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As Everett Hughes noted, there is an "underside" to all work. Each job includes ways of doing things that would be inappropriate for those outside the guild to know. Illusions are essential for maintaining occupational reputation, but in the process they create a set of moral dilemmas. So it is with ethnographic work. This article describes the underside of ethnographic work: compromises that one frequently makes with idealized ethical standards. It argues that images of ethnographers—personal and public—are based on partial truths or self-deceptions. The focus is on three clusters of dilemmas: the classical virtues (the kindly ethnographer, the friendly ethnographer, and the honest ethnographer), technical skills (the precise ethnographer, the observant ethnographer, and the unobtrusive ethnographer), and the ethnographic self (the candid ethnographer, the chaste ethnographer, the fair ethnographer, and the literary ethnographer). Changes in ethnographic styles and traditions alter the balance of these deceptions but do not eliminate the need for methodological illusions.

TEN LIES OF ETHNOGRAPHY

Moral Dilemmas of Field Research

GARY ALAN FINE

The only safe way to avoid violating principles of professional ethics is to refrain from doing social research altogether.

—Urie Bronfenbrenner (1952, 453)

Master sociologist Everett Hughes (1971) often trenchantly noted to his apprentices that there is an "underside" associated with all work. Each job has techniques of doing things—standard operating procedures—of which it would be impolitic for those outside of the guild to know. Illusions are essential to maintain an occupational reputation. Such actions are typically hidden in the backstage regions from which outsiders are excluded. As has been said, "no one without a strong stomach should watch sausage or laws being made." The production of *good things* might not be pretty. The reality of life in an operating room, in a

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kitchen, on a baseball diamond, or in a judge's chambers is not always the stuff of heroic public images.

Illusions are necessary for occupational survival. Indeed, as umpire Hank Soar remarked in response to former professional pitcher Jim Bouton's (1970) "exposé" of professional baseball: "If we all wrote about what we know about other people, there'd be no baseball" (Bouton 1971). No baseball—not in the physical sense but on the moral plane. The world is secured on secrets.

Yet illusions have a way of growing, of laying down roots, of becoming taken for granted. This begins to be problematic when practitioners take illusions for real. It is not that practitioners operate out of cynical knowledge but, rather, they should operate with the recognition that they must make choices, which impel them to behave in ways that differ from how they would like "the general public" to assume that they behave. This is reality in a division of labor in which work lives are enacted behind gauzy curtains. Unresolvable moral dilemmas are endemic to work.

I examine the underside of qualitative methodology. In a methodology that is increasingly self-critical, self-conscious, and self-reflective, such a review is legitimate. Yet my title stings. I use the word "lies" rather than "myths" or "dilemmas" because "lies" capture better the assertion that we should be aware of the reality that we are shading in our assumptions about the world—and being provocative is sometimes a virtue. My argument is not that we can avoid these choices because occupational truth is unattainable and perhaps not even entirely virtuous. I do not suggest nor do I believe that we are a cabal of cynics but, rather, that such choices are constrained by the conditions of academic work and acceptable textual practices. Although I do not call for us to abjure all methodological or textual practices that lead to these dilemmas, I do believe that it is crucial for us to be cognizant of the choices that we make and to share these choices with readers.

Qualitative research is both more and less than its public image. We indulge in claims, assumptions, and rationalizations about the method and the analysis behind it that require close and cold scrutiny. Humans have unlimited abilities to justify their

actions through moral discourse. Further, so much of the process of fieldwork is hidden and backstage that judging texts is complex. Researchers are lone rangers, cowboys, individualists. Analysis is private, field notes are rarely available for secondary analysis, and much ethnographic writing is accepted on faith. We assure ourselves that there are good and sufficient *ethical* mandates for this secrecy. Opportunities for deception are great. Although researchers are fundamentally honest, as lawyers, clergymen, doctors, and car salesmen are fundamentally honest, everyone's goal is to permit life to run tolerably smoothly—to engage in impression management.

In discussing the 10 "lies" of ethnography, I emphasize that all workers are caught in a web of demands that compel them to deviate from formal and idealistic rules. Borrowing from Becker, Geer, Hughes, and Strauss (1961) idealism is likely a luxury in a pressured circumstance. I discern a number of images of ethnographers—mental images and images for public consumption—based on partial truths or even self-deceptions. Consider the kindly ethnographer, the friendly ethnographer, the honest ethnographer, the precise ethnographer, the observant ethnographer, the unobtrusive ethnographer, the candid ethnographer, the chaste ethnographer, the fair ethnographer, and the literary ethnographer. These are not the only images that one could examine, but in each case they are common images to which ethical and competent field researchers wish to hold. It would be an authorial fiction to allege that this set has a claim to completeness; rather, they represent a set of important concerns.[1]

To bring order to this list, I divide it, like Gaul, into three parts. The first three lies represent challenges to the "classic virtues" of ethnographers. These virtues—sympathy, openness, and honor—have been challenged by contemporary, postmodern researchers, but they remain as touchstones of how the "true" ethnographer should deal with his or her informants. They represent the standards of observational morality, grounded in both science and the Western ethical tradition. As one who has been associated with this classical, realist tradition—the *Ancien Regime*, my editors sniff—they are lies to which I am intensely

sympathetic and are positions to which many novice participant observers instantly gravitate, perhaps in their naiveté. As a result, they deserve critical attention.

A second set of issues are challenges to "technical skills." When we instruct our students how to perform the mechanics of ethnography, we insist that they be precise, observant, and passive. Unlike the first trio, ethnographers are more likely to hold to the value of these demands, particularly the first two. How could we claim that we should not be precise and observant? The challenges are less philosophical than grounded in the inevitable limits of competence.

The final set of four challenge the "ethnographic self." These are tied to discursive practices: ways of presenting oneself and one's work. Data are not properly "sociology" until they are published. If unpublished, knowledge perishes. What are the conventions by which one presents oneself to one's colleagues to appear morally upstanding and trustworthy?

Just as the list of 10 is incomplete, the means by which I divide them is inexact. Each set of categories impinges on each other. After all, everything that we learn about the research of another depends on a set of discursive practices. Technical glitches can be transformed into moral virtues or the reverse. Firm lines do not separate morality, technique, and presentation.

THE CLASSIC VIRTUES

THE KINDLY ETHNOGRAPHER

Most, if not all, ethnographers make a play for their subjects, suggesting that they are intensely sympathetic chroniclers. Most ethnographers, of whatever stripe, are quite taken by the lives of those they examine, but this is not inevitably so. Sometimes, we examine unpleasant lives, groups, and organizations—and might choose to do this with malice aforethought. The examination of disparaged groups—groups that one begins the research expecting to dislike—does occur in the social sciences (e.g., Peshkin 1986), although not as often as one might expect.[2]

This phenomenon is well-explored by Jack Douglas (1976) in his provocative *Investigative Social Research*. Douglas, more than most ethnographers, is explicit about the reality of disparaging informants and of being suspicious of the information that one receives. His powerful metaphor of the investigatory paradigm of research stems from this stance. Douglas assumes that subjects might mislead, evade, lie, and put up fronts (p. 57). Recognizing this, Douglas suggests that similar interactional tools might be legitimate for the sociologist—turnabout is fair play. The illusion of being more sympathetic than we are aids research but is deceptive. Inevitably, we must confront the "agony of betrayal" (Lofland 1971), if only because our analysis is more detached than our emotions demand.

Sometimes in the course of research, we become sympathetic to the aims of the group. For instance, some years ago I became interested in the deflection of stigma and presentation of self in social movements (Fine 1992). I attended the national conference of Victims of Child Abuse Laws, a group organized to support adults accused of child abuse and to curb the power of social workers. As a parent of two young children, this was a group with which I had some qualms about being associated, both meeting these "creeps" and in having my good name associated with theirs. Although the research was not designed to debunk the organization, I assumed that members had to defend themselves. Through a relatively brief research sojourn, I found myself convinced that some of these activists were unfairly accused and others justly labeled and that the movement as a whole had a severe problem of boundary maintenance. Although I was not a hostile researcher, I was less sympathetic than I led others to believe. Should I have confessed my suspicions, or simply have made neutral and seemingly positive statements about understanding the legal system and social services agencies from their perspective? The identity that I presented was different from the one I felt.

For politically committed researchers, investigative research has a considerable appeal (e.g., Burawoy 1991; esp. Salzinger 1991). Yet such a stance presupposes limited informed consent, in that what is being informed is less than what the subjects

would wish to know in hindsight. It is also less than what the researcher recognizes that she or he should report. Our informants have given us a "gift" (Jacobs 1980, 377), but how have we returned the favor? In research of this kind—for example, the workings of the inner circles of the Ku Klux Klan (Kennedy [1954] 1990)—we might neglect the standard ethnographic injunction to understand the world sympathetically through the informant's eyes (M. Wax 1980, 278). We have "dehumanized" and "demonized" our informants, placing them outside our moral community, in the guise of justice (Appell 1980, 355).

The researcher appears to be a kindly soul but turns out to be a "fink" (Goffman 1989, 125), a spy, an undercover agent, operating against the interests of the observed group (Johnson 1975). Even though this approach is justified in terms of its overall benefit and in light of the postmodern impulse that we will always have a political stance, it is based on a lie—a lack of kindly intentions, a hidden secret.

THE FRIENDLY ETHNOGRAPHER

Will Rogers once said—and many since have mocked—that he never met a man he didn't like. This is the claim of the qualitative researcher: Will Rogers in academic tweed. The researcher should not dislike anyone. It is the rare ethnographer who admits that this is not the case. Most researchers discover that there are individuals with whom they are incompatible. We do not like everyone that we meet—certainly not everyone that we meet in the workplace, particularly when goals and motivations conflict.

This claim covers a range of emotions and types of relationships. Many emotions stand between the ecstatically fulfilling and the brutal horrid. In reality, we find individuals with whom we are close but with whom we can maintain cordial, if somewhat distant, relationships when there is no tension in the system and when we are not aiming for conflicting goals. Many relations are "temporarily friendly." Then there are others with whom we feel acutely uncomfortable and from whom we attempt to keep our distance. Even in ethnographic research we create

elaborate rationales whereby we place ourselves in other spaces. Finally, we must honor those sacred few of whom we can say with confidence that we really do not like, that we hate. Many ethnographers uncover an occasional person of that sort—a target of dislike. Hopefully not too many or this style of research, which, after all, depends on pleasantries, would be impossible. Hated individuals are found within our ethnographic world, but in the narrative representation of that world, they often vanish. We crop them from the picture. The illusion is that we have managed our affairs sweetly and well. We do this both because we wish to present ourselves as likable and also because most researchers outside the "confessional" mode (see Johnson 1975; Van Maanen 1988) see the discussion of personal animosities as irrelevant.

This assumption of irrelevance raises a problem when our dislike stems from something that relates to the research question—in other words, when the personal dislike is not merely idiosyncratic but is connected to our orientation to the research scene. Maurice Punch (1986) has asserted, for instance, that when attempting to write about the rhetoric of a progressive English private school, he and some of the major actors came to dislike each other—dislike that grew out of conflicting goals and understandings. For observers who are driven to *attempt* to like everyone, hostility might flower when their friendly face is not accepted by some of those to whom it is offered. A spurned ethnographer can be a dangerous foe. This spurning is not necessarily idiosyncratic but can emerge directly from the conditions of research, although it has tended to be treated as an embarrassing nodule, hidden from the reader's prying eyes.

I confess to several instances in which bad feelings developed between my subjects and myself. I touch upon one instance in the methodological appendix describing my seasons studying Little League baseball in *With the Boys: Preadolescent Culture and Little League Baseball* (Fine 1987), but significantly I did not reveal this dislike within the main text itself or in articles, seemingly suggesting that although such dislike was relevant methodologically it was not relevant substantively, even though

it reflected the intensity of adult needs for justifying their activity. One coach, not of a team that I had singled out for attention, felt that I was collaborating with his rivals in the league and refused to permit me to collect the questionnaires from his boys that he had previously willingly distributed. During the season he attempted to humiliate me, as, for instance, not accepting a lineup card that another coach asked me to deliver. As a consequence, I took private pleasure writing about this man and his son in my book, although I was very careful to be "ethical" in that I never mentioned his name and excluded identifying features. Perhaps if he read the book, he might recognize himself, or perhaps his colleagues would. Those of us with access to "the media" have power that others cannot match. Our structural position as reporters places us as gatekeepers into the social world. Taunt us if you dare.

THE HONEST ETHNOGRAPHER

The grail of informed consent is at the end of the twisted road of most ethical discussions. Research subjects, many say, have a *right* to know what they are getting themselves into.[3] Such a sentiment sounds proper and has been institutionalized through a maze of federal and academic regulations. However, this advice is contrary to the writings of classic ethnographers (and other methodologists) who are concerned about "reactivity"—those who want their research "clean." Two valued goals conflict: Something must give. If subjects know the research goals, their responses are likely to be skewed.

The goal of informed consent is complicated by the ethnographic commonplace, gleaned from Glaser and Strauss's (1967; see M. Wax 1977) *The Discovery of Grounded Theory*, that good ethnographers do not know what they are looking for until they have found it: Theory is grounded in empirical investigation. This model suggests that there is truth out there that we must be careful not to pollute. Not only are we unsure of the effects of explaining our plans but often we do not know what we want until well into the research project. Many qualitative researchers must complete a Human Subjects Committee doc-

ument or a grant application that asks for the hypotheses and how they will be conveyed. Often, the only honest response is that what we are studying is Them.

The expanded version of explaining that we are studying you is to say, with considerable vague truth, that we are interested in the problems faced by people in your condition, what you do, and how you think. In many research settings, this is satisfactory, particularly when groups feel underappreciated. This explanation proved admirably suited to my research with professional cooks and amateur mycologists, both of whom felt that the public did not appreciate them: The descriptive ploy seduces many an informant. I did not have to explain *precisely* what I wanted to know, although my informants eventually made educated guesses, as I came to conclusions myself.

By "not being honest," I do not mean that ethnographers fib about their research, although they might, but rather that ethnographers shade what they do know to increase the likelihood of acceptance: placing our ease before that of our informants. In the process, we construct a web of justifications for this deception. In this sense, ethnographers use the same arguments as do those who select laboratory experimentation, claiming that the truth will systematically compromise the findings and create *demand characteristics* (Rosenthal 1966).

The controversy over the absence of honesty reached its apex in the controversy over Laud Humphreys's *Tearoom Trade*. Humphreys interviewed informants who had unknowingly participated in his ethnography of impersonal sex in public restrooms, but he did so under the guise that they were chosen through random selection. He tracked down these individuals through their license plates. In the enlarged edition of his book, Humphreys (1975) reconsidered his decision. Clearly, these individuals were interviewed under false pretenses, even though there was no evidence that they suffered harm. Throughout life we mislead others for goals that appear worthy—or if not worthy, at least convenient. One might ask why honesty should *in practice*, as opposed to *in theory*, be seen as virtuous, particularly in the absence of harm.

The vigorous and heated debate in the 1960s about the legitimacy of disguised, covert observation is a debate about informed consent. Kai Erikson (1967) pointedly criticized colleagues who entered scenes in which they had no legitimate standing, professing bogus claims to belonging. He argued that this methodology did not respect the moral stature of informants, provided misleading data, and undermined the ethical stature of the profession. He singled out for criticism research by John Lofland and Robert Lejeune (1960) in which these researchers and their colleagues attempted to explore the reaction of members of Alcoholics Anonymous to new "members" of varying social classes. The researchers "played" recovering alcoholics and dressed according to social class norms, presumably misleading members of these groups. Critics of hidden research believe that disguised observation places the researcher in the same position as an espionage agent, perhaps reflecting a lack of concern with the "right" of informants not to be deceived, particularly when the beneficiary is the deceptive researcher. Supporters, such as Judith Rollins (1985; see Reynolds 1982), suggest that hidden research does little harm and can be important in studying elites, as in her study of relations between domestics and their employers by being hired as a "maid."

How much and what kinds of explanations we provide are choices that we make from a position of power and information control. Borrowing a metaphor from the espionage community I distinguished among three strategies of information control: Deep Cover, Shallow Cover, and Explicit Cover (Fine 1980). In the first of these, Deep Cover, the researcher does not announce his/her research role. Rather, the researcher participates in the life of the group as a full member. Operating under Explicit Cover, the researcher makes as complete an announcement of the goals and hypotheses of the research as possible, not worrying if this explanation will affect behavior. The third technique, Shallow Cover, finds a middle ground. The ethnographer announces the research intent but is vague about the goals. The researcher is announced, but the research foci are not compromised. As Goffman (1989) asserted, one's story

should hold up should the facts be brought to one's informants' attention (p. 126). Such a compromise is either the best of all worlds or the worst, depending on one's orientation. These divisions, and the grey areas between them, remind us forcefully that the line between being "informed" and "uninformed" is unclear (Thorne 1980, 287) and that all research is secret in *some* ways, because subjects can never know everything (Roth 1962, 283).

TECHNICAL SKILLS

THE PRECISE ETHNOGRAPHER

A dearly held assumption is that field notes are data and reflect what "really" happened. We trust that quotation marks reveal words that have been truly spoken. This is often an illusion, a lie, a deception of which we should be aware. We engage in the opposite of plagiarism, giving credit to those undeserving—at least not for those precise words. To recall the exact words of a conversation, especially if one has not been trained in shorthand or as a court reporter (and not even then, as stenographers and court reporters attest), is impossible. This is particularly applicable for those who wish to maintain the illusion of "active membership" or "complete membership" (Adler and Adler 1987) by not taking notes within the limits of the public situation. We snicker at a hoary joke about a participant observer, noted for his small bladder, who made frequent visits to the john. There the researcher furtively and rapidly inscribed his observations. We maintain an illusion of omniscience by recreating a scene with attendant bits of talk—skating on ever thinner ice.

In such situations, we become playwrights, reconstructing a scene for the insight of our readers, depicting ongoing events in our minds (Bartlett 1932): turning near-fictions into claims of fact. Notetaking and writing demand transformation and recontextualization. We claim that the scene really happened, but the

scene did not happen in precisely the form we announce. We are like those popular biographers who, in order to make a scene compelling and "real," create dialogue that is "likely" to occur and that, in the process, supports our own arguments and morals. The dialogue is not accurate in that an attestation that these "precise" words were said is futile. One would need a gifted, encyclopedic ear: an ear never seen. When conscientiously compiled, the quotations are both true and false. They are true in that, with conscientious researchers, they represent something "along the lines" of what was said—transformed into our own words that we place in a methodologically unsanitary way in the mouths of others.

In teaching qualitative methods I assign my graduate students an in-class exercise: A pair converse informally for a minute or two, and once the conversation has been completed, I ask all students to write the exact words spoken as best they can. When I play the tape, students discover to their chagrin that although they might have captured the "gist" of the conversation, which had ended only moments before, they have not remembered the words themselves. Some students have better recall than others, but none are near perfection. This underlines my point that details of quotations and descriptions of behaviors are approximations, signposts, and minidocudramas. We make our informants sound like we think they sound, given our interpretations of who they "really" are (Atkinson 1992, 26-27).

In most of my observational research, I kept my field notebook in plain view, perhaps decreasing errors, and possibly distracting my informants. Even so, I know from a few transcribed tapes of fantasy role-playing games that the materials I presented when relying on field notes are not exact quotations. If they are not what I *wanted* to hear, neither are they what I did hear.

The illusion of verisimilitude is crucial for the grounding of qualitative research. We embrace its rich precision. The belief that this is "real life," not fiction or guesswork, provides a methodological charter for participant observation. This depiction of reality gives ethnography an advantage over survey research, experimentation, and other techniques, but it is a belief that is at best only approximately true.

THE OBSERVANT ETHNOGRAPHER

We assume that the report of a scene is "complete": that there exists a reasonably precise correspondence between what is said to have occurred and what "actually" happened. A related belief is that little of importance was missed—at least when the ethnographer was present. But suppose that this comforting belief is not accurate, suppose that the picture painted is missing critical details. The ethnographer might not have been sufficiently observant. The ethnographic picture will always lack detail and shading, and sometimes these absences are material in that other ethnographers might have reached sharply different conclusions from highlighting other material.

On the surface, this criticism primarily targets "bad ethnography"—most agree that ethnographers differ in skills. Science fiction writer Theodore Sturgeon allegedly noted in response to claims that most science fiction is of poor quality that "90 percent of science fiction is crap, but then 90 percent of everything is crap." Following Sturgeon's "law," 90% of all ethnography is crap. Although we should dispute the numbers and should be wary of transforming quality into a dichotomous variable of "crap/not crap," the point remains. However, we must transcend this chilly, assertion of scholarly incompetence to recognize that the ability to be totally aware is imperfect. We mishear, we do not recognize what we see, and we might be poorly positioned to recognize the happenings around us. Consider those wry anecdotes told on our children of when they misheard some common phrase and transformed it in comic ways, such as the child who (mis)hears the first line of the national anthem as "José, Can You See?" Ethnographers, particularly when newly observing novel scenes, are like that amusing 5-year-old. Everything is capable of multiple interpretations, and *mis*understandings stem not from incompetence but from competencies in other domains. Some things we do not see because we simply are not trained or situationally knowledgeable. Paul Stoller's (1989) rich ethnography of the Songhay of Niger, *The Taste of Ethnographic Things*, reminds us that we rely on our visual and auditory senses to the neglect of touch, smell, and taste. We are

not observant—the very skill on which competent participant observation is supposedly (and actually) based. This weakness is inescapable.

A further cause of being unobservant results from personal, temporal, and situational pressures. We know how stressful participant observation can be even in the best circumstances. Hours and hours of observations are followed by hours and hours of composing one's field notes. When I was conducting research with fantasy role-play gamers—who played *Dungeons & Dragons*—I would occasionally spend the lengthening hours from 7 in the evening until 4 the following morning with these young men. It would have required a very dramatic event to capture my analytic attention in the wee hours of a long night. Perhaps I should admit, more honestly, that for much of the time I was simply present, barely monitoring what transpired among these gamers. My powers of observation were substantially decreased. When I drank or puffed marijuana with research subjects, my powers of concentration were altered for the worse and better. When I had a vexing day at the university or a dispute with my wife, my concentration diminished. Researchers who bring their children into the field must cope with multiple distractions (Cassell 1987). How could it be otherwise? What I noticed, and my ability to take notes varied. As we know from straining to decipher scribbled field notes, sometimes we simply do not type all of the things we have noted, or worse, we cannot read our own writing. Some ethnographers, in fact, do not write field notes, trusting instead in their memory. One claimed, memorably, "I am a fieldnote!" (Jackson 1990, 21).

The ability to be observant varies, and we should not assume that what is depicted in the ethnography is the whole picture. Obviously for reasons of space, events are excluded, but much is excluded because it passed right under our nose and through our ears and because our hands were too tired to note the happening.

THE UNOBTRUSIVE ETHNOGRAPHER

Most "textbooks" on qualitative research emphasize that an observer should influence the scene as little as possible (e.g.,

Taylor and Bogdan 1984). Underlying this attitude is the principle that the researcher should not truly become a "participant" observer. After all, what would we learn if researchers burst into a social scene and immediately took charge, pushing events in directions in which they would not otherwise have gone? Although this would *still* be a social environment, it might not be the environment one had planned to examine. Too great an involvement in a social scene can transform an ethnography into a field experiment.

Yet, recognizing that the researcher should not direct a scene, one might also wonder whether competent, active observers do not and should not have influence. Ultimately, the methodological goal is to become a full member of a scene: to "settle down and forget about being a sociologist" (Goffman 1989, 129). How is this possible when one is just an observant piece of furniture? Over time, I have chosen—perhaps only to make life easy, perhaps not—to recognize my participatory desires. Although I still attempt not to put too fast a spin on a setting, I add myself to the mix, and I attempt to understand how I feel as a participant. Among mushroom collectors, I did compete with my informants for the best patches and pointed out specimens if of fungal worth. The degree to which one is an "active member" affects the extent to which this sympathetic understanding is possible, and this is a function of one's social location: I had far more success in being a member as a fantasy role-play gamer and as a mushroom collector than as a Little League baseball player or a professional cook. Once when observing high school debaters, I recalled through sympathetic introspection what these young men and women were experiencing, using emotion to my own end. Kleinman (1991), in describing her research on a holistic health center, made us recognize that our emotions, as they arise in field settings, directly influences what we see, how we get along with others, and the strategic choices that we make in our ethnographies.

We can never be a cipher. Every group is a collection of personalities and styles. As a consequence, the presence of an observer should not be too worrisome, as long as the impact is not excessively directive or substantive.

THE ETHNOGRAPHIC SELF

THE CANDID ETHNOGRAPHER

Ethnographers differ little from Erving Goffman's social actors; they rely upon impression management. Although Goffman (1989) has proposed that a good ethnographer must be willing to look like a "horse's ass" (p. 128) this is easier said than done, particularly as advice coming from one whose own self is carefully hidden in his own ethnographies (Fine and Martin 1990).

No one wishes to look "bad," and as a consequence, much information—unknown to the reader—is censored by a self-concerned ethnographer. One frequently encountered technique for this defense of the self is the fly-on-the-wall model: an ethnography without ethnographer—the fully unobtrusive ethnographer, as described above. This technique has been most dramatically perfected by *The New Yorker* magazine: Its ethnography, such as Stephen King's (1990) description of Little League baseball, has no observer. Much journalism operates on this claim, not just of objectivity but on the more radical belief that, in Edward R. Murrow's terms, "You Are There." The illusion is that everything reported has actually happened because you have been "directly" exposed to it. This illusion can be recognized for what it is when the writer relies on the passive voice, indicating that someone "was asked," attempting to elide the reality that the asker was the writer. The literary claim is that the asking happened "naturally."

The question ultimately becomes who is the "who" in the system? How many imperfections is one going to choose to report? How much is relevant for public consumption, particularly as it relates to the embarrassing actions of the researcher, as discussed in the earlier sections of this article? The issue of what and how much to report does not have any "right" or eternal answers. Answers are always grounded in choices, wherein the cynic can claim, as I do here, that the researcher is either not being candid or is overglorifying the self in a report that none but one's relatives might choose to read. Whatever choice is

made is not entirely theoretical. We cannot disentangle the personal demands of presentation of self—how one will appear to others—from the question of what one should do "in the name of science." Being candid becomes a situated choice that is forever linked with how the candor is likely to affect one's reputation as a scholar. We have our careers to think of, and issues of honesty and ethics must be analyzed within this personal nexus (Barnes 1979, 179). One hopes that one does good by doing well.

Recent experimental attempts to move oneself into the center of one's ethnography can no more escape the dilemmas of exposing one's candor than can attempts to pretend that one wasn't there at all. New techniques of ethnographic description demand the same bracketing of candor as does the claim of the absent ethnographer. In discussing my attempts experimentally to manipulate fantasy games to uncover levels of "fantasy violence" (Fine 1983, 251), I selected instances that I felt made the points I wished to make while simultaneously making myself seem competent as player and ethnographer. One cannot escape the reality that the presentation of one's own role is invariably an exercise in tact. There always is a reader looking over a writer's shoulder.

THE CHASTE ETHNOGRAPHER

One of the dirty little secrets of ethnography, so secret and so dirty that it is hard to know how much credence to give, is the existence of saucy tales of lurid assignations, couplings, trysts, and other linkages between ethnographers and those they "observe." The closest that we come to this in the published record is the examination of the opposite side of the mirror: cases in which female ethnographers are harassed by male subjects (e.g., Conaway 1986; Easterday, Papademas, Schorr, and Valentine 1977; Hunt 1984; R. Wax 1979). These obnoxious and brazen attempts at sexual acquaintanceship are part of the territory in a sexist world. Why should the female ethnographer be treated differently from any other female? One wonders, therefore, about male ethnographers and their fe-

male informants—are academics more moral than other social groupings?

We hear spicy whispers about ethnographers—typically, anthropologists in distant and storied realms—who "go native." The ethnographer is so taken that he or she decides to remain embedded in that place. This decision is often linked to love or marriage, and anthropology initiates are specifically warned about this hazard (Conaway 1986, 53). Marriage might represent the validated, intense commitment to that scene the ethnographer desires. Goffman (1989) sardonically remarked that you realized that you have become incorporated into a scene, when "the members of the opposite sex . . . become attractive to you" (p. 129).

Just as long-term relationships arise, so do brief encounters—equally passionate, even if limited in time and space. Humans are attracted to each other in all domains. They look, they leer, they flirt, and they fantasize. The written record inscribes little of this rough and hot humanity. Admittedly, such relations do not always transpire. I cannot admit to more than a few looks and thoughts, but others can. Occasionally, one finds an honest, if careful, anthropological account written about a distant outpost. Paul Rabinow's (1977) account of intimacy with a Berber woman in Morocco is well known:

> Ali took me into the next room and asked me if I wanted to sleep with one of the girls. Yes, I would go with the third woman who had joined us for dinner. Before we left the house, Ali took me aside, and shuffling, said that he had promised to pay her but he didn't have any money. Everyone wished everyone a fine night, and we left. We did not say more than a few words to each other. My few Arabic expressions became garbled and confused in my mind. So, silently and with an affectionate air, she indicated that I should sit on a low pillow while she made the bed. . . . The warmth and non-verbal communication of the afternoons were fast disappearing. This woman was not impersonal, but she was not that affectionate or open either. (Pp. 68-69)

One admires the tact with which the passage is written. Rabinow only implies that he agreed to sleep with this woman, never writes that they had intercourse, and explains that this woman

was a prostitute provided by his "real" informant, not an informant herself. His ethnographic relations were not defiled. He does not address whether Berber sexual activity differed from his experiences in Chicago. For him, this evening was not *data*. The scene dissolves as in old Hollywood films: PG ethnography.

Similarly careful is Colin Turnbull (1986), whose apparent sexual liaison with a Mbuti woman, sent to him by her father, the tribal chief, is described obliquely and presented to explain how he carved out his social identity (pp. 24-25). Dona Davis (1986), studying menopause in a Newfoundland fishing village, is coy about her sexual relationship with another stranger in the community, an engineer working on the water system (pp. 253-54). She discussed how this man fulfilled her "private needs" and discusses in some depth the reactions from villagers, but their interaction is not data.

Ethnographers value and demand their privacy. This privacy is surely understandable and, from the ethnographer's perspective, no doubt quite desirable and defensible. Sexual contact stigmatizes the writer, particularly female writers (Whitehead and Price 1986, 302). We are to create science, not porn. Malinowski's (1967) diaries were only published posthumously and a rare book about a female anthropologist and her relations with a local male appears under a pseudonym (Cesara 1982, 55-56). The taboo on including these data misleads a naive reader about the emotional and personal qualities of this methodology. Participant observation is a methodology in which the personal equation is crucial, and yet too many variables remain hidden. The question is whether we can preserve our privacy while we reveal the impact and relevance of our behavior, both private and public. Where is the balance?

THE FAIR ETHNOGRAPHER

What does it mean to be fair? Is fairness possible? The label "fair" can consist of two alternative meanings: that of *objectivity* or that of *balance*. Each is problematic, and each is far from universal in qualitative research narratives. Some suggest that they should not even be goals.

Qualitative researchers need not be warned about the difficulty, if not the impossibility, of pretending objectivity. Objectivity is an illusion—an illusion snuggled in the comforting blanket of positivism—that the world is ultimately knowable and secure. Alas, the world is always known from a perspective, even though we might agree that often perspectives do not vary dramatically. The new ethnographic movement, originating in anthropology in the writings of James Clifford and his colleagues, has steadily spread outside of that domain into other arenas of ethnographic work—for instance, education and sociology (Atkinson 1992; Gubrium 1988). Few ethnographers accept a single objective reality, but in realist ethnographies (Van Maanen 1988), such a doubt is not explicitly stated. Indeed, the illusion is quite the reverse. So, my study of Little League baseball masquerades as informing the outsider about the "real facts" of this hidden social world, without my being self-conscious (except in the appendix) about my role in this doing. I ask and demand your trust, even while my theories of child rearing and my own fitful and unsuccessful experiences as a young athlete are discretely ignored. I presented myself as an "honest broker"—an individual with nothing to hide and everything to share. I could be trusted to parse the facts. This claim helped my professional reputation for responsibility while ignoring my romanticism of a sitcom suburban life I never shared.

In that it ignores the motives and themes of the researcher in interpreting what we call "reality," accepting an image of fairness in the name of objectivity is misguided. However, excising such a claim does not solve the problem. The response, embracing subjectivity, is also problematic. The reality of occupational backstages is that values will inevitably come into conflict. By admitting one's perspective and/or by seeing the world in terms of ideology and narrative, we wear a mask of openness but without doing justice to all the ways in which a setting might be understood. We have not presented the diversity of worldviews because we are, by nature, an "interested party," whose definitions of the worldviews available will be distorted by what we can see and by our unwillingness to accept that, for our participants, objectivity exists—in practice, if not in fact. My point is

not that this can be avoided but, rather, that we should come out more forcefully and admit the paradox. As Margery Wolf (1992) demonstrates in *A Thrice Told Tale,* the same set of events can be understood quite differently through different sets of discursive practices. We simply must make presentational choices.

This realization becomes particularly salient for ethnographers engaged in "policy relevant" or qualitative applied research: a branch of qualitative research that expanded in the 1980s (Estes and Edmonds 1981; Loseke 1989). Perhaps the classic example of "motivated ethnography" is Kai Erikson's (1976; see Glazer 1982, 62) *Everything in Its Path,* an ethnographic examination of the aftermath of a dam collapse in the Buffalo Creek area of West Virginia. Erikson represented a law firm that was attempting to sue the mining company for negligence; his task was to collect data to this end. This does not mean that Erikson was dishonest in his report but, rather, that his perspective channeled the data that he collected (and couldn't collect) and oriented his interpretations. However, policy issues need not be central to the research for selection and "self-censorship" of data to be an issue. Data are never presented in "full," and choices are inevitable. In protecting people, organizations, and scenes, we shade some truths, ignore others, and create fictive personages to take pressure off real ones (Adler and Adler 1993; Warren 1980). A colleague once informed me that he shaved data that might harm the public perception of the ethnic grouping with which he was in sympathy, feeling that they had enough trouble without having to confront his truth.[4] Car salesmen, clergymen, politicos, and participant observers massage the realities they share with their audiences.

Participant observation often becomes participant intervention: Finding a problem, we wish to fix it. Identifying with our informants *in loco parentis* we wish to take their side (Barnes 1979, 171), to protect them from harm, and make everything right. This human reality suggests that qualitative evaluation research, like all evaluation research, is always "contaminated" by the perspective that the researcher brings to the question and by the emotions generated in the field. Although this is

inevitable, and connected to evaluation in general (answers depend on questions), the researcher must admit the lack of "fairness" while alleging that this lack is evident in all policy claims.

THE LITERARY ETHNOGRAPHER

Ethnography is nothing until inscribed: Sensory experiences become text. The idiosyncratic skills of the ethnographer are always evident, and nowhere is this more apparent than in the literary production of ethnography. Each ethnography is an attempt to fit a world into a genre (Atkinson 1992, 29-37) and to make the account seem like a competent version of the "kind of thing" that this genre should entail. This is the heart of the textual practice of the qualitative researcher.

Inscription is dangerous for all writers (Fine 1988; Fine and Kleinman 1986)—those that are "bad" and those that are "good." For the bad writers, the problem is in keeping the interest of one's readers, assuming that one is able to get published. One must insure that the writing is not so muddled that the intentions of the author gets lost or that the author becomes so verbose that the reader gets lost (Richardson 1990). Bad writing, assuming that we can define it, is a rather simple problem. Teaching social scientists to write, while not easy, is at least something that we know how to do.

But what about writers who are not burdened by literary incompetence? Many writers write well but do so in a language that is not easily translatable for those outside the community. Postmodernists and radical feminists express themselves fluently, but not enough of their readers have acquired an easy sense of what things mean in their texts. These authors belong to a different universe of discourse from much of their potential audience. Other writers might write so well, in conventional terms, that the reader is more taken by the writing than by the substance. The writing can hide a lack of evidence, as it sometimes does in quasi-popular works (see Becker 1986). One of the most influential ethnographies of the past decade is Arlie Hochschild's (1983) estimable *The Managed Heart*. It is surely

effective prose. Yet it is not richly ethnographic and is limited for that reason. She has not provided enough data for readers to judge the lives of stewardesses from whom she generalizes. She writes too well and shares too little.

Then there are those who write ethnography as poetry—Dan Rose's (1987) study, *Black American Street Life* (see also Rose 1990) comes to mind: impressionist ethnography (Van Maanen 1988). The problem confronting Rose's reader is to determine through the web of the literary text what he means, what he wants us to think he means, or at least what we are learning. Using this technique Rose means for us to confront his images, but sometimes, as readers, our minds become heavily confused. The writing carries too much meaning, and inevitably meaning gets shuffled and is imprecise.

CLOSING: OPENING LIES

All trades develop a body of conceits that they wish to hide from those outside the boundaries of their domain; so it is with ethnographers. I do not denigrate our common enterprise but, rather, specify what we can and cannot claim. In which cloaks can we wrap ourselves? Limits remain to what we do—obdurate limits—and we must not be blind to these limits: Let us open our conceits to ourselves and our readers. A tension exists in my arguments: Am I suggesting that we produce *better* ethnography, or should we embrace our frailties? Do I provide advice or succor for inevitable failings (John Van Maanen, personal communication, 1992)? Like most cheery cons, I do both. As a psychoanalytic son, I believe in the maxim "know thyself" more than I believe in "better thyself." By knowing oneself, one can improve *a bit*, but more significantly, one can recognize that the limits of the art are part of the data. Some of the lies are more "foundational" than others in that one can hope to be reasonably observant and precise, whereas protecting one's self from harsh critique is central to one's professional standing.

My goal is not to expose the sins of individual others so much as it is to expose the claims of our collectivity. Yet, sometimes,

as in the April 1992 *Journal of Contemporary Ethnography* special issue on William Foote Whyte's *Street Corner Society*, an account of life in the North End of Boston, ethnographers attempt to debunk previous efforts, being debunked in their turn. Some Boston brownstones are glass houses. Although some value exists in exposing the flaws of others and challenging the nature of those truth claims, the greater good is to explore process and theory rather than to critique description.

Perfection is professionally unobtainable. These lies are not lies that we can choose, for the most part, not to tell; they are not claims that we can avoid entirely. We must suffer the reality that they are part of the methodology by which we prepare a reality for a transformed presentation. Ethnography is ultimately about transformation. We take idiosyncratic behaviors, events with numerous causes, which may—God forbid!—be random (or at least inexplicable to us mortals), and we package them. We contextualize events in a social system, within a web of meaning, and provide a nameable causation. We transform them into meaningful patterns, and in so doing, we exclude other patterns, meanings, or causes. Transformation is about hiding, about magic, about change. This is the task that we face and is the reality that we must embrace. We ethnographers cannot help but lie, but in lying, we reveal truths that escape those who are not so bold.

NOTES

1. The core, burning truth here, as with many category systems, is that they happen to represent what an imperfectly analytical and creative author happened to think of while preparing the article. Rewriting and sharing the revisions with others changed some topics, leaving the final set as the "official" list.

2. This has been a particularly salient issue in social movement research, in which there are "good" and "bad" social movements, which are often studied differently. Civil rights groups, gay rights movements, and pro-choice lobbies are treated quite differently and with more frequency than are groups that are racist (arguing for civil rights for majority groups), homophobic (arguing for family values), and anti-choice (arguing for the sanctity of human life). For an extreme, if justified, example of how to observe a despised group, examine crusading journalist Stetson Kennedy's ([1954] 1990) *The Klan Unmasked.*

3. The notion of a "right" to informed consent represents another in the long series of expansions of rights in modern society about which many have written (McIntyre 1984; M. Wax 1982). Are there truly any rights to be told the truth in the sociopolitical sense of natural rights?

4. The observant reader will not have missed the fact that I have shaded his identity and ethnicity. The truly cynical might wonder whether—here—I use the male pronoun to cover both genders.

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