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"Assuming your argument is empirically sound, so what?" Ethnographers are especially vulnerable to this question because their warrants are commonly diffused throughout their texts, because they aim to describe what is obvious to their subjects, and because such rude questions usually are raised only silently. Perhaps the most common warrant for ethnography is a claim that social forces have created a moralized ignorance that separates research subjects and the research audience. The author discusses several dilemmas that plague ethnographers when they attempt to bridge the gap, and then he describes the strategy of naturalistic ethnography. Last, he briefly addresses a broader range of warrants, identifying five additional, frequently used, complementary justifications for ethnographic studies.

Ethnography's Warrants

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Ethnographers must find a raison d'être in response to a powerful paradox. On one hand, the ethnographic method is distinctively committed to displaying social realities as they are lived, experienced, understood, and familiar to the people studied. Statistical social research, by contrast, is particularly successful when it can demonstrate that features in individuals' backgrounds (e.g., birth order, geopolitical region of residence, parents' education, gender, race) pattern their behavior even while they remain unaware of the influence. Statistical analysts have reason to celebrate when their subjects, citing their own experience, display astonishment or vehement object to research findings, as such reactions only prove that special methods, inaccessible to the layperson, are necessary to uncover fundamental realities. For the researcher working with large quantitative data sets, such "counterintuitive" findings are delightful resources for responding to the common criticism that sociology only

documents common sense. Ethnographers faced with similar reactions from their subjects have good reason to be unnerved. If ethnographic descriptions do not fit the texture of experience as lived by research subjects, then they may be useful only as projections of the researcher's imagination. Few ethnographers have created a style of analysis or a cult of personality sufficiently robust to make that an effective warrant.[1]

But if ethnography must describe its findings as matters of everyday experience to its subjects, then the other horn of the dilemma pops up: Why are realities that are obvious to the subjects not also obvious to the ethnographer's audience? All ethnography is haunted by the paradox that its distinctive methodological respect for its subjects' meanings implies that its labors are gratuitous. Put in the form of a question addressed to the ethnographer: If all you have to offer is just a description of commonsense reality, then what is your contribution? If you claim to describe what everyone studied already knows, then who needs you?

Such questions are instances of a general challenge that ethnographers face: establishing a warrant for their research. Without a warrant for a study, no matter how beautifully one's empirical claims may be established, one still may be bowled over by the question "So what?" or "Who cares?" What distinguishes ethnography as a research practice is not only that the process of inquiry must execute a warrant well (e.g., by gathering data that are nuanced, densely textured, locally grounded, meaningful to the subjects, etc.) but also that ethnographies risk being considered banal unless they discover, in the data-gathering process itself, grounds to argue that there is a need for the study in the first place.

Within the vast field of ethnographic study, there are many subgenres. These may be differentiated by noting the kind of bridging work by which ethnographers connect their subjects to their audience. The picture that emerges is far more complex than the one drawn by Mills (1963) when he attributed the motivation of many of the early studies of social problems to the social distance between immigrant, poor and working class, urban, often Catholic subjects and the native-born, Protestant, middle class, rural or small-town origins of the sociologists, many of whom came from ministers' families. But Mills's underlying theme about the sociology of sociological knowledge remains valid;

of the warrant as a general methodological concern that is relevant for all ethnographies. In the final section, I briefly note several warrants that are applicable to a more general range of ethnographic research subjects. Ethnographic methodology is bedeviled by doubts that are both blunt and frequently unstated. But once made explicit, the challenge of "So what?" calls out numerous, mutually supportive answers.

DANGEROUS SOCIAL AREAS
AND MORALLY PERVERSE PEOPLE

A provocative social distance obtains when the people and places studied are thought to pose risks to nonnatives of such a magnitude that they can gaze only from afar and through a veil of mystery that is sustained rather than dispelled by the glimpses provided by journalism, routine police reports, and periodic riots. It is thus not by accident that deviance, social disorganization, and neighborhoods that are regarded as breeding grounds for social pathology consistently have been a focus of ethnographic inquiry in sociology. Not only do funding sources disproportionately support ethnographic research on deviance, but would-be ethnographers searching for topics to study can easily appreciate that, by choosing a terrain with a deviant reputation, they will avoid the wonder, confusion, and indulgent pity that they can anticipate from family, friends, and academic critics if they choose to study something that calls for no special moral notice—topics such as why Jews in American cities have favored Chinese restaurants (Tuchman and Levine 1993), how people play fantasy games (Fine 1983), or what people on rollerskates must do to get around on city streets (Wolfinger 1995).[2]

NORMALIZING AND BOHEMIAN PORTRAITS

The fact that a study may be conducted under the auspices of a conventional belief that the subjects are scary or troublemaking does not necessarily build in a bias to sustain the beliefs that justify the study in the first place. On the contrary, one of the most common ways to warrant ethnographic research is to produce a text that demonstrates that the anxieties behind conventional opinion are unfounded. An area

thought to be a slum, in the sense of an area suffering from the effects of social disorganization, is shown to be governed by an elaborate internal order (Gans 1982; Suttles 1968; Whyte 1955). A practice such as heroin use, depicted in popular culture as enslaving, is shown to be governed by cycles of abstinence and relapse (Ray 1964). Seen up close, street-corner men (Anderson 1978) and adolescent gang members (Klein 1971) often seem to live lives that are more banal than frightening. Again and again, ethnographers claim to have made a novel contribution by asserting that groups conventionally thought to be deviant actually are serving conventional motives or at least conducting deviance via familiar social conventions.

It is relatively rare for an ethnography to confirm that an area of social life is indeed as physically unnerving as, or even more morally perverse than, conventional views would have it. But there are some examples. Recent ethnographic accounts of crack houses depict them as cubicles in which humanity is tortured and degraded to such extremes that the everyday realities would blend in as natural only if they were configured as a patch in a painting by Hieronymus Bosch or in a poetic nook of Dante's imagination (Ratner 1993). Polsky (1969) described hustlers in pool halls and beats in coffee houses as morally unconventional and proud of it. Burglars recently have been depicted as desperately poor, but the pressure of their poverty is shown to be the recurrent result of the very illicit "partying" that they are desperate to rejoin (Wright and Decker 1994). Middle class, well-educated women are found to get pregnant and have abortions not, as much conventional opinion would have it, from ignorance, unavailability of contraception, or psychological resistance but rather by making "a de facto choice of abortion as a method of fertility control" (Luker 1975). This choice becomes understandable only when one appreciates that, in the precise social situations and sequential contexts in which the choice is exercised, there is not only a range of costs to contraception but also, and in ways outsiders might judge to be morally perverse, brief but fateful appreciations of the benefits of pregnancy.

Ethnographies that are warranted by the deviant social reputations of their subjects may undermine or promote a sense of social distance. Using an outdated and thus presumably innocuous term, we might dub as *bohemian* those studies that find that the moral fabric of subjects' lives is more deviant than conventional opinion had imagined. The

tendency to document "them" as essentially like "us" but living in troubled circumstances with which "we" need not struggle might be referred to as "normalizing."

Whether the bohemian or the normalizing view is more correct for any given area of socially defined deviance is not simply a matter of the author's or the readers' preferences and values. Ethnographic research on deviance owes much of its methodological strength to the powerful ways in which the social realities of subjects' worlds resist the imposition of the researcher's own moral preferences. Bohemian and normalizing portraits face different but equally insistent methodological challenges, and both risk equally systematic, if different, sources of error.

Bohemian studies risk romanticizing (or, borrowing a term from anthropologists' critiques of anthropology, "exoticizing") their subjects. Klein (1995) documented a tendency in popular culture to exaggerate the depth of the social reality of adolescent gang culture. Outsiders commonly read the symbols of gang life (e.g., graffiti, hand gestures, clothing styles, dramatic acts of violence) as indicating that gang members live in vibrantly distinctive ways. But sometimes what initially appears to be the tip of an iceberg is, when carefully investigated, only a cold tip. For various reasons, in Los Angeles and other gang-plagued cities, the police and often the liberal urban public want to believe that gang culture organizes much of youth violence. But youth violence rates do not vary among cities or over time in relationship to the perception of gang organization. What gangs more obviously organize are two related patterns of urban American life. One is a pattern of large-scale, constitutionally improper police interventions. The other is a public understanding of a disconcertingly high level of youth violence in parts of the city's population that are especially difficult to comprehend, namely the culturally foreign and politically muted neighborhoods. If gangs do not in fact increase the tendency to violence among young minority men in urban poverty areas, still the idea of the gang effectively explains otherwise incomprehensible incidents of peer-directed violence and addresses the public's anxieties over growing sections of the population that maintain a low profile in representative politics.

The point is that ethnographic methods, by looking beyond symbols to everyday realities, will reveal and can correct errors of romantici-

zation. The error of romanticization is that of falsely suggesting that an inspiring culture diffusely and powerfully organizes behavior along deviant lines. Such errors are commonly betrayed in ethnographic texts that fail to describe how the subjects make situated use of deviant themes in their everyday lives.

The errors of the normalizing view are similarly detectable by examining the extent to which the research has met the ethnographic challenge to document local meanings. For historically emergent reasons, this direction of error has come to loom especially large in the current generation of ethnographic work. The avoidance of this error currently is a major pedagogical challenge.

A generation ago, Becker (1967) argued that the researcher of deviance naturally colored his or her research, depending on the "side" he or she took as a practical matter when conducting the research. If one uses officially collected statistics, then one operates from a perspective fashioned by those in power. Ethnography distinctively offers the opportunity to convey local meaning, that is, the meaning of subjects' actions to the subjects. Ethnographies of deviance, to the extent that they describe the perspectives of those "below," should be expected to highlight the distortions of views imposed from "above."

An exception, Becker (1967) pointed out, is when the moral status of the putatively deviant group has become politicized. Then the "deviants," or others acting on their behalf, are likely to have produced and dis- seminated a portrait of themselves that is far more favorable than what an innocently motivated ethnographer is likely to describe. Becker was writing early in a powerful trend. In 1979, John Kitsuse (1980), in his presidential address to the Society for the Study of Social Problems, offered a commentary whose implications for the ethnographic study of deviance have still to be fully appreciated (Kitsuse 1980). Kitsuse was writing during times when the civil rights movement was proliferating beyond race to offer political power to a seemingly endless series of groups that had long suffered under reputations as deviant. Groups that had hidden "in the closet" were now publicly organizing to demand an end to official sanctions and a destruction of the harmful stereotypes on which they were based.

For the ethnographic research community, the institutional successes of the civil rights movements have fundamentally revised the warrant that, since the Progressive Era, had consistently underwritten

the study of deviant populations. In the period between the papers delivered by Becker (1967) and Kitsuse (1980), new social actors were rapidly emerging specifically to filter "our" perceptions of "them." One after the other, stigmatized groups were deputizing specialized agents to provide the preferred political spin on the appearance that is conveyed to the general public. As a result, the political thrust of the ethnographer's work has developed a profound ambivalence. Now any account of the everyday realities in social worlds whose members are battling reputations as deviant is likely to uncover realities that the group's public relations agents will find embarrassing and counterproductive.

There may be no greater challenge to the quality of ethnographic research today than appreciating and thinking through the response to this challenge. A group's social reputation as deviant still serves as a powerful warrant for doing ethnography because life behind a deviant label is likely to be in some respects different, if only because of the distinctive challenges that a reputation for deviance brings along with it. But as a matter of politics, the ethnographic researcher who moves onto "deviant" turf is apt to find that he or she has unwittingly moved into the service of those who will find bohemian portraits useful for repressive purposes. Because sociological ethnographers almost always want to benefit the interests of the people they study, the predictable result is an intense conflict.[3]

Behind the public relations lines, members of "deviant" groups perceive, respond to, and often undermine the images that the researcher may assume best serve the subjects' interests. The participant observer who hangs out on Los Angeles street corners where illegal immigrants seek work may hear them talk about the availability of public benefits in tones that sound joyfully cynical. A researcher who observes from inside gay rights organizations will see interactions in which erotic themes and *femme* stereotypes are engaged playfully in ways that would give comfort to conservative critics. An observer of young women who wear very short skirts as part of the uniforms in which they serve hamburgers on roller skates to restaurant clients may expect to find a management bent on sexual exploitation and an emotionally suffering female staff; but, in Los Angeles at least, what ethnographers are likely to find is that the employees shorten their skirts in defiance of management policy, that their sisters and mothers celebrate their

costumes as cute, and that the job is a natural extension of their prior careers as high school cheerleaders.

Confronted with such material, the ethnographer realizes that it will not be easy to present field notes that will warrant the study simply as a demonstration that outsiders have failed to appreciate how much the subjects are abused by inaccurate stereotypes. But to explore fully the subjects' subculture is to risk putting oneself effectively in the employ of repressive outside forces. A common response is to cut the embarrassing material from the presented data and to make up for what has been removed by rhetorical argumentation about repression elsewhere in the social system—repression that understandably encourages undocumented migrant workers to cynicism, that brings a gallows humor to gay rights groups, and that encourages young women to exploit their physical appearance long before capitalism lays its mercenary hands directly on them.

Whatever the empirical basis of such arguments, they point to times and places in the subjects' lives that are beyond the ethnographer's reach. The methodological strength of ethnography is revealed, not necessarily in the avoidance of political or moral bias in the construction of texts but rather in the textually transparent escape from firsthand data to ringing rhetoric when significant steps must be made in the analysis. Like the research design in survey and laboratory experimental work, the ethnographic warrant does not guarantee a loyal execution of the research act, but it does distinguish between matters of relevance and irrelevance, separate documentation from exhortation, and create a frame for investigation that provides readers with a perspective for evaluation that is independent of the researcher's preferences.

To the extent that the warrant for an ethnography is that it describes social areas that are conventionally thought to be deviant, the value of the study will wax and wane depending on the fate of conventional belief. The place of marijuana in contemporary popular culture no longer fits the exotic and bohemian images of the 1950s, and so an essay on the process of learning to smoke marijuana no longer has the same bridging mandate to fulfill that it once had. It is now hard to appreciate the sense of dread that middle class readers apparently had about the Boston street corners described by Whyte (1955). By the 1960s, such Italian city neighborhoods had become favored tourist spots, and it was black and Spanish-speaking neighborhoods that were

thought to be dangerous (Vidich 1992). Whyte's study, if conducted today in North End, might still be warranted, but the nature of the warrant would have to be different.[4] Because the social distance between particular ethnographic subjects and the audience for ethnography may diminish greatly over time, the ethnographer is well advised to become self-conscious about the entire range of warrants that may sustain his or her work.

SOCIAL WORLDS OF THE ELITE AND THE ADMIRED

If the status of a group or practice as deviant provides a firm warrant for ethnographic research, then so too, for related reasons, does the self-proclaimed or imputed status of a group or practice as elite— especially powerful, charismatically inspired, possessing a rare sensibility, or otherwise worthy of great deference. Getting "behind the scenes" is a compelling basis for inquiry whether the challenging distance is created by dread and deviousness or by respectability and a privileged insularity.

The penumbra of charisma that surrounds many high-status positions in society is a reliably provocative dare for ethnographic research. Thus Morrill (1995) effectively addressed the warrant for his book on conflict management among corporate executives by asking "Why study up?" and answering,

> Despite the lack of close-up scholarly studies of executives, their folk hero status in American society has begotten voluminous popular and prescriptive literatures. . . . Much of the popular literature fits into what one business scholar calls the "great man school." . . . To some degree, all of these sources convey the image of corporate executives as twentieth-century Napoleonic men on white horses. . . . Executives in their memoirs and autobiographies portray themselves as risk takers, mavericks, and visionaries and as hugely successful in nearly all their endeavors. It is as if executive life were a series of one-man plays (and they are typically about men) brought to life through the sheer force of the protagonist's will. (pp. 9-10)

With such powerful myths touted on best-seller lists, Morrill could be confident of success even if he found nothing more than what ethnographers always find: that people act collaboratively and that what

outsiders think of as the product of individual personality is in fact the result of social interaction.

But the ethnographer of elites does not always have such a sure bet. Groups that seem elite and privileged in the eyes of people who look on from one position in society may seem unexceptional and may appear to enjoy no special deference in the eyes of people who look on from other standpoints. One of the dangers that an ethnographer faces in studying a group that he or she considers elite is that, to the ethnographer's readers, the group may be familiar and not particularly noteworthy.

It is especially likely that the subjects will see themselves as less privileged than outsiders may imagine because their outlook on stratification is developed from their own position vis-à-vis other groups. An ethnographer who studies a group or social area that has been labeled as deviant by popular culture, by the repeated enforcement actions of the criminal justice system, and by social welfare agencies' activities has something important to say if he or she finds that, as a matter of everyday culture and in the patterns of routine social organization, nothing unusual is happening. Less obvious is the contribution made by a description of normality as the tenor of work life among highly paid professionals, university scientists, religious leaders, or powerful politicians.

The problems of warranting studies of elites are not due primarily to the subjectivity of judging who and what is privileged and respectable. The reality of exceptionally high moral status is as objectively grounded as that of deviant status. As with deviance and disrepute, charisma and exceptional respectability exist as sociological facts to the extent that people and practices are treated in ways that sustain those special moral imputations (Katz 1975). For elites, this typically means such matters as the right to exploit a monopolistic license and ready access to exceptional financial investments by a supporting community.

While people in privileged positions enjoy unique abilities to operate outside of otherwise routine forms of oversight, in contemporary society they also are routinely engaged in describing and explaining their everyday practices. Lawyers who have the power to charge clients $500 an hour also have the obligation to describe their work in 10-minute segments and to log each work-related phone call. The prob-

lems of establishing an ethnographic warrant for studying elites are rooted in the culture of rationality that underpins such institutionalized accounting practices.

Thus it is a common experience in studying lawyers, for example, that they convey to ethnographers their wonder at what the researcher could possibly discover given that they already specify and record the nature and reasons for their actions in documents produced for clients, courts, and regulatory agencies. Likewise, scientists in laboratories are centrally occupied with formally explaining the results of their professional action before the ethnographer shows up. Their everyday research practices are undertaken with a constant attention to the implications for reports that will be written (Latour and Woolgar 1986). Doctors may be surprisingly unconcerned with what an ethnographic investigation might find because they already are conditioned to be concerned with what outside critics may find, as indicated by the size of the malpractice premiums they must pay. Subjects in elite positions have good reason to be disarmingly indifferent to ethnographic research.

It is common to remark that ethnographers tend to study down and to explain that tendency by referring to the self-protective secrecy of elites. But ethnographers who have studied people in elite positions do not support the view that their subjects are especially inaccessible (Ostrander 1993). Paradoxically, the relative lack of studies of people in elite positions may have less to do with their secrecy than with their institutionalized openness.[5]

The challenge of this openness goes directly to the ethnographic warrant. In effect, the ethnographer is told by the elite subject, "Here is what we do and why we do it," and then the ethnographer is asked, "What is there about us that we are not already the experts in knowing?" The problem is especially severe with elite or charismatic groups because they claim a moral autonomy, a special knowledge (e.g., medical, legal, scientific) or a special sensibility (e.g., religious, artistic) that cannot be reduced to conventional dimensions of social life. Unless the ethnographer can clarify for the subjects how he or she will advance their understanding of their own world, the implication may easily arise that the study is a hunt to uncover material to embarrass the subjects by undermining their public image.

Thus a moral problem arises for the ethnographer of elites that complements the problem faced by the researcher who would study a group that he or she regards as unjustly labeled deviant. If the thrust of the research is to debunk the respectable group, then the researcher will be pressed to dissemble in order to proceed. If the thrust of the research sustains the group's claims of moral autonomy, then it may not be clear why the group itself, rather than an outsider/sociologist, is not the best source for information on its ways and whys. In this latter case, the challenge of the warrant is restated: Why should we think that we, or anyone else, needs you to study us? A common practical resolution to this dilemma is for the ethnographer to be massaged by the subjects into the model of a science writer who popularizes knowledge that is too esoteric in its natural form for the lay public to understand. But that will not satisfy the ethnographer's own professional research audience, and so the dilemma of dissembling arises again, albeit in less stark moral forms, because in this case the ethnographer's "cover" has been designed by the subjects.

Even if the moral dilemma is resolved when the researcher is invited to play an emissary role, the ethnographer of elite subjects still faces a severe challenge in establishing a warrant for the research. On one hand, if the ethnographer takes as data the special knowledge that the subject group claims, then on what basis can the ethnographer claim to understand more than experienced practitioners already know? On the other hand, if the ethnographer skirts distinctive features of the subjects' culture and practices and treats social relational phenomena in terms applicable to any social world, then what is the relevance of the subject group's special status for the researcher's sociological analysis?

SOCIAL FRAMING AND CONTEXT-SPECIFIC ACCOUNTS

Ethnographies of elite worlds can be sorted out with respect to their response to this dilemma. One approach, which may be characterized as *social framing*, avoids describing culturally distinctive matters in favor of analyzing the social relations that set up and support the subjects' world. Consider the study of musical work and careers. Making music and developing a career as a musician might be considered charismatic or at least as requiring a special competency, but

one can study musicians' social worlds sociologically without describing the practice of any of the music itself. Thus one can describe the relations that musicians create with each other and with their audiences to set the stage to play their music (Becker 1963), and one can analyze turning points and contingencies in musicians' careers (Faulkner 1985). Such work neither debunks nor explicates the unique sensibilities that presumably characterize this artistic social world. The authors only implicitly address readers' assumptions about the special sensibilities of musicians. Explicitly, such studies are offered as relevant to sociologies of work and careers.

An alternative approach might be characterized as *context specific*, and this approach in turn has its subtypes. Ethnographies become studies in *folklore* when they describe local cultures without addressing either social relations within the examined world or the contingencies of the production of its distinctive culture. Ethnographies become *ethnomethodological* when they focus exclusively on the sequential production of what it is that practitioners of esoteric competencies distinctively and in detail do. In contrast to social research that uses standardized and preset definitions of variables, ethnography is uniquely able to enter the culturally specific world of subjects. But once the researcher begins to make descriptive use of the culturally autonomous language of elite or charismatic practices, sociological readers are likely to get glassy-eyed and, for their part, expert practitioners may not grant that they have learned anything new. Thus David Sudnow's monographs on his solo piano playing became, for sociologists who were not themselves musicians, exercises in applied philosophy more than contributions to sociological theory (Sudnow 1978, 1979), and they left music critics unimpressed by Sudnow's admittedly accurate explication of what practitioners tacitly know (Lipman 1979; Rothstein 1979). Ethnomethodological studies in which the sociologist attempts to represent the perspective of a competent practitioner in worlds of science risk a similar fate (Lynch 1985).

In ethnomethodological ethnography, the effort to make substantive statements about the social relations that frame esoteric work is eschewed as reducing or glossing what is distinctive to the social domain at issue. But in making a bow to the need to be "loyal to the phenomenon," the researcher may abandon the sociological audience. Studies of this type risk becoming exercises in applying the ideas of

figures such as Maurice Merleau-Ponty, Alfred Schutz, and Ludwig Wittgenstein, philosophers who pointed the way toward new forms of empirical investigation but did not invite sociological research to reform philosophy.

I have argued that, in fashioning a warrant to study subjects conventionally regarded as morally exceptional, ethnographers predictably swing between characteristic forms of error. The ethnographer of people who are thought to deserve some special deference, for example, risks missing what is culturally distinctive about the research target if he or she focuses on matters of social framing. If the focus is on what is culturally distinctive, then the ethnographer risks losing a sociological audience. There are two common ways to maneuver around these risks. Both focus directly on the social distance that creates a special status for their subjects.

DEBUNKING CHARISMA AND DECONSTRUCTING DEFERENCE

First, the research can reveal behind-the-scenes matters that undermine historic presumptions of honor and special sensibility. Elite lawyers may be shown to aid their clients to destroy evidence sought by criminal investigators (Mann 1985). The households of the socially and financially elite may be found to display images that are substantially similar to those displayed in households of socially lower and presumably less refined tastes (Halle 1993). Surgeons may be described as no more morally sensitive to the objects of their work than were the butchers from which modern surgeons evolved (Millman 1977). Close description of the practices of futures traders may explode the mystique of supposedly great technical complexity and high pressure that is sustained by outsiders who are overly impressed with the size of the financial stakes (Abolafia 1996). Participant observation research on medical students may reveal that their youthful idealism remains sturdy with respect to matters outside of immediate demands but that, in the context of everyday work, a professional cynicism quickly begins to take hold (Becker, Geer, Hughes, and Strauss 1961).

Less frequently, the debunking light of ethnography is thrown in the opposite direction, onto the perspectives that create elite statuses and institutions of charisma. Two important examples are Becker's

(1982) work on art and Latour's work in science studies. Becker in effect sides with the artist in a bemused skepticism about the imputations that would set art worlds apart as uniquely refined, inspired, or inherently transcendent. His work gives pause to the adulation of art in several ways. One is by documenting the essential similarity of activities regarded as art and those regarded as craft. As with his work on deviance, Becker indicates that the labeling process, which he demonstrates in several instances to be dependent on historical contingencies, is arbitrarily related to the nature of the activities and products that are labeled arts and crafts. What was a craft at one point in history comes to be regarded as an art at another, and vice versa.

Another thread of the argument is the appreciation of the multitude of actors and activities that must be fit together for an art product to be recognized as such. The taste and practical pressures on gallery owners, the inventiveness of firms that create new paints, and even the physical labor of the carpenters and electricians whose work puts an art object in a place and light to be admired all not only are essential to the emergence of work as publicly recognized art but also may entail acumen, discriminating judgment, and idiosyncratic talents that may exceed the qualities of the person admired as "the artist." A thickly textured ethnographic description of "art as collective action" makes a powerful case that the attribution of artistic status to particular people and objects says something more specifically about the needs and fantasies of the admiring public than about the distinctiveness of the people and things admired. Himself a musician and photographer, Becker cannot fairly be said to be debunking the pretensions of those regarded as artists. His message emanates from a classic ethnographer's stance at the side of the subjects and looking out, wondering why it is that outsiders make such emphatically misguided sense of what artists do.

Latour has taken the implications of this ethnographic perspective a step further in his studies of science. Beginning with his ethnography, carried out in collaboration with Woolgar, of the Salk laboratory (Latour and Woolgar 1986), and continuing through his ambitious essay, *We Have Never Been Modern*, Latour (1993) has taken on a series of assumptions about ontological separations between science and the humanities, between reason and passion, and among people, nonhuman animals, and inanimate things.

For our purposes, it is enough to note that Latour's appreciation of the practical workings of scientific activity is not necessarily an attack on the scientists' pretensions. Scientists go beyond and undermine the apparently rigid control of explicit research designs and reports in many senses. Research designs help scientists get funding. They portray a purposiveness and a force of rational control that inspires nonscientists to make similar commitments, a point that Latour (1988) makes in his analysis of Pasteur's importance for the rise of anti-Prussian militarism in France. Research designs also guide the ad hoc and otherwise chaotic problem solving of everyday scientific practice so that they fit a post hoc framework of rational predesign. But scientists are pragmatists, not charlatans, and they do not worry that research designs may not control research practice empirically. That outsiders may, for their own independent purposes, wish to believe that research designs govern the actual conduct of research practice is a problem in the frame of mind that would distinguish itself as rational and "modern" as opposed to some imagined irrational and premodern epoch in human history.

Whether Latour is "right" is not the issue here. Our concern is to clarify the warrants that ethnographers may establish when they study social worlds that are deemed to be morally exceptional. One powerful warrant is that of the debunking ethnography, but the debunking need not be of those considered to be elite. With Latour, the debunking blow is executed not to bring scientists down to earth but rather to deconstruct taken-for-granted distinctions among political, empirical, moral, rational, and sensual realms. (Latour's radicalism holds that these distinctions, when imagined to have empirical and existential justifications, can set up terrifying authoritative centers of practically autonomous power.)

NATURALISTIC STUDIES OF THE MORALLY EXCEPTIONAL

I have reviewed two sets of strategies for warranting ethnographic research on prestigious, charismatic, or admired subjects. The first pair of alternatives is to focus on social framing versus local culture. The second pair of alternatives debunks the social distance that sets elites apart. There is a third, rarely attempted way in which to warrant the

ethnographic study of elites, and it parallels a way in which to warrant the study of deviants that also is rarely executed. This is a naturalistic approach, sometimes more and sometimes less phenomenological in its execution, that evokes the distinctive social interactions and the unique cultures that create genuinely exceptional sensibilities.

Studies of especially respected groups such as surgeons follow this path when they describe, in a manner that is neither debunking nor adulating, the distinctive moral codes with which the group recognizes, sanctions, and covers up its errors (Bosk 1979). In the study of deviants, naturalism may mean documenting the special understandings, distinctive interactional competencies, and sensual attractions that motivate deviance. With respect to the worlds of street criminals, for example, this could mean all of the following: revealing interactional knowledge that is acquired only through the repeated practice of violently attacking other people (Athens 1980), describing ways of being "bad" that cannot be reduced to familiar rationalities or to conventional goals such as material gain, and conveying the animating spirits that the serious pursuit of evil may conjure up for the subjects (Katz 1988).

A complementary contribution may be achieved in the study of subjects who identify with exceptionally positive moral themes. In his studies of Catholic charismatic healers, Csordas (1994) provided a good example. Csordas does not skirt their claims of distinctive religious sensibility. He takes up the description and analysis of his subjects' distinctive processes of imagination, memory, language, and emotion, emphasizing such matters as posture and movement in ritual practices, through which the forces of charismatic healing are conjured up. Writing neither as a debunker nor as a believer, he reveals how a curative religious spirituality is embodied in particular interactional forms that are special versions of universal processes.

An ethnography that takes this third path and respects the authenticity of morally exceptional phenomena need not convey a tone of ridicule or offer the ethnographer's personal embrace of the subjects' moral perspectives. The ethnographer takes up the construction of compelling forces of deviance or respectability, or how the subjects create for themselves not only the representation of negative and positive spirits but also ways of acting, understanding, and feeling that are otherwise unobtainable.[6] Deviant or elite (or, for charismatics,

"elect") status is treated not only as authentic but also as a socially constructed framework with which the subjects must cope. Thus one looks to the deviant to learn about the special strategic interaction that one masters to cope with a stigmatized identity (Goffman 1963), and one looks to a charismatic group to understand the machinations by which a set of people can sustain commitments to beliefs in their sacred status while living within a mundane world that ridicules the group's religious and curative claims. Indeed, Csordas (1997) made understandable the appeal to middle class Americans of pentecostal religion as a way in which to add a distinctively resonant dimension to otherwise mundane aspects of everyday life. His analysis has utility for a genuinely nondebunking, nonconfirming, and yet empirically falsifiable explanation of the appeal of New Age culture in general.

THE RANGE OF WARRANTS AND
THE SILENCES THAT HAUNT ETHNOGRAPHY

The warrant is an especially troublesome methodological challenge for sociological ethnographers as compared to anthropologists. Sociological ethnographers have been more vulnerable to the question "Who needs that study?" because they have studied their own societies and, since the beginnings of academic ethnography in the early decades of this century, sociologists' own societies have maintained various rival, nonethnographic means of describing themselves, namely novels and journalistic exposés, the files of social reform agencies, interest group documentation, and official records made by governmental organizations. As similar homegrown institutions have developed for self-description in the societies studied by anthropologists, the warrants that anthropologists had become accustomed to exercising have come into question in ways that sociological ethnographers have long faced.[7]

But even when local voices rise to contest the monopolistic license that anthropologists have exercised in depicting far-flung landscapes and conveying them to a home market, the reach of local voices is relatively limited. The intended audience for the social or cultural anthropologist's writings still generally does not reside in or comprehend the language of the society being described. The anthropological

ethnographer's audience usually lacks access to many of the alternative sources of knowledge with which the sociological ethnographer must compete if he or she is to maintain a license in good standing. In effect, the anthropologist can more readily take for granted the social distance between subjects and audience that the sociological ethnographer often must construct as a substantive feature of his or her text. "Foreignness" is a magic methodological wand that anthropologists are, on the whole, still freer to wave, however ferocious the contests that have developed in recent decades over the rightful ownership of that wand.[8] Sociological ethnography more often must bootstrap its legitimacy by establishing the foreignness of a domestically located social world.

Among sociologists, the warrant is a distinctive methodological challenge for ethnographers. In part because ethnographers usually do not lay out their warrant simply and quickly in an introductory page or chapter, ethnographies often seem somehow less rigorous or scientific than studies that start by setting rival hypotheses against each other and promising a data duel that will produce a clear winner. Ethnographers routinely must finesse their way into the field, gaining support from funding agencies and dissertation committees on the queasy and inarticulate promise of documenting something that only needs documentation because no one has good evidence that it exists. In ethnographic research, the challenge of providing a warrant is largely a matter of anticipatorily responding to a series of potentially killing silences: the silence of readers who never pick up one's text in the first place because it does not address a clear controversy, the silence of readers who abandon reading because they do not find a compelling point, and the silence of readers who grasp a text only to fulfill obligations in hiring or publication review processes and are too polite or too shy of open controversy to make explicit their blunt sense of "So what?"

Another dangerous silence addresses issues of evidence. In search of an accurate account of social life, the ethnographer changes questions, approaches, methods of recording, and so on, constantly adapting research practices to find and fit the substance of inquiry. As such, ethnographic methods do not allow the researcher to answer questions about reliability, representativeness, reactivity, and replicability in the ways that have become standardized in traditions of quantitative

research based on fixed research designs.[9] The rhetoric of "proof" is habitually begged off by ethnographers.

Faced with all these forms of systematic silence, what can the ethnographer say to articulate a warrant for his or her research? In addition to exploiting the moral status of their subjects, ethnographers traditionally have turned to five other, frequently complementary justifications for the flexibility of their research practices.

HISTORICALLY EMERGENT SOCIAL PHENOMENA

In addition to the first general warrant reviewed in this article—a demonstration that the ethnographer has found meanings of people's conduct that have been kept hidden because of moral condemnation or deference—a second type of warrant is invoked when the ethnographer claims to have located historically new phenomena. "High-tech" jobs have been understood to call for ethnographies that could provide indications of the fate of working class identity in occupational contexts far different from the factory settings that social class analysis has long presumed (Halle 1984). When computer technology brought new forms of play into children's lives, ethnographers perceived the need to map out this new area of culture and interaction (Sudnow 1983). When new forms of suburban housing communities arose at midcentury, ethnographers responded to the implicit call for studies of their potentially novel patterns of social life (Gans 1967).

It should be recalled that sociological ethnography emerged in the United States in the context of the then unprecedented growth of urban immigrant Chicago. Researchers introduced study after study by suggesting that new social realities and new forms of social problems were taking shape. The need to map out new urban realities led to a collective justification of individual studies of social areas and social types as contributing to a "mosaic" of the city (Becker 1966).

In recent decades, there has been a continual succession of new social problems that have justified waves of ethnographic research. Prominent examples are AIDS (Weitz 1991), crack cocaine (Williams 1992), and an explosion in homelessness (Snow and Anderson 1993). New forms of charisma also establish strong warrants for ethnography. The wave of Christian pentecostalism in this country and in the Third World makes a compelling case for ethnographic investigation. An

unprecedented increase in the scale of social organization and eco-
nomic individualism in high-powered law firms may be confidently
taken as a warrant for a new ethnography of elite law practices. What
happens to the "professionalism" of corporate law firms when the
intimate workings of ethics of honor are challenged by the imperson-
ality that may be required to manage staffs that number in the hun-
dreds? (To find out, see Nelson 1988.) Scandals over massive bank
failures related to the sale of "junk bonds" and overmanipulation on
futures and options markets have been appreciated as warranting the
ethnographic study of the normal operations of these relatively new
social worlds of financing and investment (see Abolafia 1996).

THE A FORTIORI LOGIC OF THEORETICALLY STRATEGIC SITES

A third common form of a substantive warrant for sociological
ethnography is the ethnographer's documentation of how people in a
certain time and place are confronting exceptionally vivid interac-
tional challenges or devising rarely occurring but generally relevant
interactional solutions. In the "second Chicago school" of the 1940s
and 1950s (Fine 1995), the students of Everett C. Hughes and Herbert
Blumer seemed to intuit this warrant as a guide for their occupationally
focused ethnographic studies. Often, after entering a field site for any
number of extraneous reasons (e.g., family connections, the need to
make money) and gathering data without a clear guiding definition of
the substantive issues, an ethnographer would identify a social process
for which the site just happened to be a brilliantly strategic data source.
Research on janitors became appreciated as an especially useful focus
for information on "status dilemmas" or how people who are put in
positions of practical control manage interaction with others who are
their superiors in prestige and respectability (Gold 1952).[10] Observa-
tions of doctors became appreciated as a ground on which to test
Weberian notions of the exercise of control among status equals in a
bureaucracy (Freidson and Rhea 1963). The social world of the taxicab
driver, with its constantly recurring, brief, one-shot relations with
clients, was characterized as a wonderful place in which to study how
moral constraints limit what one might expect to be an utterly crass
performance of a mundane role (Davis 1959). All of these studies play
to the a fortiori argument that patterns of social life that are systemati-

cally present in analytically extreme circumstances also should be present, although in diluted and obscured forms, in more commonly occurring, less theoretically "pure" circumstances.

Appreciating the rich possibilities for strategic analyses, ethnographers often have focused on distinctive features of the work of people who, as a routine part of their occupational responsibilities, interact with people they label as deviant. Studies of such work often turn up practices, feelings, and cultural phenomena that are generally occurring but are especially vivid in the context of work with deviants. Thus those who work with deviants often must do what they regard as "dirty work," a category that has salience wherever work is governed by a moral division of labor (Emerson and Pollner 1976). And what work institution is not? As Hughes (1971) wrote, "It is hard to imagine an occupation in which one does not appear . . . to be practically compelled to play a role of which he thinks he ought to be a little ashamed morally" (p. 343).

Bosk (1979) explicitly considered the strategic value of studying the social management and meanings of mistakes made by surgeons in "an elite, academic environment":

> Increased accountability is generic to surgery. . . . Both formal and informal mechanisms for achieving accountability are more available in elite than in nonelite settings. . . . In elite settings the practice of surgery focuses on difficult surgical cases that represent what is presumptively the "cutting edge" of the field. . . . Where preeminence is unquestioned, there may be a greater willingness to explore the reasons for failure. . . . My site selection limits the universe to which I can generalize, but at the same time it provides a setting in which controls are both salient and displayed in their most primitive form. It allows us to see most clearly what surgeons consider an error, why they use this definition, and how they enforce it. (pp. 31-32)

One need not be a sociologist to appreciate that an ethnography is warranted when circumstances turn up an especially compelling case. Laboratory psychologists interested in studying *cognitive dissonance*, the psychological processes that emerge when belief and perception conflict, produced a celebrated ethnographic study when they came across a group of believers who were awaiting the end of the world on a specific date. Although *When Prophecy Fails* (Festinger, Rieken, and Schacter 1956) was a study of a group enthralled by charisma, cha-

risma was important to the ethnographic warrant not in its own right but rather as predictably setting up a strategic test of cognitive dissonance. In a wholly different substantive area, but with an analogous application of the ethnographic warrant, Wacquant (1995) recently showed how, for young boxers and their older trainers as well, the city gym has a fascinating charm that sustains participants' motivations far beyond what any practical calculation of personal material advantage could justify. The institutional charisma of the boxing gym[11] makes it a strategic site for a novel examination of the relationship of violence, poverty, and masculine and black identity in the contemporary inner-city ghetto.

NEEDS FOR NARRATIVES

A fourth way to warrant ethnographies is to describe people acting in ways that build previously undetected personal and communal life stories. This is perhaps the single most compelling warrant for ethnography: the telling of the story of how people, through collaborative and indirectly interdependent behavior, create the ongoing character of particular social places and practices. It may be, as some recent commentary on quantitative research suggests, that key issues of causal explanations are being finessed whenever research reports—even those that summarize huge numbers of quantified cases—are not readable as narratives; " 'cases' in standard quantitative methods . . . lose their complexity and their narrative order" (Abbott 1992, p. 53). If so, then no social research is complete without an ethnographic treatment of its subject matter.

The narrative warrant for ethnography is deeply, even existentially, underwritten. As individuals, we all may search for the naturally hidden stories of our early associations to comprehend ways of acting and understanding that became matters of habit long before we could focus self-consciously on social interaction (see, e.g., Agamben 1993). In our everyday conduct, we are routinely indifferent to the multifaceted contributions of multiple others to the thoughts, phrases, and life strategies that we refer to as our "own." As some of Garfinkel's (1967) famous "experiments" demonstrated, social life would be impossible without the artifice of such a seemingly natural egocentrism. All organizations develop investments in ways of hiding and denying

collective responsibility for the sometimes untoward results of their members' activities (Katz 1977). There are systematic biographical, social interactional, and corporate organizational reasons that stories go untold until ethnographers take on the task.

Ethnographies have a widely recognized ability to depict communal realities that become visible only when one documents the collective acts through which people subtly fit their lives together over time. Erikson's (1976) study of the consequences of a destructive flood in an Appalachian mining community and Harper's (1992) study of Willy, an upstate New York auto mechanic and farm tool repairman, are two complementary examples of exceptional narrative achievements in ethnography.

Erikson (1976) artfully evokes the loss in the flood of something that had always existed in an enigmatically invisible manner: a taciturn community of presumptive mutual understanding and support.

Harper (1992) brings out the interdependencies that are served by the work of Willy, a Saab and tractor repairman who runs a shop next to his home in an area of rural poverty in upstate New York. His clients may visit only occasionally, and then they may hang around his shop silently for hours without providing the ethnographer with much recordable data, but the ingenuity and care that Willy exercises, in a "hands-on" way that Harper conveys in the photographs that accompany his text, enter their lives in profoundly significant ways. It might be months or even years later before a client could appreciate how, in repairing a farm tool, Willy had anticipated the tests that the tool would face when put to work in an unusually demanding terrain. Willy kept his shop going by keeping decades-old tractors going. Harper's close account reveals that Willy obviously was not getting rich but was subtly sustaining a richness of associations without which much of rural social life in upstate New York could not practically survive.

SUMMARY IMAGES AND POLICY RELEVANCE

A fifth common ethnographic warrant is to transform understandings about a pattern of action, a set of people, or a social institution by describing the object of study more processually and more fully in social context than do the representations produced routinely by the people studied and more than had been the case in previous research.

The result is not simply to dismantle bad stereotypes but rather to construct good stereotypes by producing a more holistic and satisfying summary view of the subject. In revising the prevailing summary images of types of people and types of social places, ethnography at times plays a role of unappreciated significance as a vigorous form of *policy research.*

In policy discussions, quantifiable data (e.g., the percentage of recidivism within a treatment group, the costs of environmental pollution, the amount of taxes paid by illegal aliens, the frequency of illegitimate births among different sectors of the population, the rate of crime before and after penalties are changed) have great rhetorical utility. To make a significant contribution, it is sufficient to show, for example, that a given policy reduces pollution, increases the employment rates of women who have been on welfare, or reduces school dropouts and raises test scores. Who could be in favor of increasing pollution and dropout rates or of reducing test scores and the employment of low-income populations? Policy research, as conventionally defined, is geared to produce the summary indicators on which policy debates thrive.

The relative value of ethnographic research takes on a different light when we ask a question that is not commonly addressed in reports of policy research: How are policy views actually formed? Where is the evidence that policies that depend on public and official support for school financing, gun control, and immigrants' rights are in fact affected by findings on test scores, crime rates, and tax contributions? If directions of policy change routinely run independently of the signals of policy research, then perhaps there are severe restrictions on the social worlds within which policy research is treated seriously. Studies demonstrating the lack of deterrence of given increments in criminal punishment, or studies showing high rates of accidental injury from guns that are kept in the home, may define the relevant issues so narrowly as virtually to guarantee the practical irrelevance of their results to those who are not already convinced of the resulting policy recommendations. If what is more fundamentally at stake in these public debates is the meaning of guns and criminal punishment for promoting an enhanced sense of control in the everyday lives of gun owners and capital punishment supporters, then it will require ethnographic research to document these concerns in their local con-

texts, to discover their situational contingencies, and to suggest how they may be effectively altered by public policies.

If public views about public school education, immigrants' lifestyles, the motivations of criminals and gun owners, and the like all depend profoundly on stereotypes, then research may be most relevant to policy when it works to reshape the prevailing summary image of the type of person involved. The impact on policy of ethnographic research is not easily evaluated and can easily be exaggerated. Intellectual currents in ethnographic research run closely with the general history of social thought inside and outside of academia. Revisions in the images of types of peoples and places often are promoted simultaneously by ethnographic research and by changes in popular culture. But the warrant for ethnographic research as policy relevant is no less proven than is that for research that proceeds directly under that banner, and some overlap with trends in popular culture may be essential if any social research is to have rhetorical efficacy.

The case for the policy relevance of the studies of Suttles (1968) and Gans (1982) in urging a rethinking of urban renewal policies is at least as compelling as that for the generation of research that has futilely promoted gun control and attacked capital punishment. Becker's (1953) ethnographic portrait of marijuana users certainly was not independently effective in reducing confinement penalties for marijuana use; however, because it addressed the essential nature of use and the stereotype of the user in a memorable qualitative portrait, it became one of the most widely used readings in college social science courses, where it helped revise opinion on the dangers of marijuana among the more educated and, later in his mass readers' lives, the more powerful citizenry. One of the most effective policy researchers in the history of sociology was Erving Goffman, whose ethnographic portrait of "total institutions" (Goffman 1961) was a contemporaneous and scholarly complement to popular novels/movies such as Kesey's (1962) *One Flew Over the Cuckoo's Nest*. Such works helped each other's appeal, and jointly they helped reshape a generation's summary understanding of the human quality of life in mental hospitals and prisons. Goffman's writings, which were cited prominently in federal court cases that recognized constitutional objections to aspects of involuntary confinement, gave officials a re-

spectable source of authority for policy changes that were, no doubt, multiply determined.

METHODOLOGICAL EXEMPLARS

Another warrant to which an ethnography may appeal is that of serving as a methodological exemplar that shows how extraordinarily well an ethnography can be executed. The police cannot execute a search warrant with such sensitive civility that professional excellence in the execution justifies an otherwise unwarranted intervention, but sociologists may make enviable and timeless claims to professional attentions if they carry out research with unprecedented imagination. Ethnographic methodology may be exemplary because of the way in which the researcher exploits naturally occurring resources as data, the way in which data are evaluated, or the way in which data are presented. Respective examples would be Thomas's (1967) use of personal documents to study lonely young women in the big city, the weighting of fieldwork evidence by Becker et al. (1961) in their study of medical students, and Harper's (1982) use of photographs to portray the life of a railroad tramp. Studies that are exemplary for their methodology remain invaluable resources for ethnographic research training, independent of the usefulness of their studies' findings.

ETHNOGRAPHY'S WARRANTS

As an initial effort to draw attention to the subject of ethnographic warrants, this article has notable limits. I have explored at length only the warrants for studies of people and practices thought to be deviant or to deserve special deference. I tested my analysis of ethnographic warrants only against the contents of the various incarnations of the journal now known as *Contemporary Ethnography*, against books reviewed in major sociology journals in recent years, and against my bookshelves. As a set, the six warrants I have identified may not be exhaustive.

It also may be seen as a limitation that the various types of warrants specified here obviously are not mutually exclusive. Any given study usually will rest on more than one type of warrant. There is no

compelling reason why authors should separate their writings by warrants; it is common for chapters in a given monograph to speak to different warrants with the result that the ethnography as a whole rests solidly on overlapping justifications. But the potential for overlap indicates one of the advantages of being self-conscious about warrants. In selecting research topics and designing a focus in the field, ethnographers might consider that the more warrants a study can satisfy, the more effectively it may make a claim for readers' attention. If ethnographers are unlikely to discipline the design of their research so that they satisfy all possible warrants, then it still should be helpful to appreciate that they have many mutually supportive bases to speak into the silences that challenge the logic of their methods.

NOTES

1. The outstanding example of a possibly overly charming style of ethnographic writing is the work of Lévi-Strauss, who usually does not provide evidence that the cognitive systematics of his analyses of cultural texts are systematically grounded in his subjects' actual practices of storytelling, much less in their lived social relations. It is not as obvious that the neatly complementary, dialectical structures of the myths he analyzes are as charming to the peoples he has studied as they are to his readers. There is thus a disturbing character to the photographs that he took of his subjects in the 1930s (Lévi-Strauss 1995), as readers are led to wonder about the relationship between the elegant dialectical precision of the ideational structures of their myths and the flesh-and-blood reality of his subjects' lives.

2. Each of these studies, I rush to note, speaks robustly to one or more of the morally less vivid, but often intellectually more creative, warrants that are noted in the conclusion of this article.

3. Struggles with such conflicts abound in the paradoxically titled volume, *Ethnography Unbound* (Burawoy 1991).

4. One might, for example, find it warranted to document, as historically new phenomena, the social realities of an urban ethnic population that once was dominant but that has become marginalized by the population growth in the neighborhood of other ethnic groups that currently are suspect in the public's regard (Rieder 1985).

5. It is not clear whether a "vouching" process is any more or less necessary to get access to people reputed to be elite than to study people who carry deviant labels. On questions of access to elites, qualitative sociologists would do well to keep some quantitative factors in mind. The most fundamental reason that elites pose special difficulties for ethnographic study is that by definition there are relatively few of them. If one is rebuffed in the attempt to study social life on an inner-city ghetto street corner, then there are lots of alternatives. But if one wants to study the small group that presumably governs a given city, then a rebuff may be much more disturbing even if it is much less likely to occur because there is nowhere else to go.

6. The same three choices confront all areas of sociological investigation. Consider the study of sex and race. Most commonly, they are treated as *ascribed* statuses in quantitative studies

that take for granted the existence of sex and race as personal attributes and research their relationship to other features of subjects' social lives, whether those other features be how others treat the subjects or how the subjects themselves act. Less often, ethnographers treat sex and race as *achieved* statuses by describing how one learns a particular cultural version of being male or female, black, white, Chicano, and the like; analyzing how power relations press one to act in certain scripted versions of these identities; and identifying the social contingencies of acting in sex- and race-distinctive ways. The third alternative is to appreciate a personal accomplishment that might be called *the achievement of ascription*: how people come to take on not only what cross-cultural research shows to be obviously achieved features of personal identity but also natural or un-self-consciously practiced and distinctive ways in which to be, for example, male or female. This third alternative calls especially for ethnographic research because of the biographical, behind-the-scenes, personally detailed, and nuanced contextual analysis that is required to see how one comes to take for granted distinctive perspectives and sensibilities as part of one's everyday practices.

7. The genre of sociological ethnography was born only after other forms of social self-description had created an intellectual space for a contrasting form of inquiry. Before the 20th century, ethnographic work was performed by authors who were known not primarily as ethnographers but rather as writers of biographies, publishers of diaries, far-flung correspondents contributing lengthy essays to newspapers, social reformers, and the like. The early issues of the *American Journal of Sociology*, and the sources used by early American sociologists such as Robert Park and W. I. Thomas, reflected the birth of sociology on this terrain of emerging differentiation among various forms of inquiry, reflection, and commentary. As Jonathan Friedman reminds me, anthropology also was pressed to clarify a warrant for a new form of representing social life by the writings of missionaries and other prior travelers abroad. The difference between the disciplines is a matter of degree. For early ethnography in both sociology and anthropology, the metaphor of "science" was crucial for laying claim to a new intellectual field. For further discussion of differences in the development and contexts of ethnography in anthropology and sociology, see Snow and Morrill (1993).

8. See, for example, the debate between Sahlins (1995) and Obeyesekere (1992).

9. But qualitative research has its own, too often neglected answers for these methodological questions. See Katz (1983).

10. One respondent's memorable representation of the dialectic: "when you show the tenants that you have a clean character and are respectable, you can train them to be good tenants; that's what's really important in being a success" (Gold 1952, p. 488).

11. Shils (1975) extended the concept of charisma to institutions. Wacquant (1995) showed that the boxing gym is a surprisingly apt example because it is a social place that, for the young boxers and older trainers who make up its life, deserves unique respect and deference.

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