divide between the West and the non-West...and continues to be primarily the study of the non-Western other by the Western self, even if in its new guise it seeks explicitly to give voice to the Other or to present a dialogue between self and other... And the relationship between the West and the non-West, at least since the birth of anthropology, has been constituted by Western domination. (Abu-Lughod 1991:139)

The question that feminist and halfie anthropologists bring to the fore is: “What happens when the ‘other’ that the anthropologist is studying is simultaneously constructed as, at least partially, a self?” Abu-Lughod proposes that this split selfhood creates an awareness of “three crucial issues” that are instructive for anthropology generally: positionality, audience, and the power inherent in

distinctions of self and other. Because they stand at the intersection of systems of difference, feminist and halfie anthropologists: (1) “cannot easily avoid the issue of positionality” and recognition that “every view is a view from somewhere;” (2) are accountable to multiple audiences and obliged “to confront squarely the politics and ethics of their representations;” and (3), cannot escape “the dubiousness of maintaining that relationships between self and other are innocent of power (Abu-Lughod 1991:142). Anthropology as a whole, Abu-Lughod argues, needs to be more attentive to the relations of power that feminists and halfies reveal as inherent in distinctions of self and other.

Further, Abu-Lughod proposes that the concept of culture persists in anthropology because “the distinction between self and other rests on it.” Drawing on Said’s critique of Orientalism (1978), and examples of cultural feminism, she compares use of “culture” to other “dividing practices,” arguing that “whether they naturalize differences, as in gender or race, or simply elaborate them, as...the culture concept does,” these practices “are fundamental methods of enforcing inequality.” She shows that—despite its anti-essentialist intent to remove difference “from the realm of the natural and the innate”—the concept of culture still operates in anthropological discourse to freeze difference, and “enforce separations that inevitably carry a sense of hierarchy” (1991:143, 144).

This critique of the notion of culture brings Abu-Lughod to the central argument of her essay and she writes:

If “culture,” shadowed by coherence, timelessness, and discreteness, is the prime anthropological tool for making “other,” and difference, as feminists and halfies reveal, tends to be a relationship of power, then perhaps anthropologists should consider strategies for writing against culture. (Abu-Lughod 1991:147)

In this way, Abu-Lughod brings the insights of feminists and halfies to bear on her call for all anthropologists to work against the culture concept in their theories, ethnographic practice, and writing. She devotes the remainder of her essay to a discussion of three ways anthropologists might do this by focusing on (1) discourse and practice; (2) connections; and (3) writing “ethnographies of the particular.”

First, Abu-Lughod proposes that the terms “discourse” and “practice,” which have become increasingly popular in anthropological theory, “enable us to analyze social life without presuming the degree of coherence that the culture concept has come to carry.” She uses discourse in “its Foucauldian derivation...to refuse the distinction between ideas and practices or text and world that the culture concept too readily encourages;” and draws on the practice approach associated, in anthropology, with Bourdieu (1977) and Ortner (1984), to emphasize the role of agency and contradiction “over more static homogenizing cultural tropes of rules, models, and texts” (Abu-Lughod 1991:147).

Second, Abu-Lughod advocates that anthropologists work against the isolating tendencies of the culture concept by reorienting the problems and subject matter they address “to include phenomena of connection.” She writes:

An important focus should be the various connections and interconnections, historical and contemporary, between a community

and the anthropologist working there and writing about it, not to mention the world to which he or she belongs and which enables him or her to be in that particular place studying that group. (Abu-Lughod 1991:148)

She cites the writing of anthropologists working from a world-systems perspective (Wolf 1982; Mintz 1985), and of those concerned with national and transnational flows (Appadurai 1991), as examples of anthropological practices that “expose the inadequacies of the concept of culture and the elusiveness of the entities designated by the term *cultures*” (Abu-Lughod 1991:149).

Finally, Abu-Lughod advises anthropologists to eschew generalization as a “language of power” that tends to produce “the effects of homogeneity, coherence, and timelessness,” by writing “narrative ethnographies of the particular in a continuing tradition of fieldwork-based writing.” Looking closely at the lives and relationships of particular individuals, and the dependent paths by which social life proceeds, she argues, can work to “subvert the most problematic connotations of culture.” (Abu-Lughod 1991:152-54)

Each of the three strategies Abu-Lughod proposes has informed the conception of my Cyborganic research and guided me in putting situated knowledge into practice. As a community, Cyborganic is defined by the discourses and practices that brought it into being, rather than by an “ethnos.” This understanding is reflected in my research and writing by attention to everyday practices, such as the use of mailing lists, and the discourses of which they were a part. Further, I have sought in my ethnography to situate the Cyborganic project in time and place, emphasizing its

connections to the broader cultural history of the Bay Area as a “technopole” (chapter 3), while also producing a detailed account of the individuals and events through which the project was realized (chapters 4, 5, 6). As Abu-Lughod makes clear, the argument for particularity does not imply a disregard for forces and structures that are not locally based. Rather, it entails the recognition that “extralocal and long-term processes are only manifested locally and specifically, produced in the actions of individuals living their particular lives, inscribed in their bodies and their words” (Abu-Lughod 1991:150). As this discussion illustrates, the strategies Abu-Lughod proposes for “writing against culture” resonate deeply in my approach to situated knowledge. Both entail that anthropologists locate themselves in relation to their objects of knowledge—the people, practices, or discourses they study and represent. Both focus on agency and the social uses of discourses and practices. Both emphasize that attention to *micro* processes and connections of everyday life be complemented by attention to the *macro* processes and connections of history, political economy, and power. In this way, both work toward situating anthropological knowledge in Soja’s ontological nexus of space-time-being.

As noted, Gupta and Ferguson’s work on “anthropological locations” is a second source for my application of situated knowledge to anthropology. Though they focus critical attention on the spatialization of difference, rather than the self/other dichotomy, Gupta and Ferguson also propose revised strategies for practicing anthropology in the contemporary world. Their critique centers on the

“assumed isomorphism of space, place, and culture” that has dominated in anthropology; and on the challenges posed to this assumption by transnational flows of migrants, refugees, and cosmopolitan elites; by recognition of cultural differences within localities (multiculturalism, subcultures); and by “the hybrid cultures of post-coloniality” (1997b:34-35). They contend that assumed and naturalized divisions of space, such as “societies,” “nations,” “cultures,” “tribes,” and “peoples,” and “their” associated territories, persist even in contemporary anthropological practice. They criticize “articulation models” of the world economy that posit a primeval “precapitalist” local scene that is then “violated by global capitalism” for assuming the autonomy of this imagined pre-contact state rather than investigating how the local community was formed as a community “out of the interconnected space that always already existed” (1997a:36). Echoing Abu-Lughod, Gupta and Ferguson criticize the new ethnography for its unselfconscious assumption of “others” and cultural difference; and reproach Marcus and Fischer (1986) and the anthropology as cultural critique they championed for “spatializing cultural difference with ethnography as an unproblematized link between ‘home’ and ‘abroad’” (1997a:43). Gupta and Ferguson observe that the dialogical conception of much of the new ethnography assumes “others” and “other cultures” as givens, taking cultural difference as a starting point, rather than an end product of historical processes that differentiate the world as they connect it. It is precisely because the anthropological object can no longer be “conceived as automatically and naturally anchored in

space,” they argue, that anthropology “will need to pay particular attention to the way spaces and places are made, imagined, contested, and enforced” (1997a:47).

Gupta and Ferguson’s approach centers on “the idea of ‘location’ that has developed in feminist scholarship” (1997c:35). Recognizing that a well-developed sense of location has always been the great strength of ethnography, they argue that this strength becomes a liability “when notions of ‘here’ and ‘elsewhere’ are assumed to be features of geography, rather than sites constructed in fields of unequal power relations” (1997c:35). They therefore propose a new understanding of location as actively constructed and situated within a field of interconnected relations, structures, and histories. Here they draw on Haraway’s theorization of situated knowledges (1988) and cite their debt to the feminist conception of ‘location,’ not as *ascribed* (e.g., sex, race, parentage), but as “something one strategically *works* at,” writing:

We are not advocating the abandonment of the practice of fieldwork, but rather its reconstruction—decentering “the field” as the one, privileged site of anthropological knowledge, then recovering it as one element in a multistranded methodology for the construction of what Donna Haraway (1988) has called “situated knowledges.” We might emerge from such a move with less of a sense of “the field” (in the “among the so-and-so” sense) and more of a sense of a mode of study that cares about, and pays attention to, the interlocking of multiple social-political sites and locations.... But a heightened sense of location means most of all a recognition that the topics we study and the methods we employ are inextricably bound up with political practice. (Gupta and Ferguson 1997c:37-38)

In this manner, Gupta and Ferguson propose a more historical and structural understanding of all locations, including the anthropologist’s, as *constructed*, rather

than given; *connected* historically and politically, rather than isolated; and *shifting*, rather than bounded. They emphasize both the constructedness of knowledge and the situatedness of positions from which it is produced. Arguing that traditional conventions of “the field” entailed their own forms of political engagement, Gupta and Ferguson propose an updated view of “anthropological knowledge as a form of situated intervention” (1997c:38).

Gupta and Ferguson’s work also draws attention to “revitalized forms of fieldwork...already well under way in anthropological practice” (1997c:37). In doing so, it descends from theory of the field to practical questions of what and how to study. The discipline’s fieldwork tradition, they contend, has been valuable as a counter to Western ethnocentrism for its focus on everyday, embodied, and informal knowledge, and for its method of self-conscious shifting of social and geographical location. The essays collected in Gupta and Ferguson’s two edited volumes addressed to place-making in anthropology⁴ demonstrate research practices that work to preserve the traditional strengths of fieldwork, while at the same time adapting those strengths to the reconfigured contexts of contemporary anthropology. My own research draws on this work in several ways. Besides supporting my application of Haraway’s situated knowledge to ethnographic fieldwork, it has guided my focus, methodologically, to the study of everyday practices and informal knowledge. My

⁴ *Anthropological Locations: Boundaries and Grounds of a Field Science* (Gupta and Ferguson, eds., 1997d) and *Culture, Power, Place: Explorations in Critical Anthropology* (Gupta and Ferguson, eds., 1997e).

attention to domestic life, work processes, mailing lists, and voluntary association bears out that influence. Moreover, I took Gupta and Ferguson's view of "a research area"—"less as a 'field' for the collection of data, than as a site for strategic intervention" (1997c:39)—as authorization for the highly participatory methods of my fieldwork. Because there are no locations outside fields of unequal power relations, no knowledge is disinterested or apolitical. Thus, rather than striving for distance, anthropologists are called upon to situate themselves in relation to their subjects and reflect on their positionality, whatever the extent of their participation or connection.

Objectivity Revisited: Subjectivity and the Native Question

If distance has certain arguable advantages, so too does closeness, and both have their deficits. Yet classic social science has endowed the former with excessive virtues, and the latter with excessive vice... The present chapter contests the equation of analytical distance and scientific objectivity by arguing that social analysts should explore their subjects from a number of positions...

Renato Rosaldo (1989:169)

Implicit in the case for situated knowledge presented in this chapter is an argument against "the equation of analytical distance and scientific objectivity" (Rosaldo 1989:170). This argument is especially important for my research of Cyborganic given my closeness to and participation in the community. From the start of my fieldwork, my informants knew I was an anthropologist researching networked communities and this identity was central to my position in the Cyborganic project and community. While my roles as observer/researcher and

participant/member were openly interconnected—and fed back into one another in productive ways—the combination of subject positions entailed in fieldwork and ethnography is never simple or unproblematic. The tensions inherent in the fieldworker’s double persona as participant-observer are, however, a valuable source of ethnographic knowledge as many anthropologists have contended. Dorinne Kondo (1986), Renato Rosaldo (1993), and Lanita Jacobs-Huey (2002) have all examined the trade-offs entailed in identification as a source of anthropological knowledge and, in doing so, all make the case for epistemological reorientations away from the conventions of distanced objectivity towards what I have called situated knowledge.

Kondo (1986) and Rosaldo (1993) argue for the value of more experiential, participatory, and affective modes of knowing that involve, as Kondo has put it, “the whole self” (Kondo 1986:75, 85). Drawing on anecdotes from the works of Geertz (1968), Briggs (1970), and Fanon (1967), Rosaldo critiques the Weberian ethic of “passionate detachment” for restricting too severely “the legitimate sources of knowledge for social analysis;” and excluding insights from such “lesser” sources as the ethnographer’s feelings of “feebleness,” “depression,” and “rage” (1993:193).

Kondo makes a similar case against distanced objectivity, writing:

We must recognize that our emotions and sympathies are inevitably implicated in our foreunderstandings. These too can be legitimately productive of knowledge, for knowledge is not purely cognitive. It is also the product of our emotional sensibilities and affinities. I am not suggesting that anthropologists attempt a Romantic fusion with the Other, for this is not only impossible but merely perpetuates the two poles of our own conceptual oppositions: tight-lipped reason and *Sturm und Drang* emotion, objectivity and subjectivity, mind and

body. What I am suggesting is that knowing involves the whole self (at least as we define it), and not simply what we think of as “the intellect.” Accordingly, moments of identification, as well as moments of distancing, may occur during all phases of knowing, from the definition of the problem, to the experiences in the field, to the writing of the ethnographic text. A more honest appraisal of the anthropological enterprise would take these other elements—so often treated as illegitimate, unscholarly, “soft”—as integral to the process of understanding. (Kondo 1986:85)

For Rosaldo and Kondo, the ethnographer’s position as a situated subject is not simply a limit on knowledge, or barrier to objectivity, but is itself “integral to the process of understanding.” The ethnographer’s cognitive and visceral moments of identification and distancing are, they argue, crucial sources of ethnographic knowledge. Thus, they assert, rather than imprisoning anthropology in contingency, recognizing “the role of experience, power, and of the involvement of the entire self” (Kondo 1986:86) results in richer, more accurate understandings, or in Haraway’s phrase, in “better accounts of the world.”

Kondo (1986), Rosaldo (1993), and Jacobs-Huey (2002) all argue that the ethnographer’s degree of distance from his or her informants is never simply a neutral fact. That is, one’s position inside or outside the culture being studied can never be unreflectively taken for granted because: (1) it is always negotiated in the ethnographic encounter; (2) it invariably encompasses multiple positionings that can move along a continuum from complete identification to total alienation during fieldwork; and (3) it shifts as the ethnographer moves from the more participatory role of fieldworker to the more observational, distanced roles of social analyst and

writer. The fieldworker's double persona as participant-observer is matched by the ethnographer's dual identity as both fieldworker and writer. Critical reflection about the way these multiple positionalities are negotiated is a valuable source of ethnographic knowledge.

Kondo (1986), Rosaldo (1993), and Jacobs-Huey (2002) each make this case but do so for ethnographers working from different subject positions. For the most part, Rosaldo examines the conventional case of ethnographers working among people who are not "their own," while Kondo focuses on her experience as an anthropologist "not completely outside the culture" she is studying; and Jacobs-Huey reflects on the problematics of "native" anthropologists working in "their own" communities. I reference the three anthropologists jointly because together they demonstrate that identification and distancing are integral to ethnography regardless of the ethnographer's position as "outsider" or "native" to the studied culture. Indeed, they show that ethnographers are never simply inside or outside the cultural groups they study; their subject positionings are more complex, shifting, and negotiated; and these negotiations play a vital role in the ethnographic enterprise.

The issues of identification and distance these anthropologists bring to the fore have been a crucial resource for me in addressing questions about my position as the ethnographer of a community in which I was a founding member and active participant for almost ten years. Though these issues remained in the background during fieldwork, they arose upon my return from the field to the university where I

encountered the question of whether researching Cyborganic made me a “native” anthropologist. Given my membership in the community, and extent of my fieldwork, the case could be made for my native status. Yet I had never thought of myself in those terms and was initially quite resistant to identify in that way. My research had brought me to Cyborganic and I clung to my identity as participant-observer as though it were a talisman against accusations of having “gone native.” Yes, I had lived among the Web geeks for many years, I conceded, and they considered me one of their own, but, for my part, I had always been a participant-observer. Though I made no secret of my membership in Cyborganic, I certainly did not want to “play the native card” in an uncritical privileging of “insider” status. However, I came to see that I was already invoking insider status by framing my research in terms of the “repatriation of anthropology as cultural critique” that Marcus and Fischer (1986) proposed. Puzzling over the contradictions of my subject position led me to Kondo’s, Rosaldo’s, and Jacob-Huey’s works. These, in turn, allowed me to face the question of my insider status without approaching it as an either/or proposition. That is, they let me see that I might safely consider my identity as a Cyborganic member, without ceding legitimacy as ethnographer, or promulgating an uncritical notion of the “native” anthropologist.

Because they focus on ethnographers for whom the “Other is not totally Other” (Kondo 1986:75, 83), Kondo’s and Jacob-Huey’s essays have been particularly useful to me in understanding my position as the ethnographer of a

community in which I was a member. Though Kondo writes as a Japanese-American anthropologist working in Japan, and Jacobs-Huey as an African-American anthropologist working in the United States, the issues of identification, distancing, and accountability they raise parallel and illuminate my own experiences of fieldwork and writing. Their analyses led me to reflect on my position inside and outside Cyborganic, first, as a negotiation with my informants that changed with time and context, and, second, as integral to the research and writing process.

In many ways, Kondo's experience and "ambiguous insider/outsider position in the field" parallel my own (1986:82). Just as her informants worked to make her over in their image and guide her toward cultural competence, my Cyborganic informants evangelized the virtues of Internet culture and, particularly during my early days in the field, worked to school me in its ways. Kondo emphasizes both her own and her informants' agency in the "collusion" to incorporate her in a variety of meaningful cultural roles, from daughter, guest, student, and young woman, to "prodigal Japanese who had finally come home" (1986:77).

During the fieldwork experience itself, my informants often tried to recreate me as Japanese. I collaborated in this attempted recreation with various degrees of enthusiasm and resistance. The play of identities was constant in the field, changing with time and with context. (Kondo 1986:82)

She writes of being "forced to abandon the position of observer" by expectations that she would behave as a Japanese, and of feeling "bound by chains of obligation" to her informants. Finally, Kondo observes, fieldwork culminates in writing the

ethnographic text, a process that requires distance and entails “drawing away from the immediacy of the ethnographic encounter.” Though she emphasizes that her participation and “openness to Otherness” were “crucial determinants” of the field experience and the task of defining her research problem, Kondo asserts that “some degree of remove from the Other was necessary in order to recover meaning from” her experience of identification. (Kondo 1986:79-84)

Jacobs-Huey also emphasizes the way “participants and researchers co-construct the native researchers’ identities, roles, and research agendas in overt and subtle ways” (2002:796). She synthesizes the work of several native scholars to “expose the fallacy of presuming commonalities with research participants based on their shared ethnic, gendered, and class backgrounds,” arguing that all scholars, “particularly native ones, must diligently strive to negotiate legitimacy in the field” (Jacobs-Huey 2002:793).

The process whereby native scholars are attributed particular social roles—along with their subsequent attempts to comply with or contest these positionalities—illuminates how *native/insider* is an insufficient descriptor for the manner in which scholars negotiate multiple identities in the field. (Jacobs-Huey 2002:794)

Jacobs-Huey’s analysis attends to the multiplicity and tensions hidden in the assumption of native/insider status. She also draws attention to the dilemmas native anthropologists face in reconciling “multiple allegiances and accountabilities to their ethnic and academic communities;” and in writing for both lay and academic audiences. While recognizing that these tensions are, to some extent, shared by all

social scientists, Jacobs-Huey illustrates how “managing the politics of representation may entail additional challenges for native scholars” (2002:797).

Like Kondo, Jacobs-Huey examines the role of identification and distance both in fieldwork and in representing knowledge in the ethnographic text. These examinations made me attentive to the ways in which my subject position as ethnographer changed over the course of research. Kondo’s and Jacobs-Huey’s examination of the tensions and value of identification as a source of ethnographic knowledge enabled me to confront the questions of insider identity analytically. They equipped me with ways to think and talk about the multiple, negotiated, and fluid nature of my positionality; and drew my attention to the particular challenges of audience and accountability faced by ethnographers who are not completely outside the communities they study. Through their analyses, I came to see that the issue to address is not whether the anthropologist is a “native” for this settles nothing and simply opens the question of “How native is a native anthropologist?” (Jacobs-Huey 2002:792). Instead, the question to consider is what “insider” and “outsider” roles and identities constituted my subject position vis-à-vis the Cyborganic community.

Insider Roles and Identities from Fieldwork to Writing

As I have argued above, following Kondo (1986), fieldwork entails multiple positions that can move along a continuum from complete identification to total alienation and that tend to shift during the course of research. Therefore, in considering my positionality as ethnographer of Cyborganic, I begin by describing

my insider roles and identities before, during, and after fieldwork; then shift to consider my positioning as outsider over these same three stages of research and writing. Figure 2.1 below gives a schematic representation of the roles and identities that constituted me as a Cyborganic member.

<i>Before Fieldwork</i>	<i>During Fieldwork</i>	<i>After Fieldwork</i>
1	Friend, Age-cohort Member (“Generation X”), College-educated European-American with Counter-cultural Identities and Attitudes	
2	Computer User, BBS User, Film Sound Technician (“Audio Geek”)	
3	Cyborganic Community Member (1993-present)	
4	Neighbor/Housemate (Ramona Empire resident 17 months)	
5	TND Organizer & Host (16 months)	
6	Co-worker, Partner, Employee: Principle in Cyborganic Business, Community & Education Director, Mailing List Administrator (28 months)	
7	Co-worker and Colleague (Web Industry Professional 1996-2002)	
8		Community Historian

Figure 2.1: Insider roles and identities of the ethnographer

As described at the start of this chapter, I entered the field more as friend than as stranger (Powdermaker 1966). I knew Cyborganic founder, Jonathan Steuer, from college and through collaboration with him was introduced to the group household on Ramona Avenue, and to the wider community of Internet geeks in which I lived and worked for about six years. Even before I moved to San Francisco to undertake

fieldwork I was connected to the people who formed Cyborganic by common identities (ascribed and achieved), bonds, experiences, interests, and sensibilities. These pre-existing commonalities are listed in the first two rows of Figure 2.1 above. Like most of my informants, I was in my twenties, had grown up with video games and personal computers (PCs), and identified with the artsy and counter-cultural. Most of us had recently finished college, or graduate programs, and had chosen San Francisco as the place to embark on post-school lives and careers. In this regard, Cyborganic was a peer-cohort of middle-class Americans at a particular life stage, negotiating new roles and identities as full-time professionals living independently of school and family.

A further source of identity stemmed from the fact that I had used a PC for about 10 years when my fieldwork began; was familiar with bulletin-board systems (BBSes) and commercial online services; and had technical training and work experience in film sound and digital multimedia (Figure 2.1, line 2). In the course of this experience, I developed a number of practical, linguistic, and cultural competencies that enabled me to speak, act, and be seen as a geek or “techie,” a person capable and interested in technical matters. Initially, I identified more as an “audio geek” because I had only started to use the Internet nine months before entering the field and felt like a “newbie” in relation to anyone who had been “on the Net” longer. Nevertheless, a common do-it-yourself orientation to technology paved my way in the field. This identity and orientation equipped me with habits (practices

and imaginaries) of self-directed learning in a context where such learning was a central feature of social life. Like many of my informants, I gained knowledge, skills, and contacts through my involvement with Cyborganic that enabled me to earn my living in the emerging Web industry.

Together these commonalities of generation and orientation to technology drew me to my Cyborganic research, shaping not only what I studied, but also when and how. At the time I decided to go into the field in August 1993, I had just finished my Masters in Visual Anthropology and been admitted to a Ph.D. program in Social Anthropology. Rather than start my study by writing a research proposal and seeking funding for fieldwork, as is customary, I decided to take leave from my university and immediately begin an extended period of participant-observation fieldwork. The plan was to live and work among the people and social phenomena I sought to investigate; supporting myself and my research through the research itself, doing the same kind of work my informants did to support themselves. Several factors informed my decision to begin fieldwork right away and undertake it in this manner. First, it seemed clear from the groundswell of activity around new media technologies in the Bay Area that something significant was taking place with great momentum. To delay, even for a semester, seemed an unimaginable setback. Second, I was influenced by the do-it-yourself ethos and example of my San Francisco informants to take this entrepreneurial approach, bootstrapping my research project with my own wages, rather than applying and waiting for institutional support. There

were jobs to be had in the Bay Area and people with computer and Internet experience could earn significantly more than graduate researchers and still have time to pursue their own creative projects. This was the approach Steuer was taking with his Cyborganic project, and that others had taken in creating salons and innovative communities in the Bay Area. It seemed only natural for me to bootstrap my own research in the same way. In so doing, it might be said that I “went native” by choosing a direct, independent, market-supported means to accomplish my project, rather than a more institutional approach. Both the form and content of my research served as key sources of identity with my informants and enabled me to undertake fieldwork as a member of their newly forming community.

Once my fieldwork was underway, I developed a number of other insider roles and identities as one of Cyborganic’s founding members (Figure 2.1, lines 3-6). After I got to the Bay Area and secured paid employment, I began to work with Steuer on the Cyborganic project, initially as a volunteer, later as a contractor and employee. A few months after I arrived in the field, the apartment next door to the group household on Ramona Avenue opened up and I moved there with two other new Cyborganics. Within a year, I began hosting weekly potluck dinners (TND) that were key to expanding Cyborganic and demonstrating its central premise that online and face-to-face interaction are mutually sustaining and together build uniquely robust communities. In addition, I took on a variety of roles within Cyborganic, from bookkeeping and mailing list management to the writing of business plans,

manifestos, and community guidelines. For almost two years, my personal and professional life revolved around Cyborganic and during the seventeen months I lived on Ramona Avenue there was little separation between the two. During the most intense period of fieldwork, I lived in the apartments at 65/67 Ramona where Cyborganic was headquartered and also worked there as a full-time employee of the Cyborganic business. At that time, the project regularly occupied most of my waking hours and was the ever-present context of my sleeping ones.⁵ Both space and time in the apartments were organized around the business. Work and live space were integrated: residents and staff shared the kitchen, bedrooms doubled as offices, meals doubled as meetings, and community dinners spilled into the entire house. Project meetings were held evenings and weekends to accommodate staff and volunteers who had other “day jobs” and were working on Cyborganic on their own time.

Many anthropologists have written of the collapse of personal boundaries that fieldwork can entail (Briggs 1970; Kondo 1986) and the constant demands of participant-observation. What I point to specifically in this case is that I, the ethnographer, was hardly alone in co-mingling work and the rest of life this way. Home offices, 60- to 80-hour workweeks, and using the wages from one job to underwrite one’s own creative projects, were familiar phenomena for most

⁵ The following fieldnote illustrates my point: “This morning, at about eight o’clock, my boss and housemate (Steuer) stormed into my bedroom [and office] with the company check book, woke me up, and asked to see [Cyborganic’s] books and bank statements. Good thing I sleep with both nightware and computer on” (Cool, fieldnote, March 23, 1994).

Cyborganics during the dot-com boom. Living and working in this way was, thus, a source of common experience and identity with my informants—as was “burning out” from it, and learning to recover. The period of fieldwork in which I lived, worked, and socialized on Ramona Avenue involved the greatest collapse of personal boundaries. In mid-1995, after about a year and a half in the group households on Ramona, I moved across town to an apartment of my own, but continued to work for Cyborganic. This move marks the start of a second phase in my fieldwork in which I distanced myself from the business project: first by moving off Ramona and then, six months later, by quitting my job at Cyborganic altogether.

From this new vantage, I saw that my experience of Cyborganic had thus far been the exception, not the rule. Most members neither lived on Ramona, nor worked for the Cyborganic business. In my second phase of fieldwork, I joined the ranks of this majority. For the next three and a half years, I worked in the Bay Area Web industry and participated in Cyborganic’s online forums and community, without involvement in the business. I also broadened the scope of my research, studying Cyborganic and other community networks at the Institute for the Future; and returning to graduate school for a year. My position outside the Cyborganic business during this stage of fieldwork brought my experience closer to that of my informants’, making me more of a community insider in the “regular Joe,” rather than “inner circle,” sense. It gave me the opportunity to work at a number of different Internet companies—large ones, such as Netscape, as well as small Web-

shops run by other Cyborganics—and to develop co-worker and colleague relationships, and friendships, more broadly within the community and wider Web industry. While I'd previously associated with other Cyborganics primarily in the community's own forums, we now interacted regularly in contexts outside our home territory: at corporate workplaces and events, and in the company of people who were not members of the group. I saw how others spoke of Cyborganic, and presented themselves *as Cyborganics* outside the group and gained insight from similar self-presentations of my own. I retained my identity as Cyborganic's community maven (and TND founder) during this period of fieldwork; and even strengthened it in some ways. One was that I had more time for recreation in the community than I had had when employed by Cyborganic. Another was that I continued to participate in public discussions of Cyborganic's community, policies, and ideals, where my knowledge of group history and Internet communities served to bolster that reputation. I continued to be identified with Cyborganic's communitarian aims even after I disengaged from any official role in the project, and this dovetailed well with my identity as the community's resident ethnographer, an outsider position.

When I left the Bay Area in June 1999, I thought I was bringing my fieldwork to a close. By that time the Cyborganic community was dispersed and its activity much attenuated. The weekly dinners had stopped in the spring of 1997 and by the end of that year the Cyborganic business had closed its doors and website.

The community mailing list remained active, as did a small server cooperative that provided electronic mail and Web hosting, mostly *pro bono*, to dozens of organizations and about a hundred individuals, myself included. A number of people on the mailing list no longer lived in the Bay Area, and those who did no longer met face-to-face as Cyborganics on any regular basis. By mid-1999 most members had found or formed new associations, cooperatives, and identities in place of those they'd had in Cyborganic. Thus, even before I left the Bay Area, Cyborganic had become more distributed, less close-knit, as a community. Individual members still met in person with varying frequency at work and play, but the group's only public presence was on the community-wide mailing list. Though the list continued to operate until the end of 2002, it was no longer a focal point of everyday life the way Cyborganic had been in the past. For all these reasons, I looked upon my fieldwork as over when I returned to Los Angeles, where I completed my doctoral work.

In hindsight, however, I see that, though I left my field site simply by moving away from the Bay Area, this move did not, in any sense, constitute leaving the community. When Cyborganic started, it was emphatically a place-based community, but it had changed. Cyborganic had changed, but it had not disappeared: it had reconfigured and, though I did not recognize it at the time, my field site had done the same. It was no longer place-based and, thus, leaving the Bay Area did not change my experience as a Cyborganic member as much as I supposed it would. I continued to be part of the server cooperative, and to read and post to the mailing list

and, in these ways, continued participant-observation long after I thought I'd left the field. Reconfigured, both Cyborganic and the field remained with me. I subsequently came to see this period as a liminal one in which my fieldwork proper had ended, but interaction with my informants continued in the course of everyday life, even as I turned to writing up my research. In the process of organizing, transcribing, sifting-through my data, and writing, I developed new relationships with key informants who helped track down sources and supplementary data, and were regularly willing to discuss ideas and questions. From time to time, I also talked about my work on the mailing list, sharing amusing facts or anecdotes from my research, and answering questions about the group's history. In these interactions, I felt I was simply continuing as resident ethnographer, a role I had played since my arrival in the field.

However, as time went on, Cyborganic's largest, most active period as a community receded further into the past and traffic on the mailing list tapered off, stopping altogether at the end of 2002. At this point, my role as Cyborganic ethnographer started to become the source of a new insider identity: community historian. I did not become conscious of this identity until I returned to graduate school full-time and began working to analyze my field research. In discussions, interviews, and e-mail surveys during this period (2004-2007), nearly all my informants expressed that they were happy someone was recording what they saw as an important period in the growth of the Internet, one rapidly disappearing from view in the wake of mass popularization and constant change. From their perspective, my

work had new value as history, or perhaps, salvage anthropology of a bygone era when the Web was new. Given the length of my fieldwork and pace of change in and around Cyborganic, my informants came to see my role as ethnographer as that of recording the community's history. James Home, a key informant, first made me conscious of this shift in a 2004 interview. As we sat down to talk, he said he was going to share stories he used to think he would one day write himself because he wanted them told and wanted Cyborganic's legacy to be remembered⁶. On hearing this, I was struck by a deep feeling of inclusion linking Home and me, not as individuals, but as fellow Cyborganics. Of course, I was grateful to him personally for sharing his stories, but even more profound were the sense of solidarity in working together to get "the Cyborganic story told;" the honor of being entrusted with its telling; and the humbling, somewhat daunting, sense of responsibility that quickly follows on such honors.

Once Home made me conscious of it, I noticed other informants also looking to me to preserve the history of Cyborganic and early days of the Web. While I saw research and writing about Cyborganic as roles associated with my (outsider) identity as ethnographer, my informants saw these same activities in a different frame. In doing so, they were incorporating me into a new role within the community in much the same way Kondo described her informants had done "by placing [her] in meaningful cultural roles" (1986:77). By casting me as the community historian, my

⁶ Home, interview, September 24, 2004.

informants drew me in to a new insider position, illustrating the way ethnographic subjects can co-construct the researcher's identities and roles both during and after fieldwork (Jacobs-Huey 2002:796). Though my outsider identity as ethnographer and insider identity as community historian were different framings of the same interactions, the two entailed very different positionalities. Their difference was muted when I was in the liminal period of beginning to write from my research, but still engaged in participant-observation. However, as I moved deeper into the writing process, the distinction became more significant. Recognizing and naming the historian role made me conscious that other Cyborganic members would be reading my ethnography. It brought into focus what Jacobs-Huey has written of as "the dilemmas of translation that native scholars may experience while negotiating accountability to multiple audiences—which are often inclusive of the academy and the communities in which they work" (2002:797).

Decisions about representation, including whose, and which, voice(s) to incorporate in published reports, entail "cultural brokering," that is, reconciling disparate views about *how* and *to whom* one should represent the intricacies of everyday life among individuals within a community. Although this is a challenge that is, to some extent, shared by all social scientists (see D'Amico-Samuels 1997; Duranti 1997), managing the politics of representation may entail additional challenges for native scholars. For example, native researchers must be especially sensitive to the dangers of disclosing cultural secrets or airing what community members may consider "dirty laundry" (Behar 1993, 1995; Nakhleh 1979; Visweswaran 1994; Whitehead 1986, 1992). Given native scholars' presumed communal ties, negative perceptions and consequences of such admissions may be more acutely felt by native researchers and their participants; further, missteps may make it more difficult to return home. Native scholars who accommodate publication or manuscript requests by their study

participants must also be mindful of the accessibility of their rhetorical strategies—if published reports are so technical as to be impenetrable, lay readers may suspect the ethnographer of being evasive or elitist. (Jacobs-Huey 2002:797-798)

In the case of my ethnography, the most salient of these challenges was the need to write for multiple audiences that included members of both my Cyborganic and academic communities. Given the cultural value placed on open sharing of information among Cyborganics, it was not a question of accommodating “manuscript requests” from my informants, but rather an expectation that my work would be made available to the community, either on the Web or via electronic mail, when it was complete. Completing the work thus entailed negotiating insider (community historian) and outsider (ethnographer) positions in choices of language, voice, and how to represent myself in the narrative. For example, I worked to make my academic language penetrable to the Cyborganics and other Web geeks who might read it, and to make discussions of the technicalities of networked computers accessible to academic readers across the disciplines. In this ethnography, the cultural brokering goes several ways. The descriptions above of identities and roles that position me inside Cyborganic relate only to one direction of translation. Thus, I turn in the following section to those that position me outside the community.

Outsider Identities and Roles: From Participant-Observer to Ethnographer

While it is accurate to see me, the ethnographer, as an insider in the community I studied, that representation is incomplete. I was also an *outsider* in a number of anthropologically significant ways. Figure 2.2 below charts subject positions I occupied throughout the research process that situate me outside Cyborganic.

<i>Before</i> Fieldwork	<i>During</i> Fieldwork	<i>After</i> Fieldwork
1	Born and raised outside the U.S. (age 0-15), Half-American/Half-Australian Second-generation Anthropologist Third-generation Expatriate	
2	Social Anthropologist	
3	Field Researcher, Participant-Observer (1993-2003)	Ethnographer (2004-2008)

Figure 2.2: Outsider roles and identities of the ethnographer

Broadly speaking, the roles and identities that separate me from my informants are of two types: (1) those formed through personal life history prior to fieldwork; and (2) those formed through training and practice as a social anthropologist, before, during, and after fieldwork.

Unlike the vast majority of Cyborganic members, I was born and raised outside the U.S. My father, an American, social anthropologist working in

development⁷, spent his career abroad and I grew up in Nepal, India, Pakistan, and the Philippines before moving to the U.S. at the age of 15. To add to the mix, my mother is an Australian who was born and raised in British Malaya, the daughter of Colonial civil servants. Growing up, my brothers and I went to American and International Schools where instruction was in English but the students were from all parts of the world. We carried U.S. passports, and were part of the American expatriate community wherever we lived, but did not travel back to family or a hometown in “the States” on the annual “home leave” provided by my father’s employers. By the time I was born, my father’s parents had died, so we visited my maternal grandmother and relatives in Australia during summer holidays, or traveled as tourists in Europe, Asia, and the U.S. Though my mother became a U.S. citizen in the 1950s, she never fully assimilated and to this day, like the stereotypical immigrant mother, laments her children’s American ways whenever we deviate from her expectations of proper behavior.

This cross-cultural background constituted me as an outsider in American society in a number of ways, two of which have significance for the present research. One is that it led me to follow my father in becoming a social anthropologist; and the other is that it prevented my exposure to most of the American popular culture and media my college and Cyborganic peers were raised on. When I first entered college

⁷ He worked, first for the U.S. Agency for International Development (USAID), and later for the Ford Foundation, Agricultural Development Foundation, and Winrock International.

in the U.S., I planned to study biochemistry. However, by the end of my freshman year—after a rough experience with culture shock and a good experience with a medical anthropology class—I declared anthropology as my major because it, at least, was familiar territory. Though I passed for and was supposed to be an American, I certainly didn't feel like a native. Studying the culture anthropologically seemed like it would be a good idea. By the time I was a graduate student studying homeownership in America, I joked that I was a “native anthropologist” because I had been born into the discipline and felt as if I had no other home. While classmates who did not know my background might think “native anthropologist” referred to my doing research in my own society, those who knew it got the pun. This ironic self-identification reflects my liminal insider/outsider identification with the U.S. and highlights my decision to undertake ethnographic fieldwork there as a way to resolve that liminality by making a virtue of it. In a sense, becoming a social anthropologist allowed me to hide this personal dual identity in plain sight professionally, as the double persona of a participant-observer.

One reason I felt like a cultural outsider when I arrived in the U.S. is that I had had relatively little exposure to American popular culture and media. As a child, I remember my father listened to Voice of America and the BBC on his shortwave radio; brought home magazines (*The Economist*, *Time*, *Newsweek*) and newspapers (*International Herald Tribune*); and we went out to the occasional Hollywood movie (usually a classic) at the American Embassy compound. But that was about the

extent of my media exposure. When I was in eighth grade, my family moved to the Philippine capital, Manila, where theaters showed recent American movies, television broadcast a few American shows each day, and radio stations played U.S. Top 40 hits. Though, I had seen all these media before during family vacations, seeing them once a year and having regular access to them are rather different experiences. As most teenagers would have, I began learning from school friends how to engage and consume this media. Yet, television, movies, and entertaining diversions in general were not part of my family's routine. Thus, it was not until I came to the U.S. that I became aware how deeply such media were integrated into the everyday lives of my peers there. Habits of consumption such as tuning into a favorite television show each week, record shopping, reading local weeklies for event or film listings and reviews, all were foreign to me. Though I did not think of it in these terms at the time, it was not simply that I lacked knowledge of popular culture (sports, entertainment trivia, commercials, etc.), but that I lacked a sense of being part of a popular public (Warner 2002). I also lacked basic skills of consumption (how to tune in, find event information, etc.), as I learned from observing college friends. The significance of this outsider position to the present study is that it made me viscerally aware of my peers' habits of media use, and imaginaries of popular culture, as *learned*; and as things I would have to learn, if I wanted to avoid being taken for a bumpkin. I was keenly attentive to them in my efforts to fit in with my cohort of college friends. They were visible to me as cultural norms, rather than normal life; and this visibility proved immensely valuable to me

in studying the production and consumption of networked social media in the Cyborganic community.

In addition to life history, my work as a social anthropologist also positioned me in various outsider roles and identities in relation to my research subjects. Though the Cyborganics I lived and worked among knew I was engaged in anthropological research, to the outside observer, my activities as a field researcher were largely indistinguishable from my work on the Cyborganic project. During the first phase of fieldwork, that work entailed researching online services, taking notes, writing minutes and reports, making contacts, and coordinating volunteers. Even when my research activities were foregrounded, for instance, in the formal interviews and group history elicitation undertaken for the IFTF project on telecommunities, they were assimilated with my insider role as Cyborganic's community person. This took place in much the same way that my post-fieldwork role as ethnographer came to be framed as that of community historian. The idea of someone studying the community and writing about it was familiar to my subjects and only became more so as growing interest in the Internet regularly brought reporters and news crews to Cyborganic's weekly dinners. Thus, my sense of researcher as an outsider status stemmed primarily from my own self-understandings and identification as an anthropologist. These led me, for example, not to participate anonymously (or under a pseudonym) in online forums, though that was common practice; and not to take any work that required signing a non-disclosure agreement.

These, however, were private, individual choices of which few of my informants were aware.

While in the field, it seemed felicitous that my work as a participant-observer was readily assimilated with roles that were culturally meaningful to my informants. After my fieldwork, however, this very assimilation posed a challenge. My training in anthropology had prepared me to expect difficulties entering the field, but in the case of my Cyborganic research, it was exiting that proved most challenging. One challenge, as I have discussed, was that my field site followed me home, in a sense, and my participation in the community continued online after I left the Bay Area. In this context, my process of writing up was compromised by continued interaction that kept me from “drawing away from the immediacy of the ethnographic encounter” (Kondo 1986:82). Leaving the field after such a protracted period proved exceedingly difficult, particularly because it entailed replacing a dense field of social relations and collaborative activity with the solitary tasks of writing up. During fieldwork, I had come to regard my computer, not simply as a place to work, but also as a social portal. With e-mail and instant message channels open on the desktop, the temptation to ask informants to elaborate some point or confirm some detail was ever present. Initially, I saw this situation as beneficial to my work. Not until I encountered Kondo’s essay on the “dissolution and reconstitution of self” entailed in ethnographic practice did I recognize my exit from the field as incomplete. As Kondo argues, writing the ethnographic text requires distance and closure, “freezing

the disturbing flux” (1986:82). In my case, the process of distancing required me to change my view of what it meant to sit down at a computer. It required a conscious break with habits and expectations of networked sociality developed over the course of fieldwork. I could no longer regard my computer as a window on my social world and had to remove myself from the various online forums that had become part of my daily life.

Once I realized the need for remove and closure, I found that the writing process itself produced distance along with an awareness of writing for multiple audiences. Defining terms that were common place to my informants and developing an analysis of their social imaginaries and practices strengthened my identity as an ethnographer and recognition of this as an outsider position. Finally, I found reflecting on and articulating the various subject positions I had occupied over the course of research and writing to be a necessary component of the distancing process entailed in the production of the ethnographic text.