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On the Rhetoric and Politics of Ethnographic Methodology

By

JACK KATZ

In a variety of ways, all ethnographies are politically cast and policy relevant. Each of three recurrent political rhetorics is related to a unique set of fieldwork practices. Ethnographies that report holistically on journeys to "the other side" build policy/political significance by contesting popular stereotypes. Theoretical ethnographies draw on political imagination to fill in for a lack of variation in participant observation data and to model an area of social life without attempting to rule out alternative explanations. Comparative analytic studies build political relevance by revealing social forces that are hidden by local cultures. Each of these three genres of ethnographic methodology faces unique challenges in relating fieldwork data to politically significant explanations. By shaping the ethnographer's relations to subjects and readers, each methodology also structures a distinctive class identity for the researchers—as worker, as aristocrat, or as bourgeois professional.

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S peaking of the ubiquitous relevance of legality, Vilhelm Aubert once remarked that even to assert authoritatively that "there is no law on that" is to make a significant legal statement (cf. Aubert 1983, 77-78).[1] Acting in an area not governed by law means you have a certain freedom to proceed and a certain vulnerability to be attacked. As with law, the hermeneutics of politics stop at no limits. (It is revealing to note that the same holds for religion and psychoanalysis [Ricoeur 1970].) To characterize a piece of ethnographic research as apolitical is a political statement.

Jack Katz is a professor of sociology at University of California, Los Angeles. His current research is on the intersections of work, household formation, and local area use in six neighborhoods that represent the range of social and ethnic stratification in Hollywood. His Web site is http://www.sscnet.ucla.edu/soc/faculty/katz/.

A debate over whether research should be "policy relevant" has a false tone, in part because policy relevance is just one form of politics. It is hard for students of the collaborative construction of social facts to resist asking the following: relevant to which policy makers, in what ways, and the most difficult question of all, when? "Policy relevance" is an indirect way of demanding that political priority be given here and now to those with at least a foothold in institutions of power.

A debate over whether research can expect to find timeless laws can also be misleading, at least when the subject is ethnography. Railings against "positivism" tend to forget the pragmatic logic of methods. People who search for timeless laws do not necessarily believe they will ever find them. This appears absurd only if we forget that in the quest for perfect, certain, or universal knowledge (Turner 1953), one does different things than when ambitions are more limited. The quest for timelessly applicable forms of theory, like the quests for love, peace, equality, or god, leads one to do different things, and while arriving at a settled end may be fantasy, the challenges structured and overcome on the way are not necessarily quixotic.

Consider the romantic logic that organizes the work lives of our practical-minded colleagues, the survey researchers. It is always absurd to conduct a random sample to generalize to a sampled population since even if generalizations are qualified with probabilities stated in the timeless standards of quantitative logic, the population is never the same, in composition or at least in biographical reality, as the population to which the study's results will be extrapolated. Between any study and the application of its results, there is always the wild bet, always made against a better wisdom, that things do not change. But on the way to the survey researcher's absurd quest for generalizable certainties, some very real dragons of competing explanation can be slain, or at least seriously injured.

Several critical issues easily can be masked in a debate between advocates of neutral and comparative analytic language and advocates of research cast in language that will be perceived as relevant for assessing the impacts of power. By critical issues, I mean something empirical: whether self-consciously or sentimentally, every ethnographer will define his or her position on each of these issues in each research project; the stance taken will have major consequences for the demands of the research project. A central choice is one of genre. This article is essentially a discussion of the three dominant genres in sociological ethnography, each of which builds political significance for a text in distinctive ways. Within each, there are politically significant choices, but the challenges differ.

First, ethnographies can be made policy/politically meaningful by presenting a picture of social life that is juxtaposed to common stereotypes. I refer to this first genre as "worker" ethnography. To develop a politically powerful juxtaposition, the ethnographer operates in the field as a novice jack-of-all-trades, laboriously detailing varied regions of subjects' lives through relatively unspecialized description. Humbling his or her authorial posture, the worker ethnographer maintains a transcending respect for the subjects, who are rendered as fully human beings. Even through self-reflexive passages, the subjects remain at center stage in the text. This is the tradition started by William F. Whyte's *Street Corner Society* (1955) and continued by Gans, Anderson, and Duneier.

I label a second genre "aristocratic": the researcher either does not spend much time with subjects, avoids the drudgery of repeatedly describing everyday events, or at least fails to present in the text empirical materials showing variations in the lives of subjects that are directly relevant to the theory offered. While this kind of ethnography can make a significant contribution at certain points in the history of research on given types of social phenomena, the rhetorical strategy is usually to provide a flat, unvaried, morally sympathetic but relatively superficial picture of subjects in order to cast them into illustrations of theory. Begun by Radcliffe-Brown, whose structural-functionalist ethnographies came to be seen as politically conservative, the leading current examples of this tradition include the anticapitalist ethnographies of Burawoy (1979; Burawoy and Lukács 1985; Burawoy et al. 2000) and, in its most powerfully rhetorical aspect, the enormously popular, feminist/anticapitalist ethnography of emotional labor by Hochschild (1979, 1983).

The third genre, which I call "bourgeois professional," is the least formally political. The ethnographer works as a specialist studying a kind of social process, constructing and analyzing series of cases that show fine variations between similar events, biographies, and types of social action. Like a dentist, each "case" has a set of X-rays taken at more or less analogous times; because the dentist cannot control if and when patients will come in, some files are more or less complete than others; and a craftlike expertise is required to make sense of what will often be invisible to lay viewers without professional instruction. The resulting text becomes political only through conveying the indirect and subtle message that local culture obscures how universal social processes shape local life. For the bourgeois professional ethnographer, the key issues for defining the political significance of a study are which spatial and temporal dimensions of a social phenomenon to include in the composition of the set of gathered or constructed cases. Research projects in this genre are distinguished by the creation of sets of closely related data. The researcher amasses situationally specific observations of behavioral interaction and diachronically described cases, such as the biographies of collective phenomena (e.g., riots), natural histories of work careers (e.g., the public school teacher), the stages through which a given type of conduct is built up (e.g., opiate addiction), and status passages in personal life (see Vaughan on uncoupling, following). The first flourishing of this style of ethnography came in the 1940s and 1950s, in Second Chicago School studies (see Fine 1995).

Prologue: The Social Construction
of a Text's Political Status

Ethnography is a relatively mushy field on which to hold a fight that pits advocates of neutral language against those favoring policy, political, or morally marked analysis. In the conduct of fieldwork, methods and theory interests are so closely mixed with each other and with historically and socially contextualized relevancies that neutrality is relatively hard to come by. The argument can be held more clearly

in fields in which methods are more fixed relative to substantive focus and where theory is prima facie neutral (exchange theory, status expectations theory, network theory, conversation analysis, etc.).

Consider a study of conversation-analytic practices as demonstrated in transcribed recordings of interrogations of complaining victims in rape trials (Drew 1992). The author uses this corpus to analyze how prosecutor and witness contest each other's descriptions of legally significant events, but he draws no implications about the justice or injustice in which violence against women is either initially performed or subsequently administered legally. The issues addressed are technical; the themes are about court versions of universal conversation-interaction processes. That the substantive material is about rape trials is of no noted relevance.

I mean not to judge such work but to highlight the moral/political challenge it presents to the researcher. As an ethnographic parallel, one might imagine a study of the table manners of SS guards at extermination camps that made no analytic relevance of the context of their dining. At some level, that is what Norbert Elias (1994) indirectly did in his historical ethnography of "the civilizing process."

The bitterness of the irony in Elias's work, published in exile in 1939, his parents dead at Auschwitz and Breslau soon after, is transformed if not eradicated by the publication some sixty years later of an analysis of the de-civilizing processes that lead up to the Nazis (Elias, 1996, esp. 299-402). The earlier, morally neutral text is qualified and recast as a foundation for a comprehensive moral appreciation of the relationship between everyday modern culture and the structure of national political power.

All of this poses a question that is too often ignored in facile rejections of technical, policy silent, universally cast analyses: who determines the context in which a work should be read? Becker's (1953) marijuana-user paper has a bit of a protest against psychological explanations, but it reads primarily as a learner's manual, consistently dry and matter of fact, without tones that either hector or proselytize. But that is not how it ever has been read. Does a study of the interaction tactics at rape trials, which never mentions that cross-examination techniques incidentally compound the assault on a victim's subjectivity by attempting to thrust undesired meanings into her mouth, show a commitment to positivist science carried to amoral madness? What about a study showing table manners being honored a mere matter of feet beyond a view onto grotesque horrors? Seen in the right context, the latter becomes a study of how people collaborate to sustain identities as good people while doing dirty work (Hughes 1962).

In an important, neglected sense, the critic who would damn these studies as amoral covers up his or her failure to import the context that would bring out the political significance. Reading Elias on table manners on its own carries one line of political implications; reading it as a precursor to *The Germans* (1996) gives another spin. And before Elias published *Studien über die Deutschen*, what was the political status of his earlier work? That depends on whether the reader is willing to bring the substance of Elias's later work into the discussion before Elias does. The same is true with Drew (1992) and cross-examination practices at rape trials.

The study can be a resource for damning Drew's moral indifference or for using Drew to damn the way legal process compounds victimization.

To return to the initial discussion (by Becker et al., in this volume) of Erving Goffman's *Asylums* (1961), which is conceded by all sides to be both theoretically and policy/politically significant, we should note what an odd type of ethnographer he was. None of his writings have ethnography's usual monograph-like focus on people in a single organization, time, or place. All of his writings show the workings of a fundamentally simple, if extraordinarily agile, humanist mind: he takes the culture of a given institution (prison, con man's world, gambling, theater) and, with undaunted chutzpah, applies it to a range of institutions with mutually segregated cultures and distinctive languages for understanding themselves. What is locally grounded culture in one institutional setting becomes metaphor when applied to another. Something similar was the key to Georg Simmel's great fertility and was evocatively transmitted from Simmel through Park to Goffman's teacher Everett Hughes, who, as Hughes's students have reminded us, would make deliberately provocative comparisons, for example, of doctors and prostitutes as sharing intimate craft knowledge in body work.

Goffman provides an especially instructive example of the complex relations between ethnography's policy/political relevance and its use of theoretical language that transcends substantive relevancies as recognized in local institutions. In most of his writings, Goffman was not demonstrably concerned with policy or practical consequentiality. And yet *Asylums* has been directly relevant to policy formation. It has been cited prominently in judicial considerations arguing for the limitation of government powers over confined populations (see Justice Brennan's dissent in *O'Lone v. Estate of Shabazz*, 107 S.Ct. 2400, 1987). As a radicalizing perspective on the institutionalization of subordination, it also preceded, if it was a not a precedent for, the critique of modern power that became associated with Michel Foucault (1979). Before Foucault, Goffman used the prison as a metaphor for understanding the workings of power in a variety of institutions, some of them work institutions, some religious, some military. This line of critical perspective, which more or less directly challenges the distinctive cultures of each substantive institution as a misleading cover for the workings of oppression, continues in the more ambivalent political stance taken by Bruno Latour in his sometimes ethnographic, sometimes highly abstracted texts (Latour and Woolgar 1986; Latour 1993, 1999).

The example of Goffman's work points to three distinctions useful for this discussion. There is the distinction drawn by Becker et al. (in this volume), in which Gans advocates research that expresses policy/political relevance as defined by the culture currently maintained by a given substantive institution. This includes those within its central organizations and the constituencies regularly or irregularly interacting with it. Becker appreciates policy/political relevance as it appears from a perspective that transcends any one institution's culture, a perspective that uses neutral language to find commonalities across social settings segregated by distinguishing cultures.

There is a second distinction, between potential and realized policy/political relevance. Goffman gave a new political relevance to a series of ethnographies (and novels, diaries, and biographies) that, for their creators, were limited in focus to a given substantive institution. And much of Goffman's own work remains apolitical and irrelevant to policy concerns today, but only because the necessary further steps have not been taken. For example, consider his extensive studies of behavior in places that are "public," not in the sense of ownership but in access. Once this field of study matures to the point of analyzing comparatively the structure of social

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process in the uses of public spaces managed by government (parks, beaches, museums) and public spaces managed by capitalists (restaurants, malls, museums), it is likely to lead to critiques of current public policies, which subsidize some spaces and not others. What the government does in subsidizing some spaces and not others is not only allocating public goods to the use of some populations and not others; it is also shaping patterns of segregation and integration in the histories of users' lives. If we compare interaction on site with users' patterns of social interaction in their off-site social lives, we are likely to find that some public spaces separate people who reside and work in proximity, while others throw types of individuals whose lives otherwise have no points of contact into common interaction arenas. It is not obvious that publicly managed public space integrates while privately managed public space segregates. (For some scraps of research in this direction, see http://www.sscnet.ucla.edu/nsfreu.) Research might tell, and when it does, Goffman will have taken another great stride toward becoming a policy researcher.

And third is the important distinction between the self-conscious purpose of research and its social significance. Goffman was above all intellectually playful. In a way familiar to much childhood play, he would make believe that social life in one area was governed by the forms and processes of social life in another. If brought off with the appropriate élan, such play can generate a veneer of charm that covers its fundamental disrespect for the authoritative boundaries of institutional culture.

The resulting text does not necessarily look political, much less radical, even though in the resources it provides for critique, in its potential, it is. Motive is important but not the political motives we are used to discussing. The key motive behind the distinctively sociological comparative analysis that characterizes a line of research running from Simmel through Hughes to Goffman, Becker, and now, Latour, among others (I would add de Certeau, but that is another line), is a celebration of the intellectual freedom involved in the distinctively sociological game of taking one institution's serious version of itself and insouciantly using it as a metaphor for undermining the self-proclaimed uniqueness of another.

Goffman's work shows the distinctive policy significance of using a uniquely sociological perspective that overrides what is currently considered relevant to policy formation. It also shows something that should surprise no ethnographer: that the making of policy or political relevance is the product of collaborative action. Work done in an emphatically bourgeois spirit celebrating the freedom to pursue intellectual fun can become powerfully political, depending on what others, in other research projects and in positions of power, do with it. Conversely, work done in a serious spirit of political relevance and with marked policy language will often be relevant only to the collectively related careers of other politically self-defined academics. After all, if we have reason to mistrust the cultures in which institutions proclaim their values and concerns, should we not also mistrust the culture of self-proclaimed policy and political relevance?

The Three Classes of Ethnographic Work in Sociology

At least three rhetorical strategies for claiming general significance for an ethnographic case study compete for researchers' affiliations. Each rhetoric draws in a different way on theory and political sentiments. Virtually all ethnographies can be located within these three types, although texts occasionally combine genres. Each genre can be used well or poorly. The three-class system used below is not intended as a simple rating device but as a way of focusing on the distinctive choices within each genre.

1. Worker ethnographies of the "other side"

First is work that documents social life in a given time and place, and within local terms, as its central contribution. (For leading examples from the current generation of ethnographers, see Duneier [1999] and Anderson [2003].) Displays of what life is like on the "other side" of mythological projections are a mainstay in ethnography's warrants (Katz 1997). In this genre of ethnographic work, policy relevance is built into the juxtaposition; what power misperceives, it is unlikely to govern well. Generalizability is essentially of the sampling not theoretical variety. That is, the people described at a given time and place are offered as representatives of a type

of person, as addressed by public commentators, politicians, administrators, and academics: the homeless, crack users, gang members, men who hang out on ghetto street corners. Theoretical significance is often tacked on as protective bookends for the text, but the claim of significance for the study rests most firmly on the juxtaposition between the social realities documented by the ethnographer and those held to be true by people in power.

To provide a compelling juxtaposition to common stereotypes, it is essential for the researcher to operate as a relatively humble jack-of-all-trades, going along with subjects where they may travel, entering novel situation after novel situation with the anxiety of the novice, and displaying an unusual intimacy with the darker corners of subjects' lives. The text strives to present a picture of subjects that differs substantively from what some large segment of social thought and popular culture imagines to be the case; for rhetorical effectiveness, the text must also display a picture of subjects that is more rounded than stereotyped and less obviously touched up or "spun" than political commentary. The result is a diverse and relatively unspecialized set of data. The data are then sorted into chapters to depict sectors of a lifeworld in a way that always keeps the subjects whole as opposed to exploiting them as bearers of politicized categories.

Commonly, no effort is made to document or to analyze existing data to see whether the phenomena studied took the same or different forms in different social conditions. While this has long worked well to sustain a market for ethnographic research, there are dangers on this essentially theory-weak path to claiming significance. Here is an example from contemporary gang criminology.

Gang researchers create a model for the gang they study, without reconciling contradictions apparent if one reads other gang studies. For example, midwestern gangs are attributed to "rustbelt" realities of "deindustrialization," while even more populous, contemporaneous Latino gangs in the economically expanding Southwest are attributed by another researcher to their presumably unique "multiple marginalities" (for a detailed discussion, see Katz and Jackson-Jacobs 2003). Policy relevance depends on a commitment not to research comparatively.

Given the costs of immersion for ethnographers, a single geographic site is most common. A local focus also works well in the United States in part because state and local jurisdictions are pressed politically to find antigang policies, and they can independently fund social intervention solutions that make sense within local culture. Research monographs are offered in support of favorable stereotypes that are used to justify ameliorative policies. Stereotypes about causes of social pathology are locally shaped to fit local cultural realities. The Midwest knows it is a rustbelt; the Southwest knows that gangs are endemic to Latino culture. In each area, a different stereotype is used to fund antigang social programs. The contradiction remains inchoate. News, entertainment, and academic institutions ensure that every area has an unshakeable investment in a unique local culture, within which such mutually incompatible explanations resonate well and thrive (on contradictory news portraits of similar metropolitan crime realities by New York and Los Angeles media, see Katz 2003). Milwaukee residents do not vote in Los Angeles;

Los Angeles residents do not vote in Milwaukee. Local advocates virtually never contest these inconsistent parochialisms.

2. The aristocratic posture:
Singing theory versus documenting variation

A second strategy for claiming significance for ethnography puts extraordinary weight on theoretical discussions that resonate with moral and political sentiments compelling to academic sociologists. Michael Burawoy's (1998) is the most notable brief for this position. What Burawoy terms the "extended case method" should be understood as a rationalization for declining to do the work of extensive, in situ description or, if that work is done, to decline to present in texts descriptions of empirically documented variations of the explanatory ideas and/or of the matters to be explained. Instead, one predefines a problem to be researched from within academic debates and interprets the meaning of fieldwork encounters within that framework. The case is "extended" not by documenting the biography of the empirical cases studied, which was the original meaning of the concept (see Gluckman 1961, 1967), but by interpretively linking theoretical characterizations of field encounters to "macro" themes, which are referenced not through original data gathering but, at best, through readings.

I label this posture aristocratic because, besides demeaning the work of writing field notes that reflect members' meanings and incorporating them intact into the text, it proceeds from and constructs a position of privilege and power for the author relative to both the research subjects and the reader. One of the hallmarks of this posture is that it opposes the emphasis in "grounded theory," and more broadly, in the interactionist tradition of social research, by warranting a disregard for documenting social realities as experienced in situ by the people studied. Often this takes the form of a researcher's finding "problems" through a debate within the academy, through "theoretical considerations," and thus justifying overriding evidence that the people studied do not define their situation as problematic or that they define their situation as problematic in ways the researcher ignores. While the worker and bourgeois professional ethnographer humbles himself or herself to shape explanatory categories to fit what the people studied experience, the theory-informed ethnographer knows better. This posture inevitably leads to an assertion of false consciousness made from a position of presumptive superiority: what the people studied define as their reality itself is a product of powers they fail to appreciate.

Now, nothing is inherently wrong with forming initial definitions of problems from within academic debates, nor even with concepts of false consciousness. In the final analysis, few sociologists get by without resorting to notions of false consciousness or its rhetorical alternatives, such as subconscious meaning or latent function. And as members of society, we have all had the common experience of turning on our lives to realize that previously undiscovered forces were shaping us in ways we failed to appreciate. The problem is not with theory; the methodological

problem of false consciousness arises when the researcher uses it as a basis for not describing and textually presenting descriptions of how the people studied in fact live and understand variations in the situations of their everyday lives. Whether or not the case is "extended" through theoretical discussion, the key issue is pragmatic, whether the reader is disempowered by presumptive interpretation or is enfranchised to participate in the discussion by being given access, to the extent the researcher can provide it through quotations and in situ field notes, to the subjects' realities as they experience it.

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Feminist studies of beauty and appearance cultures, for example, can be handled in more or less aristocratic/democratic fashion, depending on the extent to which the researcher honors the concerns of subjects at least enough to present to the reader extensive data on the situations in their social worlds in which appearance makes a difference in the subjects' own experience and behavior. But the choice is real. I venture to suggest that virtually every U.S. graduate sociology research department has had multiple experiences of personal crises as M.A. and Ph.D. students, who typically seek to identify morally and emotionally with the people they study, discover an importance of cosmetic culture to black and Latina working-class women, which they describe in empathetic detail only at the risk of making their academy-based theoretical presumptions about the oppressive weight of appearance culture appear privileged and denigrating.

When texts fail to present data describing situated conduct as experienced by the people studied, another aristocratic feature is built into the ethnographer's posture: key explanatory categories become ambiguous, leading to an impairment of the reader's ability to understand, much less define, evidence that would directly counter the proffered explanation. In Michael Burawoy's studies of worker "consent," a label he derives from Marxian theory, it is never clear what this key phenomenon is. While he consistently takes consent as the matter to be explained, consent is not translated into indicators of strike activity, union militancy, tenure on the job, or even worker output.

In the 1970s, Burawoy entered a machine shop to collect field data for his dissertation. He came to realize that he had landed in the same machine shop that

Donald Roy had studied thirty years earlier. Roy sought to understand a variety of enigmas he found in his and his coworkers' behavior. Roy phrased the issues in commonsense terms about an observably varying explanandum. Why did they work hard at some times and hardly at all at others (Roy 1952)? They did not seem to be seeking to maximize their incomes, so what were their motives (Roy 1953)? Why were they intimately friendly to each other at one time and then in bitter conflict at another, and then friendly again (Roy 1959-1960)? Roy focused on situational variations in work effort and in the dynamics of workers' small group cultures. He created extensive, detailed data sets on the work output of different workers at different times and then took those differences as matters to be explained. Burawoy phrased his problem in a more singular, theory-derived, and academic fashion, taking workers' consent to labor, but no particular variation in their conduct, as the matter to be explained.[2]

It turned out that the level of worker output that Burawoy found in the 1970s was the same, he reports, as Donald Roy found his study in the 1940s. In both periods, workers sought to "make out" through a gamelike strategizing. Burawoy does assert changes in how workers were supervised, in how their pay was linked to their output, and in the direction of their conflicts, which he says turned from management toward each other. But in the sole description of output he provides as a comparison with Roy's extensive descriptions, he states,

> Their average "measured performances" for the entire year [referring to sixteen radial-drill operators] . . . were as follows [giving figures]. The average was 120 percent, which turns out to be precisely Roy's average in his second period. . . . The data do not suggest significant differences between the rates on radial drills in Geer's Jack Shop [Roy's site name] and on radial drills in Allied's [Burawoy's site name] small-parts department. (Burawoy 1979, 227, note 17).

Moreover, just as Roy found workers gaming around production quotas, Burawoy, searching to explain the mystery of workers' consent to work, found that "game-playing generates consent to the social relations in production that define the rules of the game" (Burawoy 1979, 82). In process and result, consent to labor remained essentially constant. Where is the variation that Burawoy would explain?

Burawoy claims that between the 1940s and 1970s, changes in the structure and managerial style of capitalism had an impact on consent, but the reader will struggle in vain to find descriptions of variation in the matter explained that correspond to differences in the explanatory categories.[3] Further ambiguities turned up when after working for a short stint in what he characterizes as an analogous machine shop in socialist Hungary, Burawoy reported that the output was much the same as he and Roy had independently found in Chicago (Burawoy and Lukács 1985). What, then, do changes in twentieth-century capitalism or in the contrast between capitalism and state socialism explain? Perhaps they explain the managerial style of relating to workers. But if managerial styles differ by political economy without affecting worker productivity, then capitalism does not exploit workers, at least not in the sense of extracting a greater value or output from them. (Notably, there is no serious argument that the Hungarian workers were better compensated than the

U.S. workers.) This minimizes Marxian theory to a brief for human relations at the workplace.

This would be fine, at least as a matter of logic, if managerial style were what Burawoy wanted to explain. But the theoretical excitement of his research comes from the promise to explain why workers go along with oppressive conditions. One way of reading Burawoy's 1974 study is that it was unnecessary: Roy had already documented variation in worker activity and attitude and had provided an explanation fit to the empirical variations he describes. Imagining this objection,[4] Burawoy provides a rhetorical rather than empirical answer. While Roy tried to explain "why people don't work harder," Burawoy tried to explain "why people work so hard" (Burawoy 2003, 654, note 9). The distinction is an esoteric version of half empty versus half full.

> I take as a point of departure the possibility and desirability of a fundamentally different form of society—call it communism, if you will—in which men and women, freed from the pressures of scarcity and from the insecurity of everyday existence under capitalism, shape their lives. . . . It is in terms of this possibility . . . that Marxists interpret the present and the past. (Burawoy 1979, xiii).

This imaginary invocation of differences in the thing to be explained, and in the explanatory conditions, might be termed *theory singing*. The ethnographer acknowledges that explanatory logic requires arguing a relationship between variations in explanandum and explanans, but that variation is supplied by "theory" not by any data describing variations in the explanandum or explanans. The power of theory, and of a theory club in sociology, is essential for this rhetoric to work. The result is reminiscent of the emperor's new clothes: readers who are haunted by their inability to perceive what it is that is being explained simply show their lack of initiation into the power club. They lack the right sensibility or, to echo Bourdieu, theoretical taste.

Another way ethnographies take on an aristocratic posture is by failing to offer materials that readers may use to develop and test ideas that were irrelevant to the author. When worker ethnographies are well done and when dossiers on individual biographies or types of social action are carefully assembled, an ethnography's readers can exploit the text's data without concern for, much less a show of obeisance to, the author's original purpose. (As noted above, Goffman's writing was full of such creative reuses of others' ethnographies.) These are relatively humble styles of work, the worker ethnography being the most malleable in the hands of subsequent users, in that they do not insist that the reader seek admission into the author's intellectual world to find value in the text. A common feature of the aristocratic style of ethnography is that the reader cannot see the subjects except as already dressed in the author's theoretical categories. There are few or no indented paragraphs or extensive quotations that show the reader what the researcher heard and saw in the field. Colloquialisms and situationally nuanced detail are absent. The reader must, in effect, accept the disciplinary guidance of the author to obtain any glimpse of the subjects. (For an extreme example, see Jankowski [1991], who

gives an extensive tour of reputedly dangerous worlds in which subjects can never be seen up close, heard in their own words, or otherwise appreciated outside the analytical lenses that the tour guide insists readers wear.)

   Finally, the aristocratic style in ethnography can be identified by an exclusive focus on a preferred explanation. Alternative explanations are simply not considered or not considered seriously. The choice not to consider alternative explanations is rhetorically effective only if the text does not show variation in the phenomena to be explained. Put conversely, the author's failure to present variation in the explanandum is rhetorically obscured by the elaboration of a single explanation, for which the description of invariant phenomena provide illustrations.

The worth of a study should not be assessed outside of a triangular appreciation of the empirical relationship between author, subjects, and readers.

   An example is Arlie Hochschild's (1983) book on the production of emotional displays by workers, especially female workers, as scripted and supervised by capitalist-controlled service firms. (Many of the following points have been made before [Smith-Lovin 1998].) Hochschild's book is a complex, and for that reason, a more informative, example of the aristocratic style in ethnography because it shares features with what I term the professional bourgeois style. Drawing on a wide range of interviews that she conducted and on a wide range of descriptive writings by others, Hochschild gathers a large, richly varied set of descriptions of situationally specific instances in which people interpret their emotions either as compelled by others or as authentic. Some of the situations are from personal life (e.g., romantic relations); some are from a variety of work settings; some cover the training of actors. On the basis of this varied data set, Hochschild develops concepts about "feeling rules" and "emotional labor" that are extensively grounded in her data. In an appendix, she relates the understanding of emotional experience and interaction that she develops through her data to the history of the study of emotions, from Darwin through Freud to Goffman.

   Left to these conceptual contributions, the book would have been a valuable offering to the sociology of emotions, on the order of Candace Clark's (1997) study of expressions of sympathy in everyday life situations. Analytical tools are offered to subsequent emotions researchers in a form that is accessible and that promises to "cut at the joints of experience," to use William James's phrase, or that offers other

researchers "sensitizing concepts," to use Herbert Blumer's phrase. But Hochschild raises the book's claims to a significantly grander status by developing a political/morally righteous theory that disdains capitalist-enforced demands for emotional labor by middle-class workers such as Delta Airlines flight attendants. The key to the great success of this project is the author's construction of a righteous sensibility that condemns corporate-enforced emotional labor in a withering regard.

What makes the study aristocratic in its posture is the refusal to be disciplined by the data. Again, the key failure is in documenting variations in the phenomenon to be explained. Hochschild claims that capitalist-institutionalized demands for emotional labor are broadly damaging to the middle-class workforce because they undermine the "signal" function of emotions: over time, the worker loses the capacity to interpret her or his emotions as self-indicating because the worker's emotions are in effect owned by management, which insists on a positive emotional expressiveness toward service clients. Hochschild massively documents the explanans: corporate demands that flight attendants display positive emotions. But the evidence for the existence of the damage claimed is meager and weak. Here and there, flight attendants are quoted in brief references to the negative effects of their emotion work (e.g., Hochschild 1983, 4), but as many or more data strips show positive effects, and many other strips show negative effects, or self-alienation from emotion, at nonwork sites (e.g., weddings).

It is telling that the most powerful data passage that indicates psychological damage to flight attendants from their emotional labor comes in the classic style of aristocratic ethnography, through the commentary of a headman, or in this case, headwoman. A sex therapist said to have fifty flight attendants as patients reports a pattern of "loss of sexual interest" and "preorgasmic problems," stating, "They hold onto their orgasmic potential as one of the few parts of themselves that someone else doesn't possess" (Hothschild 1983, 183). Scattered throughout the book, there are several passages in which passengers are described as abusing flight attendants, for example, spitting on them. But in this book of some 300 pages, a single sensational quotation from a sex therapist is virtually the only data indicating that emotional labor has negative aspects that endure situational ugliness.

We can see here the rhetorical relationship between the failure to present data describing variations in the explanandum and the failure to discuss alternative explanations of the data illustrating the explanandum. We learn at one point in the text that Delta management recruits for certain flight-attendant personalities. Whatever sexual problems these employees may have, there is no evidence they emerged after their employment began. There is no discussion of the possibility that whatever emotional problems flight attendants may have, they may have preceded working for the airlines, much less that they might even have been greater in scale. There is no discussion of the possibility that women who are similarly situated to the flight attendants but do not take emotional labor jobs may suffer greater emotional self-alienation. With respect to the startling figure of a caseload of fifty sex-therapy patients from one airline, there is no consideration that the therapist may build her practice through a referral network specific to flight attendants and

that even more disturbed women, located in more isolated settings, suffer without treatment. There is no effort to provide evidence, or even discuss the possibility, that young women may seek therapeutic help at rates relatively high to males of the same age, regardless of employment status.

The political/moral thrust of the book rests on a series of empirical claims that remain more implicit than explicit. These include that service work increases the pressures on workers to manifest emotional expressions as opposed to the demands in off-work social life or nonservice work (were women better off when they were seamstresses in sweatshops?); that women face this pressure more than do men (what about Willy and Biff Loman?); that large-scale capitalism, especially the impersonal, multisited corporation, creates this pressure more than do alternative political-economic systems; and that management perverts, distorts, undermines, pollutes, alienates, or otherwise negatively affects workers' emotional makeup by demanding and disciplining emotional expression. We hear virtually nothing of the emotional lives of women, or men, at working-class jobs that lack a personal service component, or who are primarily homemakers, with the exception of a few passages that indicate that emotional self-alienation is also part of some nonwork, intimate relations. What we do not see includes descriptions of the psychological makeup of flight attendants when they start Delta jobs; descriptions of similar women who work for pay as lonely writers at home; materials on mothers who feel injunctions to maintain enthusiastic, positive interactions with their young children; descriptions of social interaction among men in machine shops, where emotional display to colleagues may be intensely scrutinized and frequently tense (Roy 1959-1960); and descriptions of professional and managerial men at home and at work. On the latter, I note that for eighty years, *The New Yorker* has run cartoons depicting executive-type males, and now, occasionally females, absurdly using domestic emotions at work and treating spouse, child, or pet like a subordinate employee at home.

Note the relationship between the lack of documentation of emotional problems increasing as women become flight attendants and the most subtle resonances of the book's theory singing. The failure to document biographical change for the worse among emotional laborers is a relatively minor problem. The greater problem is the unstated but constant inference that nonservice work, or nonemotional labor, or not working at all, is less emotionally self-alienating. Like Burawoy's imagination of a communist utopia but more in the style of a Chekhovian dreamer idealist than militant Marxist, Hochschild implies but never asserts the possibility of an alternative world of plentiful, wondrous orgasms.

In the style of aristocratic sensibility, Hochschild's argument sets up its own feeling rules. A powerful message conveyed to the huge masses of undergraduates who have been taught this book is that going along with corporate injunctions to smile is to betray one's colleagues. Flight-attendant unions, we are told, sometimes bargain with smiles, which they withhold until pay and working conditions improve (Hochschild 1983, 129). To smile on command, or even to come to experience emotional labor as natural and pleasant, is to sustain the management system that wrecks the psychological makeup of masses of other employees. This elegantly

written book professes a superior moral perspective that instructs its young adult readers on the feeling rule that they should question their feelings, even when they seem otherwise unproblematic, if they find themselves in employer-demanded forms of emotional labor. I wonder if emotional labor reaches into tender souls more powerfully than does academic teaching by model mentors.

Hochschild does not limit her analysis or theory to the data she actually has accumulated. She will not be disciplined or humbled by her empirical materials; the successful aspirations of this ethnography are to a much higher and intellectually free position, one from which data are attended to primarily to discipline them into the outline of a transcending moral power. Instead of showing before-and-after or synchronic comparative descriptions of people in and outside of emotionally scripted service jobs, Hochschild sings theory by invoking a category, "deep acting," which conveys the idea that after enacting superficial emotion scripts long enough, their artificiality must reach deep into the soul.

As I stressed in the prologue, the worth of a study should not be assessed outside of a triangular appreciation of the empirical relationship between author, subjects, and readers. My point is to bring out the rhetorical relationship between monolithic theory singing and the presentation of data showing only constant rather than varying forms of key explanatory categories. Reading *The Managed Heart* in its historical context, my evaluation is uneasy but, on the whole, favorable. When first published, the book, and the journal article that preceded it, gave much-needed direction to countless researchers who wanted to study the intersection of gendered identities, the discipline of the workplace in a changing economy, and multinational capitalism. Emotional labor, feeling rules, and emotion scripts became indispensable descriptive tools. But to continue to sing the book's theory some twenty years later—that is, to use theory to elide the challenge of describing relevant patterns of experience over biographical time, home, and workplace and gender identity and occupational status—is increasingly to rely on the power of moral/political righteousness and its academic club to blunt criticism and to block research progress.

3. Analytic induction and bourgeois professional ethnography

Studies in the genre of journeys to the other side rely on political sentiments in stereotypes and powerful myths to give juxtapositional significance to the cases they detail. Theory singing leans on politically charged characterizations to evoke the variations necessary for a sense of explanation and to invite sympathy for side-stepping challenges to document and present data that would rule out plausible rival explanations. A third general ethnographic strategy for relating policy or politics and theoretical language puts data variation and emergent analysis at the center of the project.

Comparisons most vigorously test explanation when they are developed in data describing given cases as they change over time in multiple social contexts. The rhetoric of methods may be "constant comparative," "grounded theory," or simply, the familiar language of causal explanation's necessary and sufficient conditions

(for a straightforward example of the last, see Newman 2004). As a matter of convenience, I will refer to this genre in terms of its most explicit explanatory form, as "analytic induction" (Katz 2001).

While any ethnography is likely to refer to its database by noting the raw amounts of field notes collected, the number of interviews conducted, and the time spent in the field, professional ethnography is the only style that can be said to work with the more or less self-conscious concept of the data set. A data set is an analytically formed collection of descriptive materials, typically organized around a given type of social situation or a given type of social process. Perhaps the first data set constructed in this style consisted of observations of medical-student perspectives (Becker et al. 1961); each observation is given equal or weighted value in relation to each other. There is a separable, explicit effort to create quality in the data set, independent of the substantive explanation. Contextualized, situated descriptions, for example, are treated as more probative than are descriptions of opinion or attitude that do not describe the context in which the opinion emerged.

I use the term *bourgeois professional* because the constitution of the data set is a task not unlike what dentists, lawyers, accountants, and others do in occupations focused on "cases," each of which has certain common features. For ethnographers, the common features are likely to be incidents of behavior that are situationally contextualized, in the sense of being described as occurring within an immediate context that is relevant to the actor's organization of conduct; descriptions of the stage in a larger process within which any described scene or act occurs; and descriptions of the social interaction within which the act or action emerges, meaning a description of how, in shaping his or her conduct, each person is taking into account the likely responses of others present and anticipated.

We can distinguish bourgeois professional ethnographies by the complexity of the variation they create for data sets. The greater the variation in the data examined, the stronger the resulting explanation. By greater variation, I refer not to the quantity of data but to its qualitative form. In particular, ethnographies may use three progressively challenging forms of data variation.

A first test comes when the researcher tries to fit her initial explanatory ideas to the variations she documents. A second test of the explanation occurs when the analyst draws on cases documented by others for further evidence. A third test is the one that the current discussion highlights. This occurs when the explanation is tested across a range of social areas that are segregated in culture and that may never have been considered in the same breath by anyone before the researcher, using distinctively sociological thinking, saw the analogy.

Consider Diane Vaughan's (1986) study of "uncouplings," or how people make transitions out of intimate relationships. Vaughan tests her theory primarily on information she collected on 103 people whom she interviewed at some point in their process of uncoupling. Second, she refines her explanation by examining others' descriptions of divorces and transformations out of nonmarital intimacies. Third, she develops her explanation by examining the process of ending other types of relationships and terminating other kinds of interaction: she compares

what she has found about the phases of uncoupling with what she and others have learned about movements out of jobs, terminations of religious affiliation, and even experiences of "small girls" terminating a game of Monopoly.

While these comparative analytical data are less numerous than the other two types, they are especially critical to her analysis. This is the kind of "neutral" and seemingly policy-irrelevant thinking that Becker (2003) has advocated in his discussion of the language of Goffman's essay on "total institutions." At the time Vaughan conducted her study, uncoupling was a major emotional and therapeutic concern but not much of a political issue. And her reflection on what happens when small girls end Monopoly games is clearly entertaining but hardly seems meat for political debate.

A key challenge routinely ducked by ethnographers is confronting biases specific to the time and place of their work.

But her recollection of the cruelties involved when children stop playing with each other, along with other instances of "interaction termination" drawn from innocuous, everyday life situations, helped specify a key qualification of the phenomenon to be explained. Early on, Vaughan realized that *marriage* and *divorce* would not provide definitions of variation that could be explained. The formalities of marriage do not, she saw, make for uniform differences in the dissolution of relations among nonmarried intimates, whether they are homosexual or heterosexual. In the style of analytic induction, she redefined what others might have treated as a study of divorce into a study of *uncoupling*, a term she developed. While not a neologism, the term was somewhat independent of any precedent in popular culture.[5]

The example of girls' ending games of Monopoly was evidence for a further qualification of the explanation. Vaughan was seeking to specify necessary conditions or phases of uncoupling, not "why couples break up" but uniformities in the stages they go through. One of her most important findings was that despite the great pain and hostility common in uncouplings, there was also a great deal of mutual caring; she found evidence of caring in all cases, even the most bitterly contested (Vaughan 1986, 193). This was not what she recalled about how small girls stop playing monopoly. She recalled that frequently one would lose interest, but instead of confronting the other with the desire to end the game, the bored or distracted player would take advantage of a break in the action not to come back, break the rules so as to precipitate a fight, withdraw by attending to the TV, and so

on. Caring among the uncouplers sometimes took the form of direct confrontation; that may seem harsh, but, Vaughan noted, it clarifies the situation for the other, giving the other an opportunity to define a new stage in their lives.

This third type of test of her theory, in which she examined events in substantively foreign types of social situations, provided a crucial specification of the importance of the public nature of the couple's commitment to caring in the process of breaking up. Although she does not formulate the analysis in quite this way, we may theorize that the public nature of a commitment, which can be constituted by marriage but also by an ongoing intimacy that has become an open fact within a couple's personal public, makes dissolution more kind by making it more difficult. The dissolution of a previously public relationship is more likely to be reviewed by others and thus more carefully handled by oneself.[6] This Simmelian irony had no particular policy relevance at the time Vaughan's book was published. What could be more lacking in relevance than observations of how girls end games, how bus passengers disengage from annoying seat companions, or how bored partygoers manage their exits?

Today, the gay-marriage debate suddenly makes this preeminently sociological comparative thinking directly policy/politically relevant, albeit ambivalent in its implications. If making public commitments are valuable, not necessarily in keeping people united but in humanizing their separation, then gay marriage, as an explicit form of public commitment, has much to recommend it. On the other hand, gays were already in her sample, and the knowledge of their friends that they had been a couple provided enough public commitment such that their dissolutions demonstrated phases similar to those of formally married heterosexual couples.

Note that we can clearly mark the point at which Vaughan's analysis goes beyond obvious relevance for understanding intimate dissolutions: when she starts drawing analogies that only sociologists would bring into the discussion of uncoupling. This seems a commitment to the kind of neutral analysis that Becker (2003) has recommended. And yet, years later, it has become a key move in shaping the relevance of the study for a hot policy issue. The possible uses of her study in the current gay-marriage debate are ambivalent, but they greatly contribute to the debate by helping transform what is a highly emotional and symbolic discussion into a consideration of potentially decisive empirical consequences. If, for example, marriage enhances gay couples' public commitments in a way that diminishes cruelties in breaking up, then that bolsters the legal case that limiting marriage to heterosexuals violates constitutional guarantees of equal protection.

Being Here and Being There: Current Biases toward the Parochial and the Present

Each class of ethnographic work, then, has political significance depending on the place it takes within a larger collective act that is shaped by readers and histori-

cal processes that the author cannot control. A key challenge routinely ducked by ethnographers is confronting biases specific to the time and place of their work. The field of ethnographic research as a whole is woefully indifferent to biases that push fieldworkers to limit their studies to the parochial and the present. Too often, when concerns arise that ethnographic research is too "micro" and not sufficiently contextualized historically, the response is to abandon fieldwork for reading and theorizing.

"Being here and being there" captures a constant dilemma for ethnographers. *Ethnography*, in this usage, means a coherent narrative picture of social life. Ethnography's subject may be social life as lived at a particular geographic or organizational site, a set of people whose way of life shares a common theme, or a theme that characterizes a social movement or episode of collective behavior. As sociological texts, ethnographies contrast with texts that show relationships between variables or the features of ideas without conveying the social context in which those who display the variables or embody the ideas live out their lives. It is the commitment to a contextualized narrative that sets up ethnography's central strategic challenges in defining the sets of data that will underlie and discipline analysis.

On one hand, a variety of intellectual traditions point ethnographers to appreciate that whatever site they study is an artificially bounded fragment of a larger social reality. Whatever the "here and now" that the participant-observation fieldworker can study up close, the events observed and the ways of the people encountered have always already been shaped by social experiences in some other "there and then." On the other hand, the social-psychological realities of ethnographic fieldwork as an occupational practice constantly tempt the researcher to limit data gathering and analytic perspective to the here and now. In addition to the fieldworkers' occupational egocentrism, there will always be an egocentric, current reality bias in local culture that obscures the artificiality of local boundaries. The people encountered by the fieldworker care principally about realities here and now because that is principally what they can affect and because others they regularly encounter insist that they attend to local exigencies, and soon. The first step in making a new advance in methodological quality is recognizing that the quality of ethnographic work is in this sense of the phrase formed at the crossroads of being here and being there.

Here are some of the challenges to move beyond parochial and (to borrow a term from historiography) presentist biases that contemporary ethnographers have yet fully to acknowledge. Large-scale immigration into the working class, for example, poses unrealized challenges for Arlie Hochschild's emotion theory. In 1979, I started buying garden supplies from an "OSH" store in Hollywood. As a newcomer to Los Angeles, I was struck by the bizarrely exaggerated greetings and farewells that I would receive from cashiers, who then were almost all black and white. Today, the cashier who greets me at OSH is likely to be a Ukrainian, Thai, or Guatemalan immigrant, and she is likely to utter an enthusiastic phrase that may sound to me like "Chuvahnashdee!" It is striking how much has changed demographically at this site, but sociologically, what is even more interesting is how little

has changed. Despite the difference in accent and acculturation, I have no problem in hearing the merry mandate to "have a nice day!"

Now, if it is no problem for me to make out what the cashier is saying, it is also, in that situated work task, no problem for her that her accent is "heavy." The reason it is no problem for me is not because I am good with accents but because hers is a strictly regulated performance of emotion labor. I know what I am likely to hear at that place and at that phase of the shopping process. And the cashier knows I know. We both use the script of emotion work to obliterate the possibility that her "foreignness" will make her a less-than-effective worker.

The question of why some people stay in conditions of disadvantage and oppression while others leave is routinely neglected in urban ethnography.

For my imaginary immigrant service worker (and yes, here I am theory singing), it is a boon that she is doing closely managed, situationally specific, precisely scripted emotion work. Were I to encounter her elsewhere in the store and ask where to find fertilizer, the confusion that might result could be embarrassing. At the cash register, the routinized script of emotional expression provides the immigrant cashier with cultural clothing that she can wear perfectly well, even though it was not made for the body of ethnic culture she brings to the job.

Is emotion work dehumanizing, alienating, or otherwise harmful in this context? In areas like Hollywood, where emotion work has mushroomed as part of the exploding service economy, routinized scripts for expressing emotions fit remarkably smoothly with the predominately immigrant labor force now doing it. Perhaps after months or years of uttering these superficial niceties with an automaticity that rivals her cash register, the Ukranian immigrant cashier will become a "deep actor" of superficial California culture, profoundly alienated from her originally passionate folk soul.

What emotional labor might seem to mean to researchers from families that have been native-born for generations is not necessarily what it means to immigrants. The social distance between the biographies of university researchers and the current American workforce means that no amount of "reflexivity" will solve this problem: no single researcher is likely to be as diverse as the subjects he or she studies. If ethnographers are not to turn these questions over to survey researchers, they now need to strategize their research designs. The relatively comfortable

fieldwork design of situating oneself at a work site, learning how to work there, and observing how others do the work needs to be supplemented by more dicey data gathering that will reveal how workers manifest emotions and deal with accent and cultural misunderstandings in other areas of their lives.

We need a new wave of ethnographic research strategically designed to reveal both the "there and then" and the "here and now" that together create the lived biographical and situated meaning of emotion labor. This new wave is likely not only to point in surprising policy/political directions but also to require a fundamental rethinking of basic theoretical preconceptions. For example, researchers will probably have to struggle with the dizzying complexities of the relationships between home and work, as revealed by Christena Nippert-Eng's (1996) creative study. How do we know which behaviors and sentiments to attribute to "home" and which to "work," given that formally defined work is increasingly done at home and as we increasingly appreciate how personal life is shaped and sustained at work sites? How can we hold onto these categories, which at once render their meaning in dialectical relation to each other and richly provoke people to undermine their opposition in practice?

The spatial metaphor in phrasing the ethnographer's struggle as one of simultaneously being "here" and "there" obscures an even more difficult challenge of documenting the extralocal temporal reaches of local social realities. I will use community research as an example. The tradition goes back at least to Jahoda et al. [1930/1971]). In community research, ethnographers are massively pressed to limit their focus to the meanings of events for current local residents. There is a revealing irony in the facts that ethnographies of neighborhoods threatened by displacement reveal the sacrifice of valuable social cohesion and community values (see Gans [1962] and Suttles [1968]. Was there ever a neighborhood studied by a sociologist that was not deemed worth saving? But studies of residential life formed after urban renewal and urban planning reveal a positive meaningfulness of local area that is scorned by critics of the plastic character of housing plans, who see only sterility rising in the wake of destruction. (See Gans [1967] and a study of how residents actually live in perhaps the most ridiculed planned community in Milton Keynes, United Kingdom [Finnegan 1998].)

A reliance on the boundaries of social realities as defined in the here and now usually fails the ethnography of community research in at least two fundamental ways. The social reality of any place exists not only as a present for those currently in occupancy but also as a past in the lives of those who have left and as a future denied to others who took courses of action that led them elsewhere. In the genre of community studies, we do not learn much about the lives of those who have left the town or city; we learn even less about the meaning of the local area to those who settled elsewhere. By studying only those present at the time and place of the study, the ethnographer biases the policy/political message and sidesteps the novel challenges for theory.

By studying those who leave, one may find otherwise hidden meanings of attachment among those who stay and, even more, that policies of preserving people in place may be shortsighted. As millions of low-income migrants have crossed

vast geographic and social barriers to enter and move across the United States, ethnographers have continued to explain the pathologies suffered by low-income, urban populations by reference to local conditions. The question of why some people stay in conditions of disadvantage and oppression while others leave is routinely neglected in urban ethnography. Political sentimentalities may be at work here; it is a hard sell to convince local leaders to subsidize the costs of moving their constituents away. But political pressures aside, answering the question of persistence in place would seem to require carrying research beyond what can be learned in the current neighborhood. It is debatable whether policy/political conviction or methodological convenience is the stronger influence on research design.

Current populations are also shaped by processes in the lives of those who never arrived. What this means for the policy and theory limitations of urban ethnography can be quickly indicated by the challenge faced by ethnographers of California's coastal cities. (By coastal city, I mean not just where there is salt in the air but anything within ten miles of the coast.) Battles over land use and neighborhood preservation in coastal cities are constantly fought between those in place and agents who would bring in populations that do not yet have a local face. Most visible to the ethnographer are the local "growth entrepreneurs." Given their superior wealth, self-serving economic interest, corrupting contributions to the politically powerful, and relatively small numbers, developers routinely lose the narrative battle in the ethnography to the more numerous and usually less affluent residents allied against development, who seek no obvious material gain and are always at risk of becoming victims of politically corrupted capitalist schemes.

The joy with which ethnographers rail against capitalist developers is increasingly tempered by discomfort in defending what, in the most extreme cases, are becoming superrich communities. But the elephant in this collective act is not the capitalist developer; it is the mass movement of millions of workers who are deflected in their residential settlement further and further into desert communities toward the east. Dangers to historic preservation and to the precious coastal ecology are stressed by residents as they oppose development proposals. Meanwhile, workers' commutes mount to four-hour daily routines requiring countless tanks of gas to sustain lifestyles that must try to take root in new, air-conditioned desert homes.

A decisive theoretical commitment is necessary to break out of the increasingly unsatisfying political sentimentalities shaped by community ethnographies that give a priority in narrative voice to those living on site during the period of the research project. We must first be able to imagine a perspective that will include not only current residents, and not only those who commute in and out, but also the masses who never contemplated arriving because local conditions have indirectly but powerfully conveyed signals of blocked entry. At present, only the economists, through the concept of opportunity cost, know how to incorporate lost futures in their theoretical models. Unless we are to turn the field over to them or become servants of the privileged, what we need is a way to model the present that includes negative realities, the futures conditional that never materialized but that may

even more powerfully shape the ethnographic scene than what is positively in evidence in the fieldworkers' encounters.

Any social place consists of positive constructions and of powerful negations. Our community studies must begin to enable us to describe how current residents live side by side with the ghosts of those who have left. We must also find a way to document how some live with their own unrealized possibilities of exit. And we need to bring the spirits of the banished into contact with the precious tranquility of California's Santa Barbaras. The ethnographic demographics of a local community exist not only in the lives of those who reside and work there but also in the unrealized fantasies of those who have never been and will never be present. The ability not just theoretically to evoke the locally absent but to bring them into the description of what the fieldworker confronts at his site is not well developed in American sociology, but in other traditions, there are strong leads as to what might work.[7]

Ethnography as a Search for Community

All research is essentially a search for community, at least in the sense of an effort to be embraced by an audience to which the study's results will be pitched. In its original meaning (Geertz 1988), "being here and being there" refers to a tension between communities. Genres of ethnographic research differ perhaps most fundamentally in how they handle this tension.

The locals we seek to know at field sites typically have their own sense of community, one independent of the researcher's academic home. After leaving the university for the field, one way for the researcher to reestablish community is to do relevant research—research that speaks to policies that are of local concern. The dilemma of being here and being there can be substantially resolved when research speaks in terms that locals see as advancing their causes.

But there are other ways of finding community, and I would submit that as forms of sociological practice, it is the strategy adopted to search for community that most fundamentally distinguishes fieldwork methodologies. Because community is always political, each search for community has its distinctive political aspirations or pretensions. Researchers who are suspicious of local cultures have two alternative escape routes. One is horizontal; the other vertical.

The horizontal route to escape is not back to the academy or to some vacation land outside of social research but into another immersion and then another. The trajectory is not unlike serial monogamy, with the passionate involvements and risk of treachery at every transition along the way. Analysis grows as one questions the claims of unique local culture that are made at each site, which one does by using as a critique not some overarching sociopolitical theory but the professions of unique culture at other sites.

The series of involvements across substantively segregated research projects encourages an appreciation that each site contains all the others. Play is discovered

at work, and work, in the ethnomethodological sense of careful doings geared to a responsive payoff, is revealed in play. Education ethnographies do not turn up much evidence about learning in school; in ethnographic data, school looks more like a struggle between the classes. But education is highly visible in participant-observation evidence of life on the job. Art thrives in every corner of everyday life, from morning makeup routines to evening episodes of loving. Meanwhile, what people do in art museums has more in common with rituals of shame avoidance. Museum visitors train their attentions in anticipation of demands to identify artist, style, and epoch.

Community is sought not at any one site but in a sensitivity to the universalities of social process. Political significance comes from debunking the claims of authentic boundary made by local culture and by offering the liberating perspective of commonalities found across formally segregated sites.

An alternative escape from immersion in a field site is through a vertical movement. Whether because of personal precommitments (the fieldwork training course is over; it is time to get a job; another research opportunity beckons) or from the discovery that one cannot manage to make the self sufficiently malleable to become locally accepted, community is sought in some more powerful and elevated region. Escape may come through the always open doors of academic discourse, in the form of endless theorizing, teaching, and methodological reflection. Or escape from the field may be justified as an act of opposition to the powerful, who rule from distant, inaccessible sites: if the forces shaping social life in the lower regions that one can enter are really in higher locations, staying in the lower regions too long risks accepting the false consciousness that people there can really rule their lives.

Indeed, what does the growing global reach and consolidation of multinational corporate and political powers tell us about the source of the social patterns that occur in the small-scale settings that we may enter as participant observers? That our site is "here" seductively denies that the real causes are located "there," in socially distant, higher regions. Is it not increasingly foolish to immerse oneself in a search for explanation in *any* local site? And even if one could enter the halls of the powerful, theory tells us that the action really is not much there anyway but in the long course of history and in the structuring of large-scale social formations in which we can only aspire to be fleeting, tightly circumscribed, insignificant participant observers. In the increasingly common songs of theorized globalism, the only justification for participant-observation ethnography is to give a first-person witnessing of the sufferings and horrors that distant powers cause and then ignore (Bourdieu et al. 1999).

I have suggested that the following are three class strategies: the worker's, the bourgeois professional's, and the aristocrat's. Class in this application is a matter of the researcher's way of relating to the practice of fieldwork. In the working-class version of ethnography, the researcher operates like a jack-of-all-trades, hanging out with the guys and using relatively unspecialized skills to portray wide-ranging sectors of their lives. Because fieldwork is the closest thing we have to a sine qua

non of the ethnographer's identity, in this social arena, if in no other, the usual strat-
ification of class status often is turned on its head. Hence the extraordinary imme-
diate recognition given to Duneier's (1999) *Sidewalk* and the extraordinarily
enduring appeal of Anderson's (2003) corner study.

As bourgeois professional, the ethnographic fieldworker develops files that
describe the evolution of cases over time. A set of case files is created by developing
a specialized expertise that is substantively narrow. Over time and multiple studies
in diverse substantive contexts, the researcher's self becomes shaped as a tool that
facilitates subtle appreciations of how the fate of each case, regardless of substan-
tive context, is shaped by what happens at given points in its trajectory (see, e.g.,
Emerson 1981, 1983). A common intellectual result is an ongoing suspicion of any
local culture as artificially claiming distinctive social realities and causal forces.

> *That our site is "here" seductively denies*
> *that the real causes are located "there," in*
> *socially distant, higher regions.*

The aristocrat may write at great length about his or her own theoretical preoc-
cupations and in-the-field subjectivity. Spending relatively little time in the embar-
rassing business of shaping a self that strangers in the field will embrace, or at least
tolerate, the aristocrat quickly develops the confidence to model the world from a
removed study. Unlike the working-class or bourgeois professional, the aristocrat
happily dispenses with the grunt work of ethnography, the laborious recording and
meticulous examination of others' detailed doings. Or, having done some grunt
work as a kind of rite of passage, the ethnographer may find life at that level unsatis-
factory. Without any showing of relevant variations in the matters to be explained,
the ethnographer may then elegantly select illustrations and invoke theoretical
imagination to construct a narrative posture that operates as a tasteful sensibility,
condemning what he or she cannot in fact explain.

It might be said, "You choose your class and you get your methodology," but as
sociologists, we know that class identities are not freely chosen. If there is anything
universally distinctive about participant-observation fieldwork as a research
method, it is that it is a socially structured, existential crucible. What you will be
able to do, as a matter of personality, you cannot know in advance. What you can do
at one life stage is not necessarily what you can do at the next. Ethnographers
always have a class status but, we may hope, not one that is fixed at birth.

Notes

1. In his ethnographic study of life aboard ships, Aubert (1982), a Norwegian sociologist insufficiently appreciated in the United States, provided some of the evidence that Goffman drew on in writing *Asylums*.

2. Roy's studies were exemplary of what I call the "bourgeois professional" style of ethnographic work of the Second Chicago School. He created similarly structured dossiers on the work activities of multiple workers at multiple times, then disciplined his explanation to fit with those differences. For an exemplary "worker" ethnography of related matters conducted at about the same time as Burawoy's study, see Kornblum (1974), who, in a text that is at once extraordinarily detailed and holistic in the understanding it conveys, studies social life on and off the job at several steel factories in South Chicago, relating differences in worker perspective to differences in ethnicity, immigration history, unionization, strikes, and local political history as well as to differences in the structure of the production process across factories. Stepan-Norris and Zeitlin (2003, 183) have recently noted that Burawoy ignored readily available evidence that would have shown variation in worker militancy. "While Roy was working in his shop, Orvis Collins was also working as a milling machine operator, in a shop employing 90 to 110 machine operators, in another factory. (Roy's shop employed only some 50 men). Collins worked in his shop for about six months and he also spent many months afterwards interviewing the men he had worked with there (Collins, Dalton, and Roy 1945). This ethnography is not cited by Burawoy." In Collins's shop, were substantial "radical" sympathies, as manifested in part in discussions about the Soviet Union, and greater militancy in union activities. In other words, Burawoy's failure to specify the meaning of consent by describing differences in worker perspective was not the result of a lack of available evidence that could do just that.

3. Significantly, Burawoy's own evidence on the timing of changes in the structure of capitalism and the introduction of a new, more humane managerial philosophy after World War II does not line up with his macroexplanation. The internal labor market, a relaxation in management constraints that increased workers' freedom to move around jobs, was created *before* Allied bought Geer. The managerial change was instituted *before* the change in the structure of capitalism. It appears that Milton Friedman, not Karl Marx, is sustained by his data: an increase in the demand for labor following the expansion of the economy after World War II led to an improvement in the bargaining position of employees, which received expression in a variety of ways: higher wages, more union strength, better working conditions.

4. Burawoy attributes this objection to a passage in Howard Becker's writing. If one reads the passage that Burawoy cites, one finds that Becker (1998, 89) actually treats Burawoy's work as an inspiring model for research. Apparently aware that his work is vulnerable to the critique of no difference, Burawoy creates a context to defend himself by imagining an attack.

5. This move from a popular cultural definition of a problem to a phenomenologically grounded definition of the problem from the standpoint of the people whose behavior is at issue is typical in the procedures of analytic induction. See Katz (2001).

6. While Vaughan says that she is aiming for "how" not "why" explanations, I would put it a bit differently. She is not aiming to explain why people break up, but she is trying to explain why breaking up has certain processual features, in this case, caring. Analytic induction inevitably presses toward causal, "why," sufficient condition explanations and makes great contributions in advancing knowledge, even if it usually gets only to "distinctive" as opposed to "necessary and sufficient" conditions.

7. I think of de Certeau, Derrida, and Latour. See, for a tantalizing example, Octave Debary's article in this volume.

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