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Anonymity and Place in Qualitative Inquiry

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This article examines assumptions embedded in the routine practice of trying to make the places represented in qualitative accounts anonymous. Anonymity is usually seen as an ethical issue, but like any representational strategy, it conceals assumptions about the nature of entities in the world and our relations with them. Focusing on place anonymization, the author argues that the use of pseudonyms and the omission of identifying historical and geographical information align research accounts with certain ontological assumptions, modes of theorizing, and corporate constructions of the public sphere. The author concludes by suggesting ways that place and identification can be rethought in qualitative inquiry.

Techniques of obscuring identities¹ are commonly employed in qualitative accounts but rarely discussed in texts on methodology or representation; their methodological, political, and theoretical implications go largely unexamined (e.g., Behar & Gordon, 1996; Clifford & Marcus, 1986; Denzin & Lincoln, 1994; cf. Hopkins, 1993). Even pseudonyms, the most common anonymizing tools, are usually considered only as devices for protecting participants, not as strategic tools that play important roles in constituting objects of inquiry (e.g., Deyle, Hess, & LeCompte, 1992; Lincoln, 1990; Szklut & Reed, 1991). Anonymization thus approaches the status of a “blackboxed” or “stabilized tool” in qualitative research, one “no longer questioned, examined, or viewed as problematic, but . . . taken for granted” (Clarke & Fujimura, 1992, p. 10). The problem with such tools, useful though they may be, is that they congeal all sorts of assumptions that need to be scrutinized, and fit together representational genres that make certain kinds of accounts easier and others harder to articulate. My aim here is to provoke discussion of these issues by unpacking one form of anonymization—that of places and settings—that I’ll argue has both (a) ontological effects, in helping decouple events from specific locations and facilitating their use in certain kinds of theoretical claims, and (b) political

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implications, in distancing the participants and events described from a public sphere shared with researchers and readers.

THE PROBLEMATIC ACCOMPLISHMENT OF ANONYMITY

But first a short detour. Inasmuch as anonymization is defended as a default position largely on ethical grounds, I begin by briefly examining how it actually works in this respect. It is, after all, a methodological axiom in some fields that researchers should withhold the real names and locations of the settings and participants they study. As Punch (1986) puts it, "In general, there is a strong feeling among fieldworkers that settings and respondents should not be identifiable in print and that they should not suffer harm or embarrassment as a consequence of the research" (p. 45) (cf. Bulmer, 1982).

This position seems to assume that (a) identification can harm, embarrass, or invade the privacy of participants; (b) the use of pseudonyms and other anonymizing techniques can prevent identification; and (c) identifying settings and locations makes participants more easily identifiable. Although the first assumption is plausible, there is curiously no systematic research on the consequences of being identified by name in research reports (or in journalistic accounts or video documentaries, where real names are commonly used).² The second assumption is correspondingly difficult to evaluate, but the available commentaries, reviewed below, suggest that the standard techniques—pseudonyms in particular—are unreliable. Hence, the third assumption, even if true (again, evidence is lacking), may well be irrelevant.

Several characteristics of qualitative inquiry make anonymization efforts unreliable. First, the very activity of doing extended fieldwork implies a level of public visibility and engagement—of being seen and presenting oneself as a researcher in certain places, at particular events, with specific people—that later makes it relatively easy for others to reconstruct identities (of settings, if not individuals) from published accounts. Johnson (1982) puts the matter bluntly: "There is no way that a scientist can ensure that the identity of a community studied will remain secret. There are too many different ways in which the identity of a community can purposefully or accidentally be discovered" (p. 85).

A second and related problem is that the processes of gaining access to settings produce a large cast of tangential participants—for example, the administrators, managers, and officials with whom one negotiates for access—who will know where the research is being undertaken and who some of the main participants are and may knowingly or accidentally publicly reveal their identities to others.³

Third, as Hopkins (1993) argues, when researchers focus on close-to-home groups or situations, seek out collaborative relationships, merge friendship

and research relationships, study small, distinctive groups or isolated ethnic or lifestyle communities, or work with the same groups or institutions for extended periods of time (all increasingly common features of qualitative studies), anonymity becomes a practical impossibility: Any principled account of such inquiry must describe the shared community and neighborhood spaces and locate the groups and individuals studied with reference to the clearly identified researcher. As Hopkins states, "To really tell the story" of such groups "would clearly identify them" (p. 124).

Finally, as Wolcott (1973) has pointed out, "To present [qualitative research] material in such a way that even the people central to the study are 'fooled' by it is to risk removing those very aspects that make it vital, unique, believable, and at times painfully personal" (p. 4) (see also Lincoln, 1990; Whyte, 1964, p. 56).

More fundamentally, the information required to make accounts persuasive and true to central participants can identify settings and individuals even to those less fully involved, including outside observers or people who simply know or work with participants. This quandary would seem to hold for any study that focuses closely on individuals, self-identified groups, or specific institutional or public settings. It suggests that anonymization is likely to be most problematic precisely where it would be most useful—at the local level—and that it can do little to protect the identities of participants from intimates and associates or from the midlevel officials and bureaucrats they deal with—the very people likely to be in positions to react or retaliate against them.

Compromised on a local scale, it is also unclear if devices such as pseudonyms can afford participants protection from extralocal agencies of surveillance. Szklut and Reed (1991) argue that "it is doubtful if governments and other agencies, which caused our original concern with confidentiality, are so powerless as to be unable to identify fieldwork locations" (p. 101).⁴ Depending on the size and heterogeneity of the setting and the type of research conducted, even people remote from depicted events can identify pseudonymous individuals. In life history research, for example, "so much intimate detail is likely to be revealed that it will not be too difficult for anybody dedicated to finding out who the subject is, actually to do so" (Plummer, 1983, p. 92). More generally,

protecting the anonymity of informants whose disclosures have been quite detailed or of individuals identifiable by their specialized roles or idiosyncratic behavior becomes extremely difficult. Names can be changed, but that does not always disguise the individuals. In fact, a determined investigator could almost always discover who had worked with the resident fieldworkers. (Deyle, Hess & LeCompte, 1992, p. 633)

As Plummer (1983) points out, "Fifty years after the original study, Shaw's *Jack Roller* could be located for a re-interview (Snodgrass, 1978), and after only

a month's detective work could Oscar Lewis's *Children of Sanchez* (1961) be tracked down by a reporter (Diener and Crandall, 1978, p. 103)" (p. 142).

Anonymization, in short, is inherently problematic: Participants or observers near the events described are unlikely to be misled by pseudonyms or other anonymizing practices, and distant outsiders who have the resources to investigate a case can likely identify the sites and individuals it describes.⁵ Anonymization protects participants from identification and consequent harm or embarrassment only insofar as local people have no objection to what's written (or cannot or do not bother to read it) and what's written is of too little import to attract the scrutiny of outsiders.⁶

PLACE ANONYMIZATION

It is not my concern here to fix the weaknesses of anonymizing measures. I summarize them only to forestall the automatic insistence that such measures are necessary or sufficient to safeguard against identification and harm. Once we suspend this assumption, we can scrutinize anonymization for what it is: a representational strategy with interesting ontological and political implications. The most striking of these, I think, have to do with the way anonymization naturalizes the decoupling of events from historically and geographically specific locations (and with the way location or place itself is conceptualized).

It is easy to imagine situations where, in spite of the difficulties just outlined, we would want to give people pseudonyms and make it as difficult as possible to identify them.⁷ It is much harder to think of good reasons to use pseudonyms for regions, cities, or communities. The rationale for place anonymization is presumably to make people more difficult to identify. Naming the particular organization someone works for or the street they live on, even if one gives the person a pseudonym, makes it easier for strangers to find and harass them (although, again, there is little evidence if or to what extent identifying accounts, such as televised documentaries, have had this effect). It is less clear if naming regions, cities, or even neighborhoods (but not individuals) increases the chances of such contacts or some other kind of harm or embarrassment to participants. Researchers in fields such as urban and community studies, for example, routinely identify towns and cities by name (e.g., Davis, 1990; Dorst, 1989; Foley, 1995; Portes & Stepick, 1993; Zukin, 1991; also see Szklut & Reed, 1991 for a review). As Orlans (1967) shows, even when pseudonyms are used in such works, the identities of sites are widely known and frequently revealed, often by other researchers: "Powdermaker names the state in which the community she and Dollard studied is located, whereas Dollard takes pains to conceal it; and the Guatemalan town that Gillin calls 'San Carlos' (1951) is identified in Tumin's study (1952: xi)" (Orlans, 1967, pp. 362-363) (see also Szklut & Reed, 1991, p. 106).⁸

Although there is no evidence of such identifications' bringing harm to participants, place anonymization persists as the standard practice in many areas of qualitative inquiry. For example, in roughly 75% of recent articles (excluding reviews, methodology pieces, and the like) in two major journals publishing qualitative research in education,⁹ authors attempted to anonymize place, in most cases by using pseudonyms.

The interesting question is not why this practice continues: Anonymization can be considered "blackboxed" precisely to the extent that it seems natural and requires no explicit justification (few researchers, I would guess, stop at the first pseudonym in a text and ask why the person or place is being anonymized). More interesting are the questions of what anonymization does and how it works. I will try to provide at least partial answers by examining place anonymization as a technology that allows researchers to coordinate case material with theoretical terms and align accounts with hegemonic forms of space produced by corporate and governmental actors.

ANONYMIZATION AND THEORY

Anonymization has long played a role in theorizing. Goldschmidt (1950), for example, suggests that early anthropologists in the United States anonymized towns (Yankee City, Plainville, Middletown, etc.) in an effort to replicate earlier accounts of indigenous peoples that treated small settings such as villages as bounded microcosms of larger cultures or societies:

The use of pseudonyms for the community of study is itself a telling trait. The argument that this is done to protect the individuals and avoid libel seems to be a rationalization. Neither the academic profession nor the local people are deceived. (A schoolteacher from the community of Blumenthal's [1932] *Small Town Stuff* told me she'd read a copy with the appropriate names filled in.) . . . I suggest that this trait is a verbal manifestation of the anthropologists' assumptions of the broader generalization of the study. (Goldschmidt, 1950, p. 486)

Anonymization works as an essential element of an academic genre—theory—in which people, organizations, and groups are dislodged from their histories and geographies (the kind of information that would make it easier for readers to identify them). Giving people or places pseudonyms and strategically deleting identifying information turns them into usable examples or illustrations of generalizing theoretical categories (cf. Smith, 1987), in which form they can stand in for social classes, ethnic groups, genders, institutions, or other theoretical constructs. As Lutz (1995) suggests, following Smith (1974), theory is

a process in which statements are denuded of their origin in a writer and his or her experience or are stripped of their reference to a concrete phenomenal world of specific contexts and history. Theory is generally and informally seen as con-

sisting of more rather than less abstract statements, widely relevant or universalistic or "deeper" statements of more ultimate or timeless [and placeless] value than others. (p. 253)

But theory is not just abstract statements; it can also be congealed in the case materials of anonymized qualitative inquiry. A particular school in a particular town or neighborhood studied at a particular historical moment can be treated as if it were a placeless, timeless, representative instance of school. Eckert (1989), for example, can assert that the anonymized high school she studied in an anonymized suburb of Detroit in the early 1980s could be any suburburban, urban, or rural high school at any time: "The dynamics to be described in the following pages are society-wide and not specific to any one school or district" (p. viii).¹⁰ This claim of generality is unusually explicit, but anonymized accounts always have the force of assertions that the schools, cities, or neighborhoods described need not be named or situated in specific locales or eras. Events, people, and organizations are given standardized labels or category names (e.g., high school) that are meant to be understood in terms of what linguists call an ideology of "pure reference" (Silverstein, 1976): That is, the term is taken to refer to a specific type of standardized and widely distributed entity. The "indexical" functions of discourse, which define the spatiotemporal relations of participants to the things being referred to (relations either preexisting or created through the discursive act itself), are ignored (Duranti, 1997; Silverstein, 1976; Silverstein & Urban, 1996). More accurately, the placelessness produced through anonymization defines the spatiotemporal relations of writers and readers in terms of a disciplinary field—the static, abstract space of academic discourse and its attendant theoretical constructs and claims (cf. Smith, 1987, 1999). Instead of mapping how authors are positioned socially, culturally, historically, and geographically with reference to identified sites and tracing their pathways to the settings described, anonymized accounts make representations or texts movable, replicable, and citable (Urban, 1996)—one can treat Eckert's (1989) account as a model of student culture at other high schools—but undercut their usefulness in the local context, where they no longer need be responded to by participants.

Discourse presenting itself through deictics and other devices as closely bound to the originator and to the local context of origination tends to be responded to rather than replicated. Hence, it serves less well as culture in the classical sense. In contrast, discourse marking itself as detached from the local [e.g., anonymized academic case studies] is correspondingly more replicable . . . a distilled type [of culture] that presents itself as decontextualized or polycontextual, not serving the local interests of any of the participants in the replication process and hence being more readily replicated by all. (Urban, 1996, p. 42)

If anonymization makes cases seem similarly decoupled from deictic reference, hence movable and "polycontextual" (see also De Certeau, 1984, p. 20; Latour, 1987), providing identifying details anchors them to particular space-

time locations and, at least in principle, allows readers or other researchers to respond to or challenge the account.¹¹ If one knows exactly where and what its setting was, for example, one could ask if the processes described in the school Eckert (1989) studied would play out in the same way in a suburb with a different political economy, with students of different ethnicities, at some other period in history, at a larger or smaller school, at a school with a different curriculum, and so on.¹² Because anonymizing the case requires the omission of information on such processes, however, we cannot draw on it to ask or answer these questions.¹³ In addition to making empirical material more theoretically pliant, then, anonymization favors certain styles of theorizing, particularly those that compress social life into the interior, reflective states of individualized actors. This interiorization is accomplished first by stripping away public identities and associations and, second, as Atkinson and Silverman (1997) point out, by concealing the historical relations of interviewers, interviewees, and the public institutions or corporate groups with which they are affiliated.¹⁴

In one type of study, for example, researchers interview a collection of people they take to represent presupposed theoretical categories—for example, working-class women or middle-level managers. The interviews typically seek “to elicit personal narratives of experience or confessional revelations” (Atkinson & Silverman, 1997, p. 309). Unanchored strips of interview discourse are then quoted to illustrate the characteristic inner, private experiences (attitudes, beliefs, etc.) of members of the category. Alternatively, researchers look for interactional regularities in an event or setting that has been spatially and temporally “sectioned out” (Smith, 1987) of participants’ ongoing practices (e.g., a classroom or school treated as self-contained and independent of everything going on outside, before, and after it). The now-bounded and well-defined setting is taken to stand for a whole class of events similarly named and sectioned out, and its regularities are treated as the characteristic essences of all such settings.

Such work can be interesting and useful. Studies such as Eckert’s (1989) or Goffman’s (1961) *Asylums* (to pick up an example thrown at me by an earlier reader) provide us with powerful categories and models with which to interrogate public institutions. My point is only that there is a price to that power: the institution (even more in Goffman, 1961, than in Eckert, 1989) is shaved off from politics, history, geography, urban form, popular culture, and so forth—that is, from the very processes that make a book such as *Asylums* meaningful by standardizing the institutional forms it analyzes across space and time.¹⁵ The idea of the particular institution where the research is conducted being a place we might visit or to which we might somehow already be linked, the idea of the people in that institution as real biographical entities such are ourselves rather than descriptive fragments illustrating constructs of sociological discourse, and finally the idea that public institutions are politically and culturally contested arenas in a public sphere are part of what we give up for the theoretical boost that comes with anonymization. As I argue in

the next section, this transformation of places into floating theoretical exemplars also presupposes a certain politics of space that first compresses the public sphere into the practices of individualized participants and then reinflates it as a private, anonymous realm populated by discrete, standardized entities—and, in doing so, links anonymization to an ongoing transformation of the public sphere in late 20th-century America.

PRIVATIZING THE WORLD

The commitment to treat places as anonymous resonates with an ongoing reshaping and privatization of public space. In the past half century, the parks, squares, streets, markets, neighborhood stores, and other local gathering places where one “knowingly accepted the responsibility for the public character of one’s actions” (Shils, 1959/1967, p. 346) have been vanishing or becoming “progressively less public . . . more exclusive than at any time in the past 100 years” (Zukin, 1995, p. 28) (see also Davis, 1990, p. 226; Fainstein, 1994; Gottdiener, 1997, p. 142; Low, 1996, p. 397; as well as the criticisms of Aurigi & Graham, 1998).¹⁶ The resulting privatized landscapes of gated communities, private parks, fortress-like malls, and the like are produced when businesses or governments create material and symbolic borders to attract certain kinds of people and keep others out (Davis, 1990; Zukin, 1995). Privacy is less about the inviolability of the body or control over information about oneself—people willingly expose themselves to metal detectors, video surveillance, and the like as conditions of access to high-status shopping or business enclaves—than about access to borders that make one invisible to certain kinds of people—in general, poor people—and allow one to avoid contact with them.¹⁷ When the groups who create, control, or actively participate in such settings invoke a right to privacy and anonymity—or when researchers automatically try to make such settings anonymous—the boundaries they create are of a different type: not gates to keep out unwanted bodies but boundaries on what can be represented and debated.

Consider the official and unofficial documents and accounts (from city plans, newspapers, and maps to local histories and archives) in which places are treated as objects of explicit attention, indeed projects that participants are trying to shape through their representations. As Dorst (1989) points out, such texts both reflect and play a key role in how people make sense of themselves:

The culture of advanced consumer capitalism or, less acceptable but more fashionable, postmodernity, consists largely in the processes of self-inscription, indigenous self-documentation and endlessly reflexive simulation. . . . Postmodernity . . . “spontaneously” does for itself, and massively so, the sort of thing ethnographers and other species of documentarist claim to do. (p. 2) (cf. Szklut & Reed, 1991, p. 106)

Efforts to anonymize place, however, seriously limit our ability to analyze the self-inscriptions that index place making and boundary construction practices, especially those of the powerful.¹⁸ When the settings and places where events unfold are simply taken as givens instead of scrutinized as contingent and unfinished outcomes of power and struggles, we detach our accounts from such struggles or, worse, become complicit in the political projects of dominant groups and organizations to produce spaces to serve their own ends. As Appadurai (1996) argues,

Both the ethnographic project and the social projects it seeks to describe have the production of locality as their governing telos. The misrecognition of this fact in both projects, as involving only more humdrum and discrete actions and settings (house building, child naming, boundary rituals, greeting rituals, spatial purifications), is the constitutive misrecognition that guarantees both the special appropriateness of ethnography to certain kinds of description and its peculiar lack of reflexivity as a project of knowledge and reproduction. Drawn into the very localization they seek to document, most ethnographic descriptions have taken locality as ground not figure, recognizing neither its fragility nor its ethos as a *property of social life*. (p. 182)¹⁹

Anonymization, by helping transform concrete, historically and politically contingent settings into private, anonymized, taken-for-granted regions, aligns researchers with a politics of space that diminishes the sphere of public discourse and contestation. Fraser (1994) points out in another context that

a rhetoric of privacy has historically been used to restrict the universe of legitimate public contestation. . . . The result is to enclave certain matters in specialized discursive arenas and thereby to shield them from general public debate and contestation. This usually works to the advantage of dominant groups and individuals and to the disadvantage of their subordinates. (pp. 89-91)

Research that ignores the historical and geographical processes that produce and maintain places in larger networks of practice becomes complicit in the silences and exclusions upon which those spaces are premised. As Bakhtin (1981) suggests,

However forcefully the real and the represented world resist fusion, however immutable the presence of that categorical boundary line between them, they are nevertheless indissolubly tied up with each other and find themselves in continual mutual interaction; uninterrupted exchange goes on between them. . . . The work and the world represented in it enter the real world and enrich it, and the real world enters the work and its world as part of the process of its creation, as well as part of its subsequent life, in a continual renewing of the work through the creative perception of listeners and readers. (p. 254)

“The real world enters the work” in anonymized accounts when, instead of showing how spaces and times are historically and geographically contingent accomplishments of power relations favoring some over others, we use

them as unproblematic discursive resources. Anonymized places situate people and events in something like the “abstract expanse” Bakhtin (1981, p. 99) describes as part of the “adventure” “chronotope” (Bakhtin’s term for a particular organization of space and time): “for a shipwreck one must have a sea, but which particular sea (in the geographical and historical sense) makes no difference at all” (p. 100); events “have no essential ties with the particular details of individual countries that might figure in the novel [or case study], with their social or political structure, with their culture or history” (p. 100). In the modern variant, this abstract space is a generic container of anonymous, interchangeable fragments (Lefebvre, 1991), any one of which can be taken to stand for all the others. The work enters the real world when readers use accounts grounded in such representations of space to make sense of their experiences and relations with others. One aspect of this failure to indexically locate the places in everyday worlds that participants, readers, and writers share (however unequally positioned we may be) is to produce or reinforce distances separating us.

ANONYMITY AND CONNECTION

Let me try to be clear: The issue here to is not simply the use (or ethics) of pseudonyms; it is the way their use can lead researchers to unreflectively produce representations of the world that obscure or ignore the connections linking places, writers, participants, and readers. Although usually treated as an aspect of research ethics, anonymization is an engine of detachment,²⁰ a technique of “spatiotemporal distancing” (Fabian, 1983, p. 159) (one of many used in academic writing) that obscures these connections.²¹ Exploring the assumptions and positioning it congeals is a preliminary but essential step in developing representational practices that can situate participants in different but articulated positions in a common world (Haraway, 1988; Strathern, 1991). In closing, I will briefly suggest what some of those practices might entail.

Mere naming is not sufficient. Naming that simply denotes people or places does not overcome the problems I have attributed to anonymization (especially if the people and places named are remote from most readers). Indeed, naming as mere denotation can foster the illusion of a singular, essential identity and mask the way that multiple identities are interactively layered through a person’s participation in multiple activity systems (e.g., Hall, 1996). The same applies for place names if places are conceptualized as small, bounded, face-to-face communities, the pleasant small towns of the Hollywood imaginary (Rutheiser, 1998). Rather than simply denoting, then, names must be indexically incorporated into acts of indicating and shaping relationships that stretch out to include distant readers and writers.

With this qualification, I would argue that a commitment to naming places and showing how they are constituted by processes that run through the everyday worlds of writers and potential readers makes it more difficult (although admittedly not impossible) for researchers to simply section out particular times and spaces (say, a certain school in a certain place at a certain time), strip away the particulars, and treat what is left as a generic, abstract exemplar of some larger category (school in general). Naming places and tracing their constitutive processes allows researchers to emphasize connections among people, places, and events and to highlight the systems of relations and processes of articulation that produce boundaries and entities.

To carry this off, however, we will need to reconsider both what *connection* means and how we understand place itself. Feminist, activist, and postmodernist researchers have already begun to reimagine qualitative inquiry as a shared project in which participants are actively engaged as collaborators from initial design to publication (e.g., Cameron, Frazer, Harvey, Rampton, & Richardson, 1994; Kirsch, 1999; Nespor & Barber, 1995). LeCompte (1993), for example, suggests that

ethical research on the disempowered, whether in a social activist or a scholarly tradition, obliges researchers to consider how informants will participate in the disclosure of their situations and secrets, as well as how researchers will participate in the future life and destiny of the people they study. None of these issues are matters which can be unilaterally decided by the researcher, or even by the researcher in consultation with colleagues, disciplinary codes of conduct, or guidelines for the ethical treatment of human subjects. . . . Rather, they are a matter for open and egalitarian discussion and negotiation between researchers and the researched. They are also the subject of possible veto by the researched. (p. 11)

In inquiry of this kind, anonymizing place deflects activist agendas. The projects summarized in Nyden, Figert, Shibley, and Burrows (1997), for example, involve university-based academics and members of community organizations working together to study issues identified as salient by the community: Anonymizing the location of the work would undermine its usefulness for informing public debate and policy on problems specific to those settings.²² More problematically, perhaps, to turn engaged participants into anonymous characters belies the egalitarian intent LeCompte (1993) invokes—unless the decision to anonymize is made by all involved after an analysis of consequences that might follow from different strategies of representation (and a discussion of these consequences with participants not intimately involved in the collaboration) (Lincoln, 1990; Shulman, 1990).²³ Turning current practice on its head and beginning with the presumption that real names should be used might provide a useful heuristic, forcing principals to think through the possibilities and risks of the inquiry and to consider, given the fragility of anonymizing measures, how identification might affect them (Johnson, 1982; on film, Tobin & Davidson, 1990, p. 278).²⁴

As complex as these issues and negotiations would be, abandoning place anonymization as a default position entails more than changing the nature of researchers' relations with other participants or becoming more openly politically engaged: It requires us to rethink the very idea of sites, settings, and places and to see them as produced by as well as producing social relations (see Lefebvre, 1991). Anonymizing a place suggests that the identities and events that happen there float, so to speak, above or outside specific historical and geographical moments. In suggesting that this detachment is problematic, my intention is not to privilege the local—at least not localities conceived as bounded and self-contained entities (cf. Gregory, 1999). Identities, as post-structuralists have stressed, are partly assembled from national and transnational discourses of gender, class, and race, from international commodity circuits, and from global flows of popular cultural (Appadurai, 1996). The weaving of these flows into everyday life, however, is always fundamentally anchored in particular material settings.²⁵ As Hanson and Pratt (1995) argue in their study of gender and work in Worcester, Massachusetts,

Individuals are connected to others according to the city's spatio-temporal layout in ways that make a difference to their experiences of gender, class, and race, among other social relations. In our case study we try to understand how contemporary social and economic boundaries are constructed, in part of previous ones, and the numerous ways that local places and identity intersect, overlap, and shape each other. (p. 22)

The conception of place embedded in such a view resembles Doreen Massey's (1994) definition of places as products of the multiple intersections of political, economic, and cultural spacetimes, as

not so much bounded areas as open and porous networks of social relations. . . . [Places] can be imagined as articulated moments in networks of social relations and understandings, but where a large proportion of those relations, experiences and understandings are constructed on a far larger scale than what we happen to define for that moment as the place itself, whether that be a street, or a region or even a continent. (pp. 121, 154)²⁶

Practices of inquiry and representation do not just discover or document relations; they presuppose and entail them or, as I have argued in the case of anonymity, hide and deflect them. In saying we should locate action in its places, I mean we should show how economic, political, cultural, and institutional practices produce places and organize them into landscapes within which (or through which) participants, researchers, and readers can jointly orient themselves. Settings such as homes, streets, and workplaces have to be mapped into political and cultural economies that are worked out at regional, state, national, or international levels across long as well as short durations. Situating focal settings in such contextualizing relations necessarily identifies them: That is, to make distant lives and activities recognizable to readers as somehow connected to their worlds, we much situate them

in systems of relations such as regions, states, and economies that cannot be plausibly anonymized.²⁷

Finally, naming and situating places as the historically and geographically contingent products of multiple, ongoing processes undercuts their value as “polycontextual” exemplars of theoretical categories. Situating action in places conceived along the lines Massey (1994) suggests implies a different way of thinking about theorizing. Instead of obscuring how activities are anchored in historically situated places and times, a goal of research would be to explicate how such anchored activities, separated in time and space, get linked together to form a shared world. Rather than containers of representative instances, places could be starting points from which to trace the networks of relations out of which they are constituted. Instead of having as a goal the creation of a superordinate account or theory of some process, the aim would be to explicate how those constitutive relations work in time and space. As Smith (1987) writes,

The single case [in the traditional sociological account] has no significance unless it can in some way or another be extrapolated to some general statement either about society or some subgroup represented methodologically as a population of individuals, or connecting the local and particular with a generalizing concept of sociological discourse.

Beginning with the everyday world as problematic bypasses this issue. The relation of the local and particular to generalized social relations is not a conceptual or methodological issue, it is a property of social organization. The particular “case” is not particular in the aspects that are of concern to the inquirer. Indeed, it is not a “case” for it presents itself to us rather as a point of entry, the locus of an experiencing subject or subjects, into a larger social and economic process. (p. 157)

The theoretical project of social inquiry would proceed in these terms not through the creation of more comprehensive and enveloping categories but through mapping the concrete relations that link (and constitute) different places, and locating and identifying those places with reference to one another. If student groups in a high school somewhere in a suburb of Detroit in the 1980s (Eckert, 1989) actually turn out to resemble high school groups at other times and places, that should be a starting point for inquiry—how is such coordination concretely accomplished across space and time—rather than a claim to theoretical generality. Instead of a methodology that sections out blocks of space-time as frozen, theoretically relevant settings, one would make the production of space-times part of the problem by following the pathways (both improvised and institutionalized) along which participants, researchers, artifacts, and information travel.

Opening the black box of anonymity is itself only a starting point in such a project. As already noted, simply giving the name of a place does not automatically reconceptualize it as a place in the sense I have advocated or shift the focus of inquiry to connections and relations. If not sufficient, however, rethinking anonymization is a necessary step in this project: It focuses atten-

tion on the central questions of what it means to identify and situate places and people in accounts that map everyday worlds—questions that default anonymization makes difficult or impossible to ask.

NOTES

1. One consequence of its “blackboxing” is that there is no generally accepted definition of *anonymization*, let alone a category system for distinguishing its various forms. Although one reader encouraged me to work through the different implications of the various techniques in detail, my sense is that such an effort would be fruitless and that the ways one can obscure the identities of people and places are limited only by a researcher’s inventiveness. No account can include everything, and every decision about what to say about someone or something is also a decision (especially if pseudonyms are used) to hide something. Anonymization is a function both of masking practices (e.g., the use of pseudonyms) and omitting identifying information, the proportions varying from work to work (and sometimes within a single work). The focus here will be on what seem to be the most common anonymizing tactics: saying little or nothing about where or when things happen or about the people involved in them (as in studies that focus on particular types of events or interactions taking place in common, recognizable settings) and using pseudonyms for people and places (e.g., Eckert, 1989). I do not address the use of composites (e.g., Rollins, 1985) or instances where real names and pseudonyms are mixed (e.g., Barton & Hamilton, 1998). I also do not try to measure the thoroughness of anonymization: that is, for example, the difference between saying nothing at all about a place on one hand or, at the opposite extreme, telling about it extensively while using pseudonyms for everything. Although there are clear differences (e.g., the more one includes pseudonyms, the easier it is to identify the setting), the arguments I make about political and ontological effects apply in either case.

2. Clearly, there have been cases where documentaries have had negative consequences for participants. Barbash and Taylor (1997) mention one, Tanya Ballantyne’s 1966 film *The Things I Cannot Change*: “Intended as a sympathetic portrait of poverty in Canada, the film was approved of by its participants after a screening. Yet when it was publicly shown, the family it focused on was criticized by the community” (p. 49). What is unclear is how common such cases are.

3. For example, someone who had been a prominent participant in a school ethnography I had done became a doctoral student at my university and, in one course, was asked by an administrator in my department (who knew the circumstance of my fieldwork) to talk to a class (which consisted mostly of school administrators from around the region) about his experiences as a research participant (I was not consulted about this beforehand). In addition to the central administration of the school division (from whom I had had to get permission to do the research), the parents, business partners, volunteers, and visitors from community organizations whom I met at the school while doing the fieldwork, along with the people in my own department who helped or collaborated with me in some way—and all the people all these people may have told about the study—a whole network of administrators from around the region would now be able to identify my pseudonymous school (assuming—and this may be assuming a lot—that they remembered the presentation in question, remembered my name, and then read the book).

4. On the other hand, publishing accounts using the real names of participants may provoke agencies to act, whereas the work needed to penetrate pseudonyms might encourage them to overlook reported transgressions in anonymized accounts.

5. Some codes of professional ethics seem to implicitly recognize this point: They allow researchers to promise participants anonymity while acknowledging that such promises cannot be kept:

It should be made clear to anyone providing information that despite the anthropologist's best intentions and efforts anonymity may be compromised or recognition fail to materialize. (American Anthropological Association A.1.a)

It should also be made clear to informants and participants that despite every effort made to preserve it, anonymity may be compromised. (American Educational Research Association, II.B.10)

6. The apparent rarity of identifications by distant readers may reflect the small readership of the research literature more than anonymization's effectiveness. Newspapers may be a different matter. Kotlowitz (1991) reported that the Illinois Department of Public Aid "regularly combs newspapers for possible hints of welfare recipients who may be ineligible for benefits" (p. 100). One of Kotlowitz's stories for *The Wall Street Journal* prompted an investigation that resulted in support being cut off for a family he had written about. The positive values of jargon and bad writing in buttressing anonymity by repelling nonacademic readers has been generally overlooked.

7. And not only to protect participants. Blee (1999) recounts that the racist activists she studied wanted their real names used to garner attention and attract recruits. Blee "imposed" anonymity to "support the academic and political goals of the researcher, against the expressed interest and desires of the informants" (p. 995).

8. Orlans's (1967) citations may seem dated, but the kind of practices he describes continue. For example, Gutierrez (1998) refers to Foley, Mota, Post, and Lozano's (1977) anonymized town by its real name and even complains that Foley "regrettably . . . utilizes the trademark of anthropologists, omitting the real names of people and places in his book" (p. 6). The regret is presumably because Gutierrez (1998) feels that anonymization robbed the book of its potential political uses to participants in the town and region. Foley et al.'s (1977) work (see also Foley, 1990, 1995) is actually unusually complex on this point, however. Although I argue in the body of this article that anonymization undercuts the need of participants to respond to an account, one can, as Foley et al. (1977, pp. 242-260) do, explicitly ask selected participants to respond and then include their responses in the published text. In this case, however, the quoted respondents seem to position themselves as evaluating the accuracy of the work, not responding to it (or attempting to use it) politically. When Foley (1990) later returned to the community, there does not seem to have been much lasting trace of the previous book (although some of the residents were clearly aware of it, e.g., Foley, 1990, p. 155). In a later work, Foley (1995) not only names the town (Tama, Iowa) but intended to use the real names of the main participants in the stories he told. Again asking participants to read the work with this in mind, the result was "long, sometimes painful conversations," but most agreed to have their real names used. A small minority, however, objected, threatened legal action, and complained to the publisher, and, in the end, pseudonyms were used for individuals.

9. I focus on education partly because it is the field I work in and partly to anticipate the argument to come, because schools are arguably one of the most public institutions in modern society, at least in the sense proposed by Bachrach and Botwinick (1992): "An organization or institution should be considered public if its decisions and nondecisions, and indeed, its very existence, has a significant impact upon the life of the community in which it is located" (p. 138). The journals in question were the *Anthropology and Education Quarterly* (AEQ) (Vol. 29, No. 1, 2, 3, and 4) and the *International Journal of Qualitative Studies in Education* (Vol. 11, Nos. 3 and 4; Vol. 12, Nos. 1 and 2). The quarter or so of the research articles that did name sites were mainly reporting research outside the United States, illustrating a point I make later regarding anonymization's role as a distancing device in Fabian's (1983) sense. In a more recent issue of AEQ, Rose (1999) uses the name of the American school he studied, but it should be noted that Rose has considerable experience writing "journalistic" accounts of schools and teachers (e.g., Rose, 1995).

10. Lest there be some misunderstanding, I admire Eckert's (1989) book and have used it in courses. I could have found many other examples, including much of my own work, to make the same points. I choose *Jocks, Burnouts, and Others* precisely because it is well known and justly well regarded.

11. A reader for this journal pointed to a couple of famous examples of researchers restudying settings (even interviewing some of the same participants) and contesting the original accounts: Freeman's (1983) attack on Mead's (1928) work in Samoa, and Boelen's (1992) on Whyte's (1955) "Cornerville" research. There are already extensive commentaries on these cases (e.g., Brady, 1983; Richardson, 1992), so I limit mine to the point that much of the controversy, especially in the Mead/Freeman case, depends on the authors sectioning out the particular groups and settings studied from the various spatially and temporally extensive processes that constitute and contextualize them. Studies done at different historical, political, economic, cultural, and biographical conjunctures are compared as though they were synchronous alternative accounts rather than differently motivated accounts of different—although linked—moments in multiple cross-cutting, cultural-historical processes. In such situations, reexaminations become attempts to supersede and obliterate the originals (although the fact that the place of Mead's [1928] and Freeman's [1983] work was not anonymized, whereas Whyte's [1955] was does produce some differences in the surrounding discourse of others interested in the controversies: in the Mead/Freeman case, a number of commentators had also done research in Samoa, knew the settings referred to, and could substantively address differences in the accounts). Although it can be useful to question and critique fieldwork methods (e.g., Mead's [1928] methods were subjected to harsh criticism by contemporaries such as Radin, 1934/1966, and Mead herself reflexively critiqued them long before Freeman's [1983] work was published), the aim should not be to privilege one referential account over another (*X's* version is the true Samoa, *Y's* is false). I am arguing here for the creation of different kinds of accounts mapping out parts of the terrain from clearly located starting points, engaging the imaginations and everyday practices of participants, and inviting other researchers to undertake complementary studies from different perspectives.

12. Another reason there are relatively few identifications of anonymized qualitative studies may be that researchers and other readers prefer to have these pseudogeneral accounts; we do not want to deal with the spatial and temporal situatedness of the events and processes described.

13. As a result, the possibilities for drawing connections between studies are limited to forms such as citations of published accounts. Eckert (1989), for example, cites work such as Coleman's (1961) earlier study of adolescent groups. Just how similar the lives and experiences of the populations in these studies might be—decades apart and living in different geographical settings—is an interesting empirical question. I would guess not too similar. The earlier study can be made relevant to the later only because both delete, by anonymizing, information that would have indexically situated them in their original space-times.

14. This includes the contextualization of the interview as a discursive event. Interviews suppress certain kinds of indexical meanings and types of information (Briggs, 1986; Mertz, 1993). Topics of discussion are decontextualized (Briggs, 1986)—that is, recentered around the research event rather than other everyday practices. The "self" of the interviewee is produced as an interiorized, personal narrative (Atkinson & Silverman, 1997), and at worst, participants' views are reduced and fragmented into reflections of the disciplinary categories that frame the researcher's interview questions (Smith, 1987).

15. Indeed, in *Asylums* (Goffman, 1961), the institution is reified into a transcendent, self-contained entity, an effect exacerbated by Goffman's bricoleur-like appropriations of decontextualized examples from Paraguay, West Africa, and other places never explicitly connected to the particular site studied.

16. Feminist geographers (e.g., Day, 1999; Ruddick, 1996) point out that many of those earlier public spaces were hostile to women (as, of course, are many of the contemporary privatized public spaces). My point is not that things were good (public) in the past and are bad (private) now. It is that as material organizations of space have changed, the very ways we think about and represent public space and public activity have changed as well.

17. This sense of privacy is coming into fashion just as older notions of privacy that dealt with freedom from surveillance by powerful institutions and the right to control information about oneself are being undermined (e.g., Bennett & Grant, 1999).

18. Some researchers, of course, do quote official and published documents, plans, and reports while inserting pseudonyms for all the proper names (a practice going back at least to Lynd & Lynd, 1929), but in such cases, the identity of the setting is easily inferred unless critical detail is omitted (e.g., quoting a pseudonymized newspaper account of an event without mapping the event in a larger historical and geographical frame of reference—a practice that decontextualizes and undercuts the value of the accounts).

19. Appadurai's (1996) comments suggest that although we cannot ignore the "indigenous" place-producing texts found in the settings we study, neither can we treat them as unproblematic. Newspapers, for example, formulate particular kinds of space-time, usually a kind of abstract space of the sort that Lefebvre (1991) described as a "space of representation" that distances readers from one another—the opposite effect of what I later argue for in this article. Walter Benjamin (1968) made this point long ago:

Man's inner concerns do not have their issueless private character by nature. They do so only when he is increasingly unable to assimilate the data of the world around him by way of experience. Newspapers constitute one of many evidences of such an inability. If it were the intention of the press to have the reader assimilate the information it supplies as part of his own experience, it would not achieve its purpose. But its intention is just the opposite, and it is

achieved: to isolate what happens from the realm in which it could affect the experience of the reader. The principles of journalistic information (freshness of news, brevity, comprehensibility, and, above all, lack of connection between the individual news items) contribute as much to this as does the make-up of the pages and the paper's style. (p. 158-159)

Thus, the researcher must rework such texts in such a way that the freshness can ripen, the brevity can be anchored in deeper time scales, comprehension can be made problematic, and the connections among seemingly independent news items can be shown.

20. A sufficient but not necessary cause: Even qualitative accounts that do name places and participants often fail to trace out their lines of connection. Shulman (1990), for example, makes a cogent argument that in the case of school teachers collaborating with university researchers on studies of their classrooms,

the question of identifying teacher informants/collaborators can no longer be automatically answered on the side of anonymity. The ethnographer's traditions of rendering informants invisible were produced in an era when informants were seen as powerless and in need of protection. In our day, research on teaching has become one of the vehicles for the professionalization and empowerment of teachers. (p. 14) (cf. Lincoln, 1990, p. 279; Szklut & Reed, 1991)

In most published teacher research, however, the focus is resolutely on classrooms treated as if they were boxed off and discrete in space and time, and in most cases, even the politics of the teachers' and university researchers' relationships go unexamined.

21. Although Fabian (1983) is preoccupied with temporal relationships in ethnographic writing, many of his arguments are germane to the issue of placelessness as well.

On the one hand, we dogmatically insist that anthropology rests on ethnographic research involving personal, prolonged interaction with the Other. But then we pronounce upon the knowledge gained from such research a discourse which construes the Other in terms of distance, spatial and temporal. The Other's empirical presence turns into this theoretical absence, a conjuring trick which is worked with the help of an array of devices that have the common intent and function to keep the Other outside the Time [and the Place] of anthropology. (p. xi)

22. The ideas of negotiation and collaboration are, of course, problematic in their own right (Clifford, 1988; LeCompte, 1995). One's collaborators usually represent only a fraction of the members of the groups or organizations the research is concerned with: Others may have been excluded for some reason or may have had no time or desire to collaborate, and whole classes and groups of participants (e.g., young children) are routinely excluded or included as pseudoparticipants in projects devised by the researchers or powerful factions within the community (see Hart, 1997, for a summary of just how involved preadolescents can be in genuinely participatory inquiry). As Fraser (1994) points out, "deliberation can serve as a mask for domination" (p. 81).

23. Gluck and Patai (1991, p. 4) suggest that promises of anonymity may make it easier to gain access to settings and get people to agree to talk. This is not necessarily a good thing, however, and may actually forestall the politically explicit negotiations over representation that should take place between researchers and participants. In any event,

journalists routinely get access and produce intimate accounts using real names and identified places (e.g., Freedman, 1990; French, 1993; Lukas, 1985; Simon, 1991), and film and video documentarists study issues in ways that not only identify but broadcast recognizable images of individuals. Indeed, the association of real names with journalism may contribute to social scientists' reluctance to use them. Anonymization techniques may function in part as markers of academic professionalism. Becker (1968), for example, remarked of Vidich and Bensman's (1968) Springfield study,

If a man wishes to identify the objects of his study, all right. . . . He can state his intentions to the people he studies and can identify himself as a journalist, or a man who wants to write a book, and thus be free to publish whatever he pleases. (Becker, 1968, p. 415)

There has been overlap between journalism and qualitative research for a long time—Whyte (1955), for example, cites Lincoln Steffens as a major influence (and one can see in Whyte's use of "Doc" as a key informant clear continuities with Steffens's techniques for studying political machines by getting the "bosses" to talk to him)—and academic researchers can still learn from the best investigative journalists. I know of no study of school desegregation, for example, to rival Lukas's (1985) as a multisite, historically grounded and geographically aware account that links biography, family process, and large-scale political and economic processes. Still, even the best ethnographic journalists retain the realist or omniscient narrator style that has been justly problematized over the past 20 years (e.g., Marcus & Cushman, 1982): They fail to locate themselves or their institutions or to situate the standpoints and practices from which they construct their accounts (exponents of the "new journalism" offer some partial exceptions; see Zeller, 1995). Journalists, incidentally, also vary in their use of pseudonyms. Some (e.g., French, 1993) ask those depicted to read their works and give them the option of choosing to use pseudonyms if they wish. Others (e.g., Simon, 1991) ask participants to read and respond to what they have written but use real names and do not allow editorial rights. Filmmakers, by contrast, may ask for consent only once and probably do not show the finished film to participants before it is broadcast (e.g., Wiseman, 1971). In any event, anonymity does not attract a lot of attention in discussions of the ethics of investigative reporting; the main concerns now seem to be deceptive tactics and the concomitant risk of lawsuits (Greenwald & Bernt, 2000).

24. Switching the default would also mean that if a decision were made to use anonymization tactics, the writers would be obliged to then explain to readers just what the measures were intended to protect against. A reader of an earlier draft seemed to think that I was saying that because anonymization practices are not perfect, we should just blow them off. What I am actually trying to say is rather different: It is that we have failed to adequately analyze how anonymization works as a representational practice—what it allows, what it hinders—because we have assumed that it was an obligatory ethical tactic. Let me be as explicit as possible: Even if anonymization practices worked perfectly and hid identities completely, I think we should discard them as automatic default positions and instead articulate a clearer politics behind our strategies of identification or masking. In the area I focus on in the remainder of the article—the practice of masking the identities of (or simply saying nothing about) the places and settings of inquiry—I think we should go even further and consider a moratorium on anonymization.

25. Material settings are also articulations of multiple processes unfolding at different tempos and rhythms (e.g., Harvey, 1996). I problematize the concept of place later in the article.

26. Marcus (1998), advocating multisite ethnography, suggests that "if there is anything left to discover by ethnography it is relationships, connections, and indeed cultures of connection, association, and circulation" (p. 16) (see also Gupta & Ferguson, 1997).

27. The relative neglect of political economy in recent ethnographic experimentation (see Di Leonardo, 1998) may thus be connected with place anonymization: the initial commitment to an anonymized account makes it difficult to situate the case in larger political, economic, and cultural systems in anything more than a vague, indirect fashion.

REFERENCES

- Appadurai, A. (1996). *Modernity at large: Cultural dimensions of globalization*. Minneapolis: University of Minnesota Press.
- Atkinson, P., & Silverman, M. (1997). Kundera's *Immortality*: The interview society and the invention of the self. *Qualitative Inquiry*, 3(3), 304-325.
- Aurigi, A., & Graham, S. (1998). The 'crisis' in the urban public realm. In B. Loader (Ed.), *Cyberspace divide* (pp. 57-80). New York: Routledge.
- Bachrach, P., & Botwinick, A. (1992). *Power and empowerment: A radical theory of participatory democracy*. Philadelphia: Temple University Press.
- Bakhtin, M. (1981). Forms of time and of the chronotope in the novel. In M. Holquist (Ed.), *The dialogic imagination: Four essays by M. M. Bakhtin* (pp. 84-258). Austin: University of Texas Press.
- Barbash, I., & Taylor, L. (1997). *Cross-cultural filmmaking: A handbook for making documentary and ethnographic films and videos*. Berkeley: University of California Press.
- Barton, D., & Hamilton, M. (1998). *Local literacies: Reading and writing in one community*. London: Routledge.
- Becker, H. (1968). "Freedom and responsibility in research": A comment. In A. Vidich & J. Bensman, *Small town in mass society* (rev. ed., pp. 414-415). Princeton, NJ: Princeton University Press.
- Behar, R., & Gordon, D. (Eds.). (1996). *Women writing culture*. Berkeley: University of California Press.
- Benjamin, W. (1968). On some motifs in Baudelaire. In H. Arendt (Ed.) *Illuminations* (pp. 155-200). New York: Schocken.
- Bennett, C., & Grant, R. (Eds.). (1999). *Visions of privacy: Policy choices for the digital age*. Toronto, Canada: University of Toronto Press.
- Blee, K. (1999). The perils of privilege. *Law and Social Inquiry*, 24, 993-998.
- Blumenthal, A. (1932). *Small town stuff*. Chicago: University of Chicago Press.
- Boelen, W.A.M. (1992). Street corner society: Cornerville revisited. *Journal of Contemporary Ethnography*, 21, 11-51.
- Brady, I. (Ed.). (1983). Speaking in the name of the real: Freeman and Mead on Samoa [Special Section]. *American Anthropologist*, 85, 908-947.
- Briggs, C. (1986). *Learning how to ask*. Cambridge, MA: Cambridge University Press.
- Bulmer, M. (Ed.). (1982). *Social research ethics*. London: Macmillan.

- Cameron, D., Frazer, E., Harvey, P., Rampton, M., & Richardson, K. (1994). The relations between researcher and researched: Ethics, advocacy and empowerment. In D. Graddol, J. Maybin, & B. Stierer (Eds.), *Researching language and literacy in social context* (pp. 18-25). Clevedon, UK: Open University.
- Clarke, A., & Fujimura, J. (1992). What tools? Which jobs? Why right? In A. Clark & J. Fujimura (Eds.), *The right tools for the job: At work in twentieth-century life sciences* (pp. 3-44). Princeton, NJ: Princeton University Press.
- Clifford, J. (1988). On ethnographic authority. In *The predicament of culture* (pp. 21-54). Cambridge, MA: Harvard University Press.
- Clifford, J., & Marcus, G. (Eds.). (1986). *Writing culture*. Berkeley: University of California Press.
- Coleman, J. (1961). *The adolescent society*. New York: The Free Press of Glencoe.
- Colvard, R. (1967). Interaction and identification in reporting field research: A critical reconsideration of protective procedures. In G. Sjoberg (Ed.), *Ethics, politics, and social research* (pp. 319-358). Cambridge, MA: Schenkman.
- Davis, M. (1990). *City of quartz*. New York: Verso.
- Day, K. (1999). Introducing gender to the critique of privatized public space. *Journal of Urban Design*, 4(2), 155-178.
- De Certeau, M. (1984). *The practice of everyday life*. Berkeley: University of California Press.
- Denzin, N., & Lincoln, Y. (Eds.). (1994). *Handbook of qualitative research*. Thousand Oaks, CA: Sage.
- Deyle, D., Hess G. A., & LeCompte, M. (1992). Approaching ethical issues for qualitative researchers in education. In M. M. LeCompte, M. Millroy, & J. Preissle (Eds.), *The handbook of qualitative research in education* (pp. 597-641). New York: Academic Press.
- Di Leonardo, M. (1998). *Exotics at home: Anthropology, others, American modernity*. Chicago: University of Chicago Press.
- Dorst, J. (1989). *The written suburb: An American site, an ethnographic dilemma*. Philadelphia: University of Pennsylvania Press.
- Duranti, A. (1997). *Linguistic anthropology*. Cambridge, MA: Cambridge University Press.
- Eckert, P. (1989). *Jocks, burnouts, and others*. New York: Teachers College Press.
- Fabian, J. (1983). *Time and the Other: How anthropology makes its other*. New York: Columbia University Press.
- Fainstein, S. (1994). *City builders: Property, politics and planning in London and New York*. Oxford, UK: Blackwell.
- Foley, D. (1990). *Learning capitalist culture*. Philadelphia: University of Pennsylvania Press.
- Foley, D. (1995). *The heartland chronicles*. Philadelphia: University of Pennsylvania Press.
- Foley, D., Mota, Post, and Lozano (1977). *From peones to politicos: Ethnic relations in a South Texas town 1900-1977*. Austin: University of Texas Press.
- Fraser, N. (1994). Rethinking the public sphere: A contribution to the critique of actually existing democracy. In H. Giroux & P. McLaren (Eds.), *Between borders: Pedagogy and the politics of cultural studies* (pp. 74-98). New York: Routledge.
- Freedman, S. (1990). *Small victories: The real world of a teacher, her students, and their high school*. New York: Harper & Row.
- Freeman, D. (1983). *Margaret Mead and Samoa*. Cambridge, MA: Harvard University Press.
- French, T. (1993). *South of heaven*. New York: Doubleday.

- Geertz, C. (1973). *The interpretation of cultures*. New York: Basic Books.
- Gillin, J. (1951). *The culture of security in San Carlos: A Study of a Guatemalan community of Indians and Ladinos* (Middle American Research Institute, Pub. No. 16). New Orleans, LA: Tulane University.
- Gluck, S. and Patai, D. (Eds.). (1991). *Women's words: The feminist practice of oral history*. New York: Routledge.
- Goffman, E. (1961). *Asylums*. Garden City, NY: Anchor Books.
- Goldschmidt, W. (1950). Social class in America—A critical review. *American Anthropologist*, 52, 483-498.
- Gottdiener, M. (1997). *The theming of America*. Boulder, CO: Westview Press.
- Greenwald, M., & Bernt, J. (Eds.). (2000). *The big chill: Investigating reporting in the current media environment*. Ames: Iowa State University.
- Gregory, S. (1999). Globalization and the "place" of politics in contemporary theory: A commentary. *City and Society: Annual Review 1998*, pp. 47-64.
- Gupta, A., & Ferguson, J. (1997). Discipline and practice: "The field" as site, method, and location in anthropology. In A. Gupta & J. Ferguson (Eds.), *Anthropological locations: Boundaries and grounds of a field science* (pp. 1-46). Berkeley: University of California Press.
- Gutierrez, J. (1998). *The making of a Chicano militant: lessons from Cristal*. Madison: University of Wisconsin Press.
- Hall, S. (1996). The question of cultural identity. In S. Hall, D. Held, D. Hubert, & K. Thompson (Eds.), *Modernity* (pp. 595-634). Cambridge, MA: Blackwell.
- Hanson, S., & Pratt, G. (1995). *Gender, work, and space*. New York: Routledge.
- Haraway, D. (1988). Situated knowledges: The science question in feminism as a site of discourse on the privilege of partial perspective. *Feminist Studies*, 14(3), 575-599.
- Hart, R. (1997). *Children's participation: The theory and practice of involving young citizens in community development and environmental care*. London: Earthscan.
- Harvey, D. (1996). *Social justice and the geography of difference*. London: Blackwell.
- Hopkins, M. (1993). Is anonymity possible? Writing about refugees in the United States. In C. Brettell (Ed.), *When they read what we write: The politics of ethnography*. Westport, CT: Bergin & Garvey.
- Johnson, C. (1982). Risks in the publication of fieldwork. In J. Sieber (Ed.), *The ethics of social research: Fieldwork, regulation, and publication* (pp. 71-91). New York: Springer-Verlag.
- Kirsch, G. (1999). *Ethical dilemmas in feminist research: The politics of location, interpretation, and publication*. Albany: State University of New York Press.
- Kotlowitz, A. (1991). *There are no children here*. New York: Doubleday.
- Latour, B. (1987). *Science in action*. Cambridge, MA: Harvard University Press.
- LeCompte, M. (1993). Frameworks for hearing silence: What does telling stories mean when we are supposed to be doing science? In D. McLaughlin & W. Tierney (Eds.) *Naming silenced lives* (pp. 9-29). New York: Routledge.
- LeCompte, M. (1995). Some notes on power, agenda, and voice: A researcher's personal evolution toward critical collaborative research. In P. McLaren & J. Giarelli (Eds.), *Critical theory and educational research* (pp. 91-112). Albany, NY: State University of New York Press.
- Lefebvre, H. (1991). *The production of space* (D. Nicholson-Smith, Trans.). Oxford, UK: Blackwell.
- Lewis, O. (1961). *The children of Sanchez: Autobiography of a Mexican family*. New York: Random House.

- Lincoln, Y. (1990). Toward a categorical imperative for qualitative research. In E. Eisner & A. Peshkin (Eds.), *Qualitative inquiry in education: The continuing debate* (pp. 277-295). New York: Teachers College Press.
- Low, S. (1996). The anthropology of cities: Imagining and theorizing the city. *Annual Review of Anthropology*, 25, 383-409.
- Lukas, J. A. (1985). *Common ground*. New York: Knopf.
- Lutz, C. (1995). The gender of theory. In R. Behar and D. Gordon (Eds.), *Women writing culture* (pp. 249-266). Berkeley: University of California Press.
- Lynd, R., & Lynd, H. (1929). *Middletown*. New York: Harcourt, Brace.
- Marcus, G. (1998). *Ethnography through thick and thin*. Princeton, NJ: Princeton University Press.
- Marcus, G., & Cushman, D. (1982). Ethnographies as texts. *Annual Review of Anthropology*, 11, 25-69.
- Massey, D. (1994). *Space, place, and gender*. Minneapolis: The University of Minnesota Press.
- Mead, M. (1928). *Coming of age in Samoa*. New York: Morrow.
- Mertz, E. (1993). Learning what to ask: Metapragmatic factors and methodological reification. In J. Lucy (Ed.), *Reflexive language: Reported speech and metapragmatics* (pp. 159-174). Cambridge, MA: Cambridge University Press.
- Nespor, J., & Barber, L. (1995). Audience and the politics of narrative. In J. Hatch & R. Wisniewski (Eds.), *Life history and narrative* (pp. 49-62). London: Falmer.
- Nyden, P., Figert, A., Shibley, M., & Burrows, D. (1997). *Building community: Social science in action*. Thousand Oaks, CA: Pine Forge Press.
- Orlans, H. (1967). Ethical problems and values in anthropological research. In Committee on Government Operations, *The use of social research in federal domestic programs, Part IV* (pp. 359-366). Washington, DC: U.S. Government Printing Office.
- Plummer, K. (1983). *Documents of life: An introduction to the problems and literature of a humanistic model*. London: George Allen & Unwin.
- Portes, A., & Stepick, A. (1993). *City on the edge: the transformation of Miami*. Berkeley: University of California Press.
- Punch, M. (1986). *The politics and ethics of fieldwork*. Beverly Hills, CA: Sage.
- Radin, P. (1966). *The method and theory of ethnology*. New York: Basic Books. (Original work published 1934)
- Richardson, L. (1992). Trash on the corner: Ethics and ethnography. *Journal of Contemporary Ethnography*, 21, 103-119.
- Rollins, J. (1985). *Between women: Domesticity and their employers*. Philadelphia: Temple University Press.
- Rose, M. (1995). *Possible lives*. New York: Houghton Mifflin.
- Rose, M. (1999). "Our hands will know": The development of tactile diagnostic skill—Teaching, learning, and situated cognition in a physical therapy program. *Anthropology & Education Quarterly*, 30(2), 133-160.
- Ruddick, S. (1996). Constructing difference in public spaces: race, class and gender as interlocking systems. *Urban Geography*, 17(2), 132-151.
- Rutheiser, C. (1998). Beyond the radiant garden city beautiful: Notes on the New Urbanism. In *City and Society: Annual Review 1997*. Arlington, VA: American Anthropological Association.
- Shils, E. (1967). Social inquiry and the autonomy of the individual. In Committee on Government Operations, *The use of social research in federal domestic programs, Part IV*

- (pp. 359-366). Washington, DC: U.S. Government Printing Office. (Original work published 1959)
- Shulman, J. (1990). Now you see them, now you don't: Anonymity versus visibility in case studies of teachers. *Educational Researcher*, 19(6), 11-15.
- Silverstein, M. (1976). Shifters, linguistic categories and cultural description. In K. Basso & H. Selby (Eds.), *Meaning in anthropology* (pp.11-55). Albuquerque: University of New Mexico Press.
- Silverstein, M., & Urban, G. (Eds.). (1996). *Natural histories of discourse*. Chicago: University of Chicago Press.
- Simon, D. (1991). *Homicide: A year on the killing streets*. New York: Fawcett Columbine.
- Smith, D. (1974). Theorizing as ideology. In R. Turner (Ed.), *Ethnomethodology* (pp. 41-44). Harmondsworth, UK: Penguin.
- Smith, D. (1987). *The everyday world as problematic*. Boston: Northeastern University Press.
- Smith, D. (1999). *Writing the social: Critique, theory, and investigations*. Toronto, Canada: University of Toronto Press.
- Snodgrass, J. (1978, November). *The Jack Roller at 70: A fifty year follow-up of the delinquent boy's own story*. Paper presented at the American Society of Criminology Meetings, Dallas, TX.
- Strathern, M. (1991). *Partial connections*. Lanham, MD: Rowman & Littlefield.
- Szkut, J., & Reed, R. (1991). Community anonymity in anthropological research: A reassessment. In C. Fluehr-Lobban (Ed.), *Ethics and the profession of anthropology* (pp. 97-114). Philadelphia: University of Pennsylvania Press.
- Tobin, J., & Davidson, D. (1990). The ethics of polyvocal ethnography: Empowering vs. textualizing children and teachers. *Qualitative Studies in Education*, 3, 271-283.
- Tumin, M. (1952). *Caste in a peasant society*. Princeton, NJ: Princeton University Press.
- Urban, G. (1996). Entextualization, replication, and power. In M. Silverstein & G. Urban (Eds.), *Natural histories of discourse* (pp. 21-44). Chicago: University of Chicago Press.
- Vidich, A., & Bensman, J. (1968). *Small town in mass society* (rev. ed.). Princeton, NJ: Princeton University Press.
- Whyte, W. F. (1955). *Street corner society*. Chicago: University of Chicago Press.
- Whyte, W. F. (1964). The slum: On the evolution of street corner society. In A. Vidich, J. Bensman, & M. Stein (Eds.), *Reflections on community studies*. New York: John Wiley.
- Wiseman, F. (1971). Interview with Frederick Wiseman. In G. R. Levin (Ed.), *Documentary explorations: 15 interviews with film-makers* (pp. 313-328). Garden City, NY: Doubleday.
- Wolcott, H. (1973). *The man in the principal's office*. New York: Holt, Rinehart, & Winston.
- Zeller, N. (1995). Narratives strategies for case reports. In J. Hatch & R. Wisniewski (Eds.), *Life history and narrative* (pp. 75-88). London: Falmer.
- Zukin, S. (1991). *Landscapes of power*. Berkeley: University of California Press.
- Zukin, S. (1995). *The cultures of cities*. Cambridge, MA: Blackwell.

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