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RETHINKING OBSERVATION

From Method to Context

◆ Michael V. Angrosino and
Kimberly A. Mays de Pérez

◆ *Observation: Basic Assumptions*

Observation has been characterized as "the fundamental base of all research methods" in the social and behavioral sciences (Adler & Adler, 1994, p. 389) and as "the mainstay of the ethnographic enterprise" (Werner & Schoepfle, 1987, p. 257). Even studies based on direct interviews employ observational techniques to note body language and other gestural cues that lend meaning to the words of the persons being interviewed. Social scientists are observers both of human activities and of the physical settings in which such activities take place. Some such observation may take place in a lab or clinic, in which case the activity may be the result of a controlled experiment. On the other hand, it is also possible to conduct observations in settings that are the "natural" loci of those activities. Some scholars have criticized the very concept of the "natural" setting, particularly when fieldwork is conducted in Third World locations (or in domestic inner-city sites) that are the products of inherently "unnatural" colonial relationships (Gupta & Ferguson, 1996c, p. 6), but the designation is still prevalent throughout the literature. In that case, it is proper to speak of "naturalistic observation," or fieldwork, which is the focus of this chapter.

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Observations in natural settings can be rendered as descriptions either through open-ended narrative or through the use of published checklists or field guides (Rossman & Rallis, 1998, p. 137; see Stocking, 1983a, for a historical overview of this dichotomy). In either case, it has generally been assumed that naturalistic observation does not interfere with the people or activities under observation. Most social scientists have long recognized the possibility of the observer's affecting what he or she observes, but careful researchers are nonetheless supposed to adhere to rigorous standards of objective reporting designed to overcome that potential bias. Even cultural anthropologists, who have usually thought of themselves as "participant observers" and who have deliberately set out to achieve a degree of subjective immersion in the cultures they study (Cole, 1983, p. 50; Wolcott, 1995, p. 66), still claim to be able to maintain their scientific objectivity. Failure to do so would mean that they had "gone native," with their work consequently rendered suspect as scientific data (Pelto & Pelto, 1978, p. 69). The achievement of the delicate balance between participation and observation remains the ideal of anthropologists (Stocking, 1983b, p. 8), even though it is no longer "fetishized" (Gupta & Ferguson, 1996c, p. 37). Objectivity remains central to the self-images of most practitioners of the social and behavioral *sciences.* Objective rigor has most often been associated with quantitative research methods, and so important has been the harmonization of empathy and detachment that even those dedicated to qualitative methods have devoted considerable effort to organizing their observational data in the most nearly objective form (i.e., the form that looks most quantitative) for analysis (see, e.g., Altheide & Johnson, 1994; Bernard, 1988; Miles & Huberman, 1994; Silverman, 1993).

Adler and Adler (1994) have, in fact, suggested that in the future, observational research will be found as "part of a methodological spectrum," but that in that spectrum, it will serve as "the most powerful source of validation" (p. 389). Observation, they claim, rests on "something researchers can find constant," by which they mean "their own direct knowledge and their own judgment" (p. 389). In social science research, as in legal cases, eyewitness testimony from trustworthy observers has been seen as a particularly convincing form of verification (Pelto & Pelto, 1978, p. 69). In actuality, the production of a convincing narrative report of the research has most often served as de facto validation, even if the only thing it validates is the ethnographer's writing skill and not his or her observational capacities (Kuklick, 1996, p. 60).

Whatever else may be said about the postmodernist turn in contemporary studies of society and culture, its critique of assumptions about the objectivity of science and its presumed authoritative voice has raised issues that all qualitative researchers need to address.[1] Earlier criticism might have been directed at particular researchers, with the question being whether they had lived up to the expected standards of objective scholarship. In the postmodernist milieu, by contrast, the criticism is directed at the standards themselves. In effect, it is now possible to question whether observational objectivity is either desirable or feasible as a goal. James Clifford (1983a), who has written extensively and critically about the study of culture and society, has called into question even the work of the revered Bronislaw Malinowski, the archetype of the scientific participant observer, who, according to Stocking (1983a), is the scholar most directly responsible for the "shift in the conception of the ethnographer's role, from that of inquirer to that of participant 'in a way' in village life" (p. 93). Perhaps more surprisingly, Clifford has also questioned the research of the very influential contemporary interpretivist Clifford Geertz; he takes Geertz to task for suggesting that through empathy, the ethnographer can describe a culture in terms of the meanings specific to members of that culture. In other words, the ethnographer, as a distinct person, disappears—just as he or she was supposed to do in Malinowski's more openly positivistic world. This assessment is echoed by Sewell (1997), who points out that Geertz did not expect fieldworkers to "achieve some miracle of empathy with the people whose lives they briefly and incompletely share; they acquire no preternatural

capacity to think, feel, and perceive like a native" (p. 40). The problem is not that Geertz failed to achieve some sort of idealized empathic state; rather, the question is whether such a state is even relevant to ethnographic research, and whether it is desirable to describe and/or interpret cultures as if those depictions could exist without the ethnographer's being part of the action.

The postmodernist critique, which emphasizes the importance of understanding the ethnographer's "situation" (e.g., his or her gender, class, ethnicity) as part of interpreting the ethnographic product, is particularly salient because the remote, traditional folk societies that were the anthropologists' stock-in-trade have virtually disappeared; most cultural anthropology is now carried out in literate societies that are part of global communication and transportation networks. Like sociologists, anthropologists now "study up" (i.e., conduct research among elites) almost as often as they study the poor and the marginalized. Doing so overcomes some of the problems associated with the lingering colonialist bias of traditional ethnography (D. L. Wolf, 1996, p. 37), but it raises new issues regarding the position and status of the observational researcher. For one thing, ethnographers can no longer claim to be the sole arbiters of knowledge about the societies and cultures they study, because they are in a position to have their analyses read and contested by those for whom they presume to speak (Bell & Jankowiak, 1992; Larcom, 1983, p. 191). In effect, objective truth about a society or a culture cannot be established, because there are inevitably going to be conflicting versions of what happened. Sociologists and other social scientists were working in such settings long before the anthropologists came on the scene, and were already beginning to be aware of the problems inherent in claiming the privilege of objective, authoritative knowledge when there are all too many "natives" ready and able to challenge them. As Margery Wolf (1992) wryly comments: "We can no longer assume that an isolated village will not within an amazingly short period of time move into the circuit of rapid social and economic change. A barefoot village kid who used to trail along after you *will* one day show up on your doorstep with an Oxford degree and your book in hand" (p. 137). The validity of the traditional assumption, that the truth can be established through careful cross-checking of ethnographers' and insiders' reports, is no longer universally granted, as contemporary social and behavioral scientists are increasingly inclined to expect differences in testimony grounded in gender, class, ethnicity, and other factors that are not easy to mix into a consensus. Ethnographic truth has come to be seen as a thing of many parts, and no one perspective can claim exclusive privilege in the representation thereof. Indeed, the result of ethnographic research "is never reducible to a form of knowledge that can be packaged in the monologic voice of the ethnographer alone" (Marcus, 1997, p. 92).

Some ethnographers (of various disciplines) have responded to this new situation by revising the ways in which they conduct observation-based research and present their analyses. No longer can it be taken for granted that ethnographers operate at a distance from their subjects. Indeed, the very term *subject*, with its implicit colonialist connotations, is no longer appropriate. Rather, there is said to be a *dialogue* between researchers and those whose cultures/societies are to be described.[2] Discussions of ethnographers' own interactions, relationships, and emotional states while in the field have as a result been moved from their traditional discreet place in acknowledgments or forewords to the centers of the ethnographies themselves. Although this practice has certainly opened up new horizons in ethnographic reportage, it raises further issues of its own. For example, because it is likely to be the ethnographers who write up (or at least collate or edit) the results of the field studies, do they not continue to claim the implicit status of arbiters/mediators of social/cultural knowledge (Wolf, 1992, p. 120)? Ethnographers may assert that they represent the many voices involved in the research, but we still have only their assurance that such is the case.

Nonetheless, we now function in a context of "collaborative" research. *Collaboration* no longer refers only to the conduct of multidisci-

plinary teams of professional researchers; it often means the presumably equal participation of professional researchers and their erstwhile "subjects" (Kuhlmann, 1992; D. L. Wolf, 1996, p. 26). Matsumoto (1996), for example, sent a prepared list of questions to the people she was interested in interviewing for an oral history project. She assured them all that any questions to which they objected would be eliminated. The potential respondents reacted favorably to this invitation to participate in the formulation of the research design. As such situations become more common, it is important that we rethink our received notions about "observation"—what it is, how it is done, what role it plays in the generation of ethnographic knowledge. To that end, it might be useful to shift from a concentration on observation as a "method" per se to a perspective that emphasizes observation as a context for interaction among those involved in the research collaboration.

◆ *Observation: The Classic Tradition*

As a prelude to an exploration of observation-as-context, we will briefly review the traditions of observation-as-method that form the basis of our exercise in "rethinking." Conscientious ethnographers have, in fact, long been aware that in naturalistic settings, the interaction of researcher and subjects of study can change behaviors in ways that would not have occurred in the absence of such interaction. They have believed, however, that it is both possible and desirable to develop standardized procedures that can "maximize observational efficacy, minimize investigator bias, and allow for replication and/or verification to check out the degree to which these procedures have enabled the investigator to produce valid, reliable data that, when incorporated into his or her published report, will be regarded by peers as objective findings" (Gold, 1997, p. 397). True objectivity has been held to be the result of agreement between participants and observers as to what is really going on in a given situation. Such agreement has been thought to be attained through the elicitation of feedback from those whose behaviors were being reported. Ethnography's "self-correcting investigative process" has typically included adequate and appropriate sampling procedures, systematic techniques for gathering and analyzing data, validation of data, avoidance of observer bias, and documentation of findings (Clifford, 1983b, p. 129; Gold, 1997, p. 399). The main difference between sociological and anthropological practitioners of ethnography seems to have been that the former have generally felt the need to validate their eyewitness accounts through other forms of documentation, whereas the latter have tended to use participant observation, "relatively unsystematized" though it may be, as the ultimate reality check on "all the other, more refined, research techniques" (Pelto & Pelto, 1978, p. 69).

The possibility of "observer bias" looms large in the thinking of both sociologists and anthropologists in the ethnographic tradition (Werner & Schoepfle, 1987, p. 259). Even setting aside the expected distortion of ethnocentrism (which can presumably be controlled for as long as the ethnographer is conscious of it), the plain fact is that each person who conducts observational research brings his or her distinctive talents and limitations to the enterprise; therefore, the quality of what is *recorded* becomes the measure of usable observational data (because it can be monitored and replicated) rather than the quality of the observation itself (which is, by definition, idiosyncratic and not subject to replication). Although theoretical or conceptual frames of analysis inevitably direct observers' observations, it was traditionally assumed that researchers could keep these in the background when recording basic observational data. For this reason, the emphasis was placed on observational *methods,* the basic theme of which was, as one important manual of field procedures puts it, "Primary reporting of concrete events and things in field work should proceed at as low a level of abstraction as possible" (Pelto & Pelto, 1978, p. 70). Theoretical analysis was therefore an epiphenomenon to the process of observation.

According to Gold (1958), the sociological ethnographers of the first half of the 20th century often made implicit reference to a typology of roles that might characterize naturalistic research: the complete participant (a highly subjective stance whose scientific validity was suspect), the participant-as-observer (only slightly less problematic), the observer-as-participant, and the complete observer. The complete observer was one who was to all intents and purposes removed from the setting, and who functioned without interacting in any way with those being observed. Because of the difficulty of maintaining the purity of such a stance (Werner & Schoepfle, 1987, p. 259), and because such research was sometimes conducted without the informed consent of the observed (an ethical lapse that is no longer tolerated by responsible social researchers), the observer-as-participant role was considered an acceptable compromise, allowing the researcher to interact "casually and nondirectively" with subjects; the researcher remained a researcher, however, and did not cross over the line into friendship (Adler & Adler, 1994, p. 380). Perhaps the most important contemporary use of this role is in classroom observational studies conducted by educational researchers (Rossman & Rallis, 1998, p. 137).

Ethnographers trained in sociology are nowadays more inclined than were their predecessors to accept participation as a legitimate base from which to conduct observation. Adler and Adler (1987) have therefore proposed a modification of Gold's familiar typology in recognition of the increasing emphasis in contemporary ethnographic research on "membership roles" as opposed to roles grounded in pure observation. In other words, the older assumption that "participation" (which bothered sociologists more than it did anthropologists) seriously compromises the validity of observational data has given way to the realities of contemporary research, which is often conducted with a greater degree of researcher immersion (deliberate or otherwise) in the culture under study than was once considered desirable. Adler and Adler describe, for example, "peripheral-member researchers" as those who believe that they can develop a desirable insider's perspective without participating in those activities constituting the core of group membership. By contrast, "active-member researchers" are those who become involved with the central activities of the group, sometimes even assuming responsibilities that advance the group; they do not, however, necessarily fully commit themselves to members' values and goals. A third category, that of "complete-member researchers," is composed of those who study settings in which they are already members or with which they become fully affiliated in the course of research. Even though practitioners in this category celebrate the "subjectively lived experience," they still strive to use their membership "so as not to alter the flow of interaction unnaturally" (Adler & Adler, 1994, p. 380).

Traditional anthropological ethnographers did not question the utility of participation or membership as a base for observation, but they often worried about the unsystematic nature of their observational methods. Werner and Schoepfle (1987, pp. 262-264) have addressed this concern by suggesting a typology of observation undertaken in naturalistic settings that focuses on process rather than on role. In this system, there are three types of observational process, representing increasingly deep understanding of the social group under study. First, there is "descriptive observation," which is, to all intents and purposes, the observation of everything. The ethnographer assumes a childlike attitude, assuming that he or she knows nothing about what is going on and taking nothing for granted. Such an approach quickly leads to a morass of "irrelevant minutiae," although it is only with increased exposure to the culture that the ethnographer begins to understand what is and is not irrelevant. At that point, he or she moves into "focused observation," in which certain things, defined as irrelevant, can be ignored. Focused observation necessarily entails interviewing, because the insights gleaned from the experience of "natives" guide the ethnographer in his or her decisions about what is more or less important in that culture. Focused observations usually concentrate on well-defined types of group activity (e.g., religious rituals, classroom instruction,

political elections). Finally, and most systematically, there is "selective observation," in which the ethnographer concentrates on the attributes of different types of activities (e.g., apart from the obvious difference in content, what makes instructing a class in language arts different from instructing a class in social studies?)

◆ *Rethinking Observation as Context of Interaction*

Contemporary social research may be characterized by (a) the increasing willingness of ethnographers to affirm or develop a "membership" role in the communities they study, (b) the recognition of the possibility that it may be neither feasible nor possible to harmonize observer and "insider" perspectives so as to achieve a consensus about "ethnographic truth," and (c) the transformation of the erstwhile "subjects" of research into ethnographers' collaborative partners. The traditional concern with process and method has therefore been supplemented with (but by no means supplanted by) an interest in the ways in which ethnographic observers interact with or enter into a dialogic relationship with members of the group being studied. In this section, we discuss several selected works by contemporary ethnographers in order to illustrate these supplemental factors in the contemporary interactive context of observational research. We use five very general principles of social interaction to organize the following review of this otherwise quite disparate body of theoretical, methodological, and substantive literature.

The Conscious Adoption of a Situational Identity

The first principle is as follows: *The basis of social interaction is the decision (which may be spontaneous or part of a careful plan) to take part in a social setting rather than react passively to a position assigned by others.* In some of the older sociological literature, this process is referred to as "role making," as opposed to "role taking." In the context of this discussion, this principle animates those ethnographers who actively seek out situational identities based on "membership" rather than on "observation" as traditionally understood.

For example, Angrosino has conducted a long-term study of adults with mental retardation and/or chronic mental illness who are served by community-based agencies in the United States. The question at the heart of this research project concerned how these adults, who had been socialized as youths in large-scale institutions, adapted to life in the community in the wake of the move to deinstitutionalize all but the most seriously disturbed individuals. Answering such a question required an immersion into the lives of these people, because they would not likely respond adequately to questionnaires or clinical survey instruments. Angrosino also expressed a desire to understand what it might feel like to be mentally "disabled" in a society that places high value on technical competence. To investigate this issues, it would not be reasonable to "observe" people served by the selected agencies in the older, neutralist, objective manner discussed above, because the ethnographer could not presume to be able to "read" the attitudes and responses of people whose behavioral cues were, by definition, not "normal." On the other hand, engaging in intensive interviewing in and of itself would not work very well, because the clients would not likely trust someone with whom they were not already familiar outside the interview setting.

Angrosino therefore actively sought out a membership role in the world in which the clients lived and worked. He did not want to adopt one of the recognized professional roles that would have been familiar to the clients (e.g., therapist, social worker, teacher, parole officer) because the very familiarity would have resulted in stereotypical responses. On the other hand, he could not just "hang out." Unlike other kinds of communities, where strangers do often show up and stay to become friends, no one just "shows up" in a sheltered workshop or at a group home. So Angrosino opted for a role as a "volunteer." He assisted the teacher as a tutor in the classroom, he occasionally drove clients to

and from appointments, he clerked at the thrift shop run by one of the agencies as a fund-raising effort and that was staffed by the clients, and he helped out at special events (e.g., he helped to organize a charity softball game). By assuming these duties, he made it clear that he fit no preconceived model of what someone did in this community, and yet he was able to demonstrate that he did indeed have a meaningful function (other than simply "researcher," which would not have explained anything as far as the clients were concerned). He was thus able to spend a considerable amount of time making detailed observations of the settings in which the clients lived and worked, because his presence after a while ceased to be novel enough to be disruptive, and he was able to conduct interviews with the clients, who had already learned that he was someone who could be trusted. (For further details about this project, see Angrosino, 1992, 1994, 1995a, 1997b, 1998; Angrosino & Zagnoli, 1992.)

Behar's (1993) study of Esperanza illustrates the ways in which an ethnographer was led, by the force of her collaborator's personality, to adopt more of a membership role than she had originally expected. Indeed, that study could fairly be described as an account by a feminist ethnographer who realized only in the course of writing the book that she was a feminist. In relating the story of a poor Mexican Indian woman who has defined herself through her life's struggles, Behar comes to understand more about herself as a Cuban immigrant to the United States who had always felt outside the social and academic systems in which she sought membership. Behar first encountered Esperanza when the latter was selling flowers on a street corner. Behar asked the woman permission to photograph her, expecting some sort of deferential acquiescence. Instead, Esperanza asked her (in a "haughty" manner) why she needed the picture. Behar (1993) admits, "I jumped on her as an alluring image of Mexican womanhood, ready to create my own exotic portrait of her, but the image turned around and spoke back to me, questioning my project and daring me to carry it out" (p. 4). Behar responded to Esperanza's challenge by questioning her own assumptions about the power relationship in ethnographic research (see also D. L. Wolf, 1996, p. 2; M. A. Wolf, 1992, p. 5); in this case, Behar felt herself to be directed by the more assertive woman she wished to study. Esperanza could in no sense be described as a "subject" of research. If she wished to understand Esperanza and the world in which she lived, Behar would ultimately have to become part of Esperanza's family network; she did so, becoming *comadre* to Esperanza's daughter in the process.

The decision to insert oneself in a social setting other than one's own has emotional consequences, which Behar (1996) discusses at some length. She translates the old anthropological problem of establishing rapport without "going native" (a question of methodology, with strong *ethical* overtones) into a problem of allowing oneself to be vulnerable without being "too" vulnerable (a question of personal psychology, with strong *moral* overtones). She is wary of using the language of theory and analysis; it is her only tool for "making sense" of new experiences, but it is also a way of distancing herself from an emotionally affecting (and perhaps painful) encounter.

An even more emotionally affecting (and definitely more painful) encounter is reported by Eva Moreno (a pseudonym), who writes about being raped while conducting fieldwork in Ethiopia. Moreno (1995, p. 246) admits that it is neither feasible nor desirable to "maintain a fiction of a genderless self" while in the field, which means that when an ethnographer chooses how to express her own sexuality, she must always be aware of the degree to which she thereby makes herself the object of attention of others who may see her as a target of (unwanted) sexual advances. Moreno suspects that the sexual violence she suffered was directed as much against her "professional" identity as against her "private" self—there was, at the time of her research, a generalized hostility aimed at foreigners, particularly those who presented themselves as "experts" and who were blamed for the civil disorder that had overtaken the country. She had heard, for example, that at least one other foreign woman had been raped by the very police to whom she had gone to report an assault perpetrated by local men. Moreno (1995) concludes that "women

must always, everywhere, deal with the spectre of sexual violence" (p. 248). It is difficult to believe that males are not also victimized in this way—although it is much less likely that they would discuss it openly—but Moreno is undoubtedly correct in her assumption that as a generalized pattern, sexual violence is most often, in most situations, directed against women. It would therefore be a painfully naïve female ethnographer who was not prepared to factor this possibility into her plan for her observations, as "reasonable precautions" should almost certainly affect what, where, how, and with whom one conducts research.

In sum, "making a role" may mean assuming a quasi-professional stance, becoming part of a family network, or becoming hyperconscious of one's sexuality—or some combination of them all. In no case is it advantageous for the ethnographer to be passive in the face of the assumptions of the community he or she is studying.

The Perception of Power

The second principle is as follows: *In most social interactions, people assess behavior not in terms of its conformity to social or cultural norms in the abstract, but in regard to its consistency, which is a perceived pattern that somehow makes sense to others in a given social situation.* This principle is related to the traditional anthropological distinction between "ideal" and "real" culture. An ethnographer who took the observer-as-participant role was largely concerned with the ideal culture and took steps not to transgress general norms of propriety. But an ethnographer who actively makes a membership role must be more familiar with behavior as it is lived. Members of a social group typically work their way through given situations in ways that do not necessarily conform to the principles enshrined in ideal tradition.

We often function in terms of an ideology that leads us to expect (and, therefore, possibly also to see) power working downward from white, Western institutions (and their representatives, such as ethnographers) to various subordinated or marginalized peoples. Yet the literature is increasingly filled with examples of "how people in subordinate positions managed to oppose and evade the predations of higher powers" (Maddox, 1996, p. 277). We also have a lingering bias in favor of conceptualizing both culture and society as unified, cohesive wholes. Yet ethnographers increasingly find themselves studying "communities" that are defined as much by their conflicts, factions, and divisions as they are by their commonalities (Hubbard, 1997; McCall, Ngeva, & Mbebe, 1997).

For example, Angrosino (1991) compiled an oral history of a Benedictine monastery in Florida on the occasion of the centennial of its founding. He spent a month living at the monastery while conducting the interviews, during which time he adhered to the round of daily prayer, work, and reflective leisure that is prescribed by the Rule of St. Benedict. The research was approved by the abbot (to whom all the monks in the community have vowed obedience), and although Angrosino was not a vowed member of the community, the abbot made it clear that he expected the same deference from the ethnographer that he received from the monks. The abbot was always very cordial, but he demarcated his position very clearly in ways both subtle (sitting behind a desk when it came time for his interview) and blatant (reserving the final say as to which members of the community Angrosino could approach for interviews). The other members of the community claimed to be very supportive of the research, but Angrosino found that a fair number of them gave very truncated interviews, explaining that it is not seemly for a monk to speak too much about his own experiences—doing so smacks of vanity. In the course of living in and observing the community, Angrosino came to realize that this humble reticence, although sincere to a point, covered other motives. For one thing, it expressed a quiet rebellion against the authority of the abbot, who had mandated their cooperation. In the ordinary course of things, they would not have dared to be seen as less than eager to carry out the abbot's wishes, but because the ethnographer was, for all his temporary immersion in the life of the monastery, an outsider, he could be disobliged in a way that would have spelled trouble had it been directed against any insider, much less the

abbot himself. Moreover, this tactic allowed some of the monks to make an oblique criticism of those who had been more fully cooperative; their own humility was a kind of symbolic indictment of their brothers, who could be seen as either toadying to the abbot or preening in their own vanity by "telling all" to the researcher. In any event, it was clear that the ideal arrangements of monastic life—with its formal, even codified system of hierarchy and deference—were not what happened in real life. Even in such a highly circumscribed culture, people could experiment with styles of interaction and involve the visitor in subtle, yet very revealingly subversive, power games, games that inevitably shaped both *what* the ethnographer observed and *how* he interpreted what he saw.

Behar (1993) discusses her inclination to fit Esperanza's story into the prevailing model of feminist studies of Latin American women. She realized, however, that Esperanza could not fit the part of the "exemplary feminist heroine." The reality of Esperanza was no less admirable and heroic, for her life was a kind of epic of female struggle, rage, and defiance of the patriarchal institutions of her culture; but she was a flesh-and-blood woman capable of "misbehavior," and not a stereotypical Third World feminist plaster saint. Behar concludes that an ethnographer's desire to produce stories that empower the people she studies must be grounded in an allowance for the way women in other cultures "misbehave." There must be respect for their "different ways of making sense," even if their sense does not conform to the European or North American expectations of the feminist ideal (p. 270). In a similar vein, Hirsh and Olson (1995, p. 23) cite Sandra Harding to the effect that feminist scholarship in general has sought to surmount the established categories of social knowledge, which have been developed from a male point of view (even when applied in the past by female scholars). Margery Wolf (1992), however, asks "whether by studying our subjects we are also exploiting them and whether by attempting to improve women's living situations we are imposing another (powerful) society's values" (p. 2). Moreover, it may be misleading to conceptualize the "power relationship" as that obtaining between researcher and "subject." In fact, Hsiung (1996) claims that this standard binary view overlooks the patriarchal context in which both the (female) ethnographer and her female informants are situated. It may be more useful to think in terms of a "multidimensional power relationship, of which the patriarchal/capitalist system, individual agents of the system, female informants, and female feminist researcher are the key constituents" (p. 123).

The injunction to pay attention to what makes sense in a given setting takes on particular importance when, as is now so often the case, ethnography is conducted "without the ethnos" (Gupta & Ferguson, 1996b, p. 2). In other words, few ethnographers function within the circumscribed communities that lent coherence to the cultures or societies that figured so prominently in the conceptual frameworks of earlier generations of observational researchers. It is no longer possible to assume that "the cultural object of study is fully accessible within a particular site" (Marcus, 1997, p. 96). Much of the contemporary ethnographic field consists of studies of those who inhabit the "borders between culture areas," of localities that demonstrate a diversity of behavioral and attitudinal patterns, of "postcolonial hybrid cultures," and of the social changes and cultural transformations that typically are found "within interconnected spaces" (Gupta & Ferguson, 1996a, p. 35). People, after all, "live in different overlapping but not always overdetermining spaces and times: domestic spaces; national spaces; broadcasting and narrowcasting spaces; biographical times; daily times; scheduled, spontaneous, but also socio-geological times" (Abu-Lughod, 1997, p. 112). Malkki (1996b), for example, describes working in "accidental communities of memory," which include "people who have experienced war together . . . ; people who were bombed in Hiroshima or Nagasaki; people who all fled a particular revolution; people who are stricken by a particular illness; or people who worked together on a particular humanitarian or development project" (p. 92). In all of these cases, "it is the communities that are accidental, not the happenings" (p. 92). The ethnographer

therefore no longer enjoys the luxury of assuming that the local scene he or she is observing is somehow typical or representative of "a" culture of "a" society. It is a nexus of interactions defined by "interstitiality and hybridity" (Gupta & Ferguson, 1996a, p. 48), factors of "the globalizing discourses and images of the media" (Peters, 1996, p. 81), that the ethnographer, the classic neither-here-nor-there person, helps to define. In some cases, the ethnographer may even be said to *create* a community simply by virtue of studying certain people and by implying that the links he or she has perceived among them constitute a society. The "street corner society" studied by Whyte (1955), or Liebow (1967), or Hannerz (1969) became a "society" only because an ethnographer chose to treat that "nexus of interaction" as a site. Oral historians are often in the position of creating virtual communities by linking several personal experiences around a central theme of their own choosing (Hareven, 1996).

The principle is confounded when gender—that "enormous, extreme" question, in the words of Jean-François Lyotard (quoted in Olson, 1995a, p. 186)—and sexual orientation enter the picture, because "differing sexualized perspectives of 'the field' influence the kind of relationship that the ethnographer has with the field and this, in turn, affects interpretation" (Willson, 1995, p. 253). Gender and sexual orientation are extremely meaningful elements in defining an ethnographer's personal identity; they can also become filters through which the observation of communities is mediated. The problem is that the meanings shift from one community to another. The observer cannot assume a universal, let alone an ideal, symbolism of gender and/or sexual orientation. The cues of personal identity must always be interpreted in the context of the reality of a given social setting.

Dubisch (1995, p. 34), for example, notes that female anthropologists, simply by virtue of being female, have not been granted the indulgence to engage in casual sex, whether their sexual encounters occur at home or in the field. Male anthropologists, by contrast, have long been assumed to have had casual flings while in the field (Newton, 1993, p. 5). There was a tacit assumption that such male behavior did not matter because it was expected and approved by all parties, whereas analogous behavior by women was always disruptive because it was neither expected nor approved. This assumption must now be called into question, because the decision of *any* ethnographer to insert him- or herself into the social setting in a sexual manner must be seen to have repercussions with respect to what he or she is able to observe. Whether or not such behavior is approved is less important than the recognition that it will make a difference to the entire set of relationships initiated by the ethnographer, and hence to the type and quality of observations he or she is able to conduct. In recognition of this reality, Killick (1995) seems to counsel abstinence, noting that "while in the field, the fear of upsetting the delicate balance of relationships with informants is likely to be a significant curb on the libido" (p. 81). On the other hand, Killick advises those whose libidos are unrestrained by such methodological niceties to "keep quiet about it if their behavior is likely to be seen as either uninteresting (a possibility we should not discount) or reprehensible" (p. 81). Altork (1995), however, argues against both repression and concealment. She believes that "instead of blocking out [the] wealth of sensory (and sensual) input, or relegating it to private field journals, we might consider making room for our sensual responses in our work" (p. 116). In her view, whether or not one "did it" in the field is less important than whether or not one is able to be honest in acknowledging what did or did not happen and why, because such admissions leave the ethnographer "open to the fertile possibilities for dialogue about the ways in which 'it' changed, enhanced, or detracted from what we felt, witnessed, and interpreted in the field" (p. 121). In an ironic twist on this old (but only recently public) dilemma, Altork suggests that "perhaps by acknowledging our own feelings and desires, we might actually look at other people and places *more objectively*, by being able to ferret out our own biases and distortions as we do our work" (p. 132; emphasis added).

The process of open acknowledgment may be hindered in the case of lesbian or gay ethnographers, who may be habituated to a degree of concealment in both their personal and professional lives. Goodman (1996, p. 50) notes that lesbian and gay male ethnographers *expect* to engage in subterfuge while in the field, but Burkhart (1996) believes that his initial efforts at concealment (rationalized as an effort to achieve the "ideal of observer neutrality") led only to "spells of inertia and depression" (p. 34). Williams (1996, p. 74) suggests a compromise: being completely honest with people in the community although less so with granting agencies. He claims to have had positive experiences with people to whom he divulged his sexual orientation in the communities in which he conducted research, but he has found it prudent to apply for funding by stressing other research topics, and then studying homosexual behavior once in the field. Even AIDS research, now a reasonably well-funded area for social scientists, was initially not something funding agencies wanted to hear about, because it was assumed that AIDS was a purely homosexual concern (Bolton, 1996, p. 157).

Lesbians and gay men are used to "constant, and conscious, identity management" and have typically carried this mind-set from their personal lives into their research settings (Lewin & Leap, 1996, p. 13). A fair number of homosexual ethnographers have chosen to study homosexual behavior in the field (apparently on the assumption that they have a ready point of reference), but it is easy to be disappointed if one assumes that the understanding of and manifestation of homosexuality is the same in all communities, just as it is easy to be misled into assuming that female ethnographers are in an advantaged position when it comes to understanding women in all cultures (Lewin & Leap, 1996, p. 17). The point is that one's gender, as well as one's sexual orientation, are matters that must be taken into conscious account when one endeavors to conduct observational ethnography; but neither factor can be considered a source of privileged knowledge in and of itself. As Lang (1996) notes, "Quite obviously, there is no 'universal gay community' " (p. 103). At the very least, homosexuality does not "override the social hierarchies of the contemporary world" (Kennedy & Davis, 1996, p. 193); it is still necessary to investigate the impact of "the hierarchies of class and race" even within a presumed "gay community."

In any case, it is clear that "no longer is it generally acceptable for [ethnographic researchers] to conceal or deny the significance of their gender identity, age, class, or ethnicity. (Sexual identity represents a sort of final frontier in this regard.) Instead, contemporary ethnographic writing tends to acknowledge these attributes as factors that shape an [ethnographer's] interpretations of what she or he observed in the field" (Weston, 1996, p. 276). For example, Edelman (1996) discusses the varying impacts his Jewish ethnicity had in three different field sites. Other factors, less well established as demographic categories, may also play a part in how an ethnographer relates to what is studied—for example, the ethnographer's personal struggle with bulimia (Tillman-Healy, 1996) or breast cancer (Kolker, 1996), or the ethnographer's having survived a nonmainstream childhood (Fox, 1996; Ronai, 1996) or having undergone detoxification therapy (Mienczakowski, 1996). In all of the cases just cited, the ethnographers' personal experiences were the main focus of both observation and interpretation. But it is clear that if these ethnographers should go on to study other people with those same characteristics, they would have to shift from a perspective that implicitly elevates the personal to the normative in order to observe what is going on in a natural setting.

Negotiating a Situational Identity

The third principle is as follows: *Interaction is always a tentative process that involves the continuous testing by all participants of the conceptions they have of the roles of others.* In other words, ethnographers and their collaborators do not step into fixed and fully defined positions; rather, their behaviors and expectations of each other are part of a dynamic process that continues to grow (one hopes in healthy ways, although the outcome is sometimes problematic) throughout the course of single research projects or as

they move from one project to another (Wolcott, 1995, p. 77). Giroux (1995) speaks of the need for "intellectuals" in general (and ethnographers in particular) to "reinvent themselves in diverse sites" (p. 197). Denzin (1997a) discusses the "mobile consciousness" of an ethnographer who is aware of his or her "relationship to an ever-changing external world" (p. 46).

For example, Angrosino (1997a) conducted an oral history of the Southern Anthropological Society on the occasion of the 30th anniversary of its founding. He had been a member of the SAS almost since its establishment and had served over the years in both elective and appointive offices. As a professional anthropologist affiliated with a department at a university in the South, and whose research often dealt with aspects of life in the contemporary South, he was in terms of status a fully integrated member of the institutional culture of the organization. Moreover, he had long-term personal and professional ties with all of the people who were scheduled to be interviewed. The bulk of the interviews were conducted during the special anniversary meetings of the society, and Angrosino brought along three graduate students to work as his assistants—and, more important, to serve as "reality checks" to make sure that he did not act like too much of an "insider" and thereby miss important cues or take for granted too many items that outsiders would find in need of clarification. The interviews, however, began awkwardly, as many of the participants seemed annoyed at being questioned by someone they assumed already knew the answers. "Oh, you remember what happened in New Orleans in '70 . . ." someone would say. "Well, why don't you tell about it in your own words?" Angrosino would respond. They usually sighed in frustration at that suggestion. It was very difficult for professional anthropologists to act as informants, particularly when the interviewer was already assumed to be in the know. Some others decided to short-circuit an uncomfortable situation and, in effect, to hijack the interview, carrying on in lecture/monologue fashion without paying attention to the interviewer's questions. Still others demanded to be interviewed by one of the graduate assistants; "I can't talk to you with a straight face," one of them told Angrosino.

After a while, the awkwardness wore off, presumably as members began to share with one another their reactions to having been interviewed. They reaffirmed all the reasons they had thought of collecting an oral history in the first place, primarily because doing so would be a good idea "for posterity." The later interviews went much more smoothly, as participants had clearly made a tacit decision to treat the overly familiar Angrosino as simply a naïve outsider to whom *everything* needed to be explained. The ethnographer came to think of himself as if he were one of his students, so that he would remember to ask all the questions that someone who had not been in on the action would want ask. Although most ethnographers seek to move from outsider status to a status of participant/member, Angrosino in this case (abetted by his collaborators) reinvented himself from complete insider to interested-but-ignorant bystander. Within the interactive context of observational research, roles mutate in response to changing circumstances and are never defined with finality.

Behar describes this process in terms of her own evolution from "feminist anthropologist" to "feminist ethnographer," by which she means a researcher who is attentive to the "reflexiveness about the politics of practicing feminism and experimental cultural writing." The focus of such reflexiveness must be "women's relationships to other women" (Behar, 1993, p. 301) rather than a scientific observer's relationship to a "subject" of research. For Behar (1996, p. 5), the very term *participant observation* is an "oxymoron." The ethnographer must, Behar suggests, be defined by the creative tension in the role of member/observer, not by some finite quantity of information gathered by one who plays that role. According to Ferguson (1996, p. 153), the exploration of how shifting connections frame experiences of place, community, and society among "the partially and provisionally dislocated" may well represent the most fertile ground for future ethnographic research.

Blackwood (1995, p. 53) therefore speaks of the *identities* assumed by the ethnographer in

the field in terms of the many different ways he or she is perceived by "others." As a woman, she has "continually tacked back and forth between various assigned and constructed identities: researcher, friend, daughter, professional, American" (p. 58), and she concludes that "identities [in the field] are never stable, never simply defined" (p. 70). At this point in the psychosocial history of Western culture, it is probably clear that our "identities" in the existential sense are always in a process of evolution and never achieve a fixed, final point. The naïve assumption that ethnographers' identities in the field should be clearly defined and finite is, perhaps, the last vestige of the old belief that "wholeness" means personal autonomy and fixity of identity. Nevertheless, there is a very strong sense in which ethnographers continue to believe, as they have since the days of the ascendancy of the Freudian perspective in social analysis, that doing fieldwork is a way in which they can come to terms with themselves. "Sharing a different lifestyle," according to Barnett (1983), "has a mirror effect, providing glimpses of an observer's foibles as well as his dignity" (p. 169).

Walters (1996) expresses this view with an interesting figure of speech: In the matter of establishing one's identity, one must "constantly . . . pivot the center" (p. 63). She was led to this perspective when it became necessary for her to deal with the problem of bringing her partner to the field—the Yemen Arab Republic. She assumed that a conservative Islamic society would not be a friendly place for two openly homosexual women, and she considered various strategies of concealment (e.g., claiming to be relatives, going through a process of adoption); she finally decided to refer to the other woman as her "companion," which, while perfectly true, seemed vague enough to avoid the suspicion of the authorities. (It even had a whiff of Victorian propriety about it—a young woman traveling in the company of an older female companion has long been a well-known image in many parts of the world.)

Despite such occasionally successful strategies, it remains true that women researchers often feel pressured to conform to the gender behavior norms of the cultures they study, even if those norms are not the ones they would freely choose for themselves, and even if they have to resort to a certain amount of deception so as to appear to conform. According to Diane Wolf (1996), "Feminist fieldworkers have lied about their marital status, . . . about their national identity or ethnic/religious background . . . , about divorce and former marriages . . . , and about their class background" (p. 11). Even in those cases where the deception did no real harm to the people the researcher was observing, the ethnographers often felt guilty, in part because the very act of deception "directly contradicts attempts at a more feminist approach to fieldwork, which includes attempts to equalize a relationship and create more of a friendship" (Berik, 1996, p. 56; see also D. L. Wolf, 1996, p. 12). On the other hand, the refusal to deceive—in effect, to defy the norms of the community being studied in order to make a principled stand for what one actually believes—can sometimes have unintended negative consequences. Berik (1996, p. 65), for example, admits that her openly feminist stance while conducting research in a Turkish village unwittingly led to one of her female informants' being beaten by an outraged husband, who assumed she was being led astray by the insufficiently submissive ethnographer.

Lang (1996) conducted a research project focusing on Native American lesbians. Although she is openly gay herself, she found it unacceptable to locate potential informants in bars or other obvious meeting places. She was concerned lest the other women assume, because she was hanging out in a bar, that she was therefore interested in finding sexual, rather than research, partners. She decided that it would be unethical to pose as a potential sexual partner in order to elicit information (Lang, 1996, p. 94), and so she had to seek her "community" in less symbolically charged environments. In this case, Lang decided that her status as "ethnographer" would take precedence over her sexual orientation as a way of defining herself to the people she intended to study, despite the fact that sexual orientation was the thematic focus of her study. Her decision was a matter of strategy dictated by her reading of the nature of the particular community.

Much of the recent literature bearing on the creation, maintenance, and creative evolution of observers' identities (and on the pros and cons of deception and disclosure) has dealt with issues particular to women and lesbians/gay men, as shown above. It is worth mentioning, however, that there are other issues of identity that are of concern to researchers who study situations of political unrest and who come to be identified with politically proscribed groups (Hammond, 1996; Mahmood, 1996; Sluka, 1990), or who work with groups that are defined by their need for deceptive concealment, such as illegal migrants (Chavez, Flores, & Lopez-Garza, 1990; Stepick & Stepick, 1990) or those involved in criminal activities (Agar & Feldman, 1980; Brewer, 1992; Dembo, Hughes, Jackson, & Mieczkowski, 1993; Koester, 1994; van Gelder & Kaplan, 1992).

Criteria for Validation

The fourth principle is as follows: *Participants validate the cues generated by others in the setting by internal and/or external criteria.* Internal criteria are those by which members of a community check their behavior against the prevailing norms of their own group. External criteria are those by which members of a community check their behavior in terms of presumably universal standards. In other words, participants in the interaction ask, "Does it work?" or, perhaps less nobly, "Can I get away with it?" (that is, "Does my interpretation help me and my potential collaborator work out a viable relationship?") rather than, "Is it correct?" (That is, "Does 'the culture' somehow 'require' people to act in a certain manner?")

For example, Angrosino (1995b) led a team of graduate students in a study of local responses to the AIDS epidemic. The research centered on a particular agency that provided limited direct service (mostly testing) but was far more important as an information and referral network. The agency had been founded by partners and relatives of people who had contracted AIDS; these people believed that their personal involvement in—indeed, their emotional commitment to—the cause was a primary reason for the success of their project. They were somewhat put out by the apparent transformation of the agency into a more professional outfit, with leadership positions being increasingly taken by human service managers and development specialists with no special concern for the particular characteristics of the AIDS crisis except for the recognition that it was a major public health concern. The professional managers were helpful to the researchers, but in a rather distant fashion; by contrast, the "old-line" founders were eager to draw the team members into a kind of social circle, the better for them to learn "what it's really all about."

This situation was one in which internal and external criteria seemed to work at cross-purposes. On the one hand, the two factions within the agency were reacting to very different criteria for validation. One side validated its activities and sense of mission with reference to the *quality* of interpersonal interactions (an internal criterion) and to general humanitarian concerns (an external criterion). The other group sought validation in the *efficiency* of interpersonal interactions (an internal criterion) and in "objective" standards of professional conduct (an external criterion). There was clearly no "correct" corporate culture in such an agency or in the client community it served; the question was, "What worked?" and the answer was, "It depends on what you want to accomplish." The founders vigorously sought to convince the ethnographers that the key to success in the crisis was to "work from the heart, not from the head." People with AIDS and their caretakers needed emotional support more than sound fiscal management from an agency like theirs. They did not deny that the clients appreciated good management, but believed that a coolly competent accountant was not the "face" potential clients wanted to see when they contacted the agency for help. They sought empathy, having already gotten quite enough unfeeling "competence" in clinical settings. The ethnographers, for their part, tended to agree, although they certainly appreciated the way in which the

"competent" managers facilitated the research process for them.

It may be useful to characterize this aspect of the interactive context in terms of the ways in which personal experience serves as an organizing principle in the process of mediating internal and external criteria in social settings. For Denzin (1997a), "the starting point is experience" (p. 55), which leads to a discourse between the ethnographer and other members of the community, a discourse that "often begins from the painful autobiographical experiences of the writer" (p. 57). Indeed, there is increasing tolerance for a discourse that ends with those same experiences as well (Quinney, 1996). "Life in the field," Hinsley (1983) points out, "is an individual experience" (p. 55), and "ethnography" (despite the traditional connotation of the term as the study of "a people") is seen in certain quarters as a species of autobiography, the "personal ethnography" (Quinney, 1996). Olson (1995b) cites Donna Haraway to the effect that ethnographic observation must be translated into written representations that place "the writer's own situatedness in history" in the foreground (p. 46). Denzin's (1997a)—and, more obliquely, Olson's—remarks are directed mainly to the production of "standpoint texts" that flow from the particular experiences of those who have been excluded from "the dominant discourses in the human disciplines" (p. 55). Stocking (1983b) refers to the same trend in somewhat less favorable terms as the proliferation of "adjectival anthropologies" (p. 4).

It is nevertheless certainly possible to apply the same perspective to anyone engaging in ethnographic research. Even those who come from traditionally "dominant" social groups must engage in a process of consciousness-raising about the nature and effects of their interaction with others. For them, as for those previously marginalized, the starting point of observational research *is* experience, for their own existential immersion in the "cultural displacement" of people, things, and cultural products is a defining quality of the state of the world today (Malkki, 1996a, p. 53). According to Mary Belenky: "We all need to understand how writing the same material for different audiences changes the voice. This is very empowering knowledge to have" (quoted in Ashton-Jones & Thomas, 1995, p. 86). On the other hand, one cannot be "preoccupied" with one's audience, because such a focus can lead to "self-censorship," according to bell hooks (quoted in Olson & Hirsh, 1995, p. 110). In effect, the ethnographer who is a member/observer is an artifact of the very situation of cultural displacement that he or she intends to study. It may not, in fact, be possible to resolve the tension between what the ethnographer "is" and what he or she must "become" in the field; rather than fret about that tension, it may now be time to "find some practical use" for it in our analysis (M. A. Wolf, 1996, p. 217).

Bolton (1995) points out that when the topic of an ethnographic study is sexuality, the ethnographer is limited by his or her inability to "observe" the behavior in the strict, traditional sense of the term. Much of the social scientific discourse on sexual behavior has been recorded via hearsay rather than eyewitness testimony, although it has often been conducted within the compass of supposedly observational research designs. The best way for the ethnographer to overcome this limitation, Bolton suggests, is through participation, *if* the ethnographer feels comfortable doing so. He admits that "I learned more through participation than by simple observation or direct interviewing" (p. 148). This solution raises some additional issues of an interactional nature: Is the ethnographer who "participates" in this manner really learning about the norms of sexual practice in the community he or she is studying, or is the ethnographer importing attitudes and emotions from his or her own culture into the field setting? Is the ethnographer, in effect, confusing internal and external criteria? More provocatively, is he or she participating in the creation of a new set of norms or standards that are specific to this particular interaction, and not of either the host community or the home community? If the latter, is the ethnographer still doing social research, or has a new field for observation been introduced, requiring

at minimum the much-discussed "blurring of genres" in reporting and at maximum a blurring of traditional academic/disciplinary boundaries in order to conceive of a new topic of discourse? As Murray (1996) notes: "Having sex with the natives is not a royal road to insight about alien sexuality. . . . In answering questions or inscribing life histories at a researcher's behest, as in having sex with them, the person whose sexuality is being studied is likely to be guessing what the researcher wants to hear rather than representing his or her most fundamental desires and identities" (p. 250).

A female variant of Bolton's point of view is provided by Gearing (1995), who fell in love with and married her "best informant" while conducting field research. Ethnographic research, she contends, is always a "joint endeavor" between the would-be observer and those he or she would observe; it is therefore dependent on the "quality of our personal relationships" (p. 207). Gearing advocates abandoning the "model of the dispassionate participant observer" and adopting instead the persona of "an emotionally aware inter-actor engaged with other actors" (p. 211).

The validation of the individual experience of the ethnographer has traditionally been bound up in the ethnography (usually written) produced as the result of observational research. The only audience that really matters has been the academic, although there have been recent attempts to write for an audience composed (at least in part) of the "subjects" of research. Nevertheless, there are now many formats in which a report can be generated (Polkinghorne, 1997), reflecting the variety of constituencies to which the ethnographer is now responsible. Thus the ethnographic observer must be concerned with the different "voices" in which he or she presents material. Traditional ethnographic reportage favored the supposedly objective third-person voice, emanating from the "omniscient narrator," as Tierney (1997, p. 27) notes. The move toward greater participation allowed the ethnographer to acknowledge his or her own presence, although this was often done via circumlocution, with the ethnographer referring to him- or herself, for example, as "the interviewer" (Tierney, 1997, p. 26). The once-banned "I" is now much more common as subjective experience comes to the fore (Tierney, 1997, p. 25; see also Ellis, 1997; Lather, 1997; Tanaka, 1997). Wolcott (1995) declares his preference for "an approach that keeps humans always visibly present, researcher as well as the researched" (p. 15). Margery Wolf (1992, p. 52) suggests that it was women and others previously marginalized by the academic world who first dared challenge orthodoxy by writing in the first person, a trend that she believes has now entered the mainstream—now that male academics are also doing so. These shifts in reference are not irrelevant matters of style; they reflect evolving self-images of the ethnographic observer, changing relations between the observer and the observed, and new perceptions about the diverse (and possibly even contradictory) audiences to whom ethnographic research must now be addressed. Certain kinds of ethnographic texts can have professional, participatory, lay, and aesthetic audiences (Denzin, 1997b, p. 188). It is clear that validating what "I" say is a very different matter—philosophically as well as scientifically—from validating what "the interviewer" says.

Perhaps the most widely cited case study of the subtle interplay of internal and external criteria is the "thrice-told tale" of Margery Wolf (1992). In her book, Wolf embarks on a personal, reflexive journey by revisiting a fictional short story she had written some 30 years earlier, when she was the wife/assistant of an anthropologist conducting his first field research in Taiwan. Since that time, she had become an anthropologist in her own right, as well as a feminist. The original story (which was based on real events) is reprinted first, followed by the field notes and journal entries referring to the same events. The third part of the book is a formal ethnographic article that was originally published in *American Ethnologist*, a mainstream academic journal. Each section of the book is followed by commentary in which Wolf explains what she remembers about the events as represented in each of the three written accounts and what she now thinks about those same events from the perspective of three decades. In the

process she sees changes in herself as both a woman and an ethnographer. For example, she notes, "Where once I was satisfied to describe what I thought I saw and heard as accurately as possible, to the point of trying to resolve differences of opinion among my informants, I have come to realize the importance of retaining these 'contested meanings' " (p. 4).

Wolf's point is that no ethnographic research, including supposedly objective naturalistic observation, can be considered complete and valid until it has undergone what Polkinghorne (1997) describes as the transformation of a "list or sequence of disconnected research events into a unified story with a thematic point" (p. 14). A good observer can develop the skill of catching cultural meanings as members of the community themselves understand them, but equally important is the skill of writing up the report in such a way as "to convey that meaning to an interested reader from another culture" (Wolf, 1992, p. 5).

Contextualizing Meaning

The fifth principle is as follows: *People come into interactions by assuming situational identities that enhance their own self-conceptions or serve their own needs, which may be context specific rather than socially or culturally normative.* Members of the community are reacting to *this particular ethnographer* and the cues he or she generates, not to "an outsider" in a generic sense. Some of those cues are matters over which the ethnographer can exercise some control if he or she is made aware of them (e.g., improving language facility, dressing in an "appropriate" way), although many others are simply part of the package (e.g., gender, race/ethnicity, relative age). In the latter case, the ethnographer may need to realize that what *he or she* observes is conditioned by who *he or she* is, and that different ethnographers—equally well trained and well versed in theory and method but of different gender, race, or age—might well stimulate a very different set of interactions, and hence a different set of observations leading to a different set of conclusions.

Angrosino, for example, has been involved with a long-term project documenting the patterns and impacts of inter-island labor migration in the Netherlands Antilles. One of his informants was an elderly woman now living on Saba, the smallest of the islands, and he published her life history in an anthology devoted to the Saba part of the project. Shortly thereafter, the same woman was interviewed by a Saba-born folklorist (and political leader) who was publishing a collection dedicated to "the island's treasures" (i.e., the accumulated wisdom of its senior citizens). The general outlines of the woman's life story were the same in both accounts, but there were clear differences as well. As might be expected, the story she told Angrosino had many more explanatory details than the one she told her fellow islander. Angrosino had obviously asked her many questions to clarify matters about which a nonnative would have no knowledge. But there were more subtle, yet telling differences. The woman had lived a life of great hardship, and yet she had survived to raise (virtually single-handedly) a large family; all of her children had gone on to become pillars of the community, and she herself was recognized as a person of the utmost integrity. When she told her story to the ethnographer, she allowed herself a bit of pride in recounting how she had surmounted all her travails; she comes across in that account as a humble, yet definitely heroic figure. In the story she told her fellow Sabian, she is considerably more self-deprecating; the island culture is not very cordial to those who "try to get above themselves." On the other hand, she included anecdotes in her discourse with the Sabian about her defiance of the white establishment, incidents she suppressed when talking with the (white) ethnographer. The point is that Angrosino, who is white, from another country, and of the same generation as this woman's grandsons, evoked a qualitatively different story from the one she told the black Sabian of her own generation. There is little evidence of conscious dissimulation; she merely responded to cues both obvious and covert in her two "audiences," and, like any good performer, she engaged her interlocutors in terms that resonated most clearly with them and their personal circumstances. (For a more de-

tailed comparison of the two life stories, see Angrosino, 1989.) As Behar (1996) notes, citing George Devereux, the observer "*never* observes the behavioral event which 'would have taken place' in his absence, nor hears an account identical with that which the same narrator would give to another person" (p. 6).

Denzin (1997a) points out that it is now important to be aware of class, race, gender, and ethnicity, and of how these factors "shape the process of inquiry, thereby making research a multicultural process" (p. 19). This insight is not in and of itself new; what is new and important for the purposes of this discussion is the implication that the ethnographer must become aware of these factors not to minimize them or "hold them constant," as classic observers were taught to do, but to integrate them creatively into both the process of observation and the production of a written representation of the fruits of that observation. Diane Wolf (1996) echoes this position in a feminist context; she advises ethnographers to analyze their field research in terms that use, rather than deny, their "intuition, feelings, and viewpoint" (p. 5).

Morton (1995), for example, conducted research in Tonga, a "seductive," "exotic" culture. On her first visit, she "nearly" succumbed to the seduction; 10 years later, she was pregnant while conducting research, and she was able to wear her pregnancy as a "chastity belt" to avoid sexual pursuit (p. 168). The reader assumes that Morton had other motives for her pregnancy, but it certainly helped her out of an undesirable situation (that of being sexually active in the field); it probably also meant that she was able to hear a significantly different side of the story of Tongan culture from the one to which she was privy as a single, childless woman.

◆ *The Ethical Dimension of Observational Research*

Observation was once thought of as a data collection technique employed primarily by ethnographers who thought of themselves as objective researchers extrinsic to the social settings they studied. It has become a context in which researchers who define themselves as members of those social settings interact in dialogic fashion with other members of those settings. This transition has also effected a shift in the parameters of our ongoing reflections on the ethics of social research.

Institutional Structures

For good or ill, virtually all social research in our time is governed by the structure of institutional review boards (IRBs), which grew out of federal regulations beginning in the 1960s that mandated informed consent for all those participating in federally funded research. The perceived threat was from "intrusive" research (usually biomedical), participation in which was to be under the control of the "subjects," who had a right to know what was going to happen to them and to agree formally to all provisions of the research. The right of informed consent, and the review boards that were eventually created to enforce it at each institution receiving federal moneys (assuming a function originally carried out by the federal Office of Management and Budget), radically altered the power relationship between researcher and "subject," allowing both parties to have a say in the conduct and character of research. (For more detailed reviews of this history, see Fluehr-Lobban, 1994; Wax & Cassell, 1979.)

Ethnographic researchers, however, have always been uncomfortable with this situation—not, of course, because they wanted to conduct covert, harmful research, but because they did not believe that their research was "intrusive." Such a claim was of a piece with the assumptions typical of the "observer-as-participant" role, although it is certainly possible to interpret it as a relic of the "paternalism" that traditional researchers often adopted with regard to their "subjects" (Fluehr-Lobban, 1994, p. 8). Ethnographers were also concerned that the proposals sent to IRBs had to be fairly complete, so that all possibilities for doing harm might be adequately assessed. Their research, they argued, often grew and changed as it went

along and could not always be set out with the kind of predetermined specificity that the legal experts seemed to expect. They further pointed out that the statements of professional ethics promulgated by the relevant disciplinary associations already provided for informed consent, such that IRBs were merely being redundant in their oversight.

In the 1980s, social scientists won from the federal Department of Health and Human Services an exemption from review for all social research except that dealing with children, people with disabilities, and others defined as members of "vulnerable" populations. Nevertheless, legal advisers at many universities (including the University of South Florida, where we are both based) have opted for caution and have been very reluctant to allow this near-blanket exemption to be applied. Indeed, at USF it is possible for a research proposal to undergo "expedited" (or "partial") review if it seems to meet the federal criteria for exemption, and even those that are judged worthy of full exemption must still be on file. USF now has two IRBs—one for biomedical research and one for "behavioral research." Because the latter is dominated by psychologists (by far the largest department in the social sciences division of the College of Arts and Sciences), this separate status rarely works to the satisfaction of ethnographic researchers. The psychologists, used to dealing with hypothesis-testing, experimental, or clinical or lab-based research, have been reluctant to recognize a subcategory of "observational" research design. As a result, the form currently required by the behavioral research IRB is couched in terms of the individual subject rather than in terms of populations or communities, and it mandates the statement of a hypothesis to be tested and a "protocol for the experiment." Concerned ethnographers at USF have discovered that some other institutions have developed forms more congenial to their particular needs, but as of this writing they have had no success in convincing the USF authorities to adopt any of them as an alternative to the current "behavioral research" form for review.

It is interesting to note that the only kind of "observational" research that is explicitly mentioned and routinely placed in the "exempt" category at USF is that defined as "public"—for example, studying patterns of where people sit in airport waiting rooms, one of the rare remaining classic "pure observer" types of ethnography. The exemption, however, disappears if the researcher intends to publish photos or otherwise identify the people who make up "the public."

Issues for Contemporary Observational Researchers

Ethical ethnographers who adopt more clearly "membership"-oriented identities—certainly a very strong trend, as this review has demonstrated—are therefore caught between two equally untenable models of research. On the one hand is the official IRB, which is tied to the experimental, hypothesis-testing, clinical model. On the other hand are those ethnographers who, in their zeal to win exemption from irrelevant and time-consuming strictures, appear to be claiming that their research is not, should not be, "intrusive" at all. Yet the interactive, membership-oriented researchers *are,* by definition, intrusive—not in the negative sense of the word, to be sure, but they are still deeply involved in the lives and activities of the communities they study, a stance fraught with all sorts of possibilities for "harm." There are ethnographers with an "applied" orientation (i.e., those who seek to use their research to effect social or institutional change), those interested in using their research as part of a project for social criticism, and those who advocate for "universalistic" values (e.g., women's rights, ecological justice) even when the local communities they happen to be studying act in ways inimical to those values. All of these researchers may do "harm" in the strict sense of the term, but it has not been satisfactorily determined whether such "harm" is necessarily and inevitably to be avoided by the ethical researcher. It is difficult to prepare an informed consent form when one cannot even begin to anticipate the possibilities that might flow from personalized interaction. In principle, at least, it might be possible to say that because research collaborators are no longer subjects, by definition they have as

much power as do researchers in shaping the research agenda; they do not need to be warned or protected. But in reality, the researcher is still in a privileged position, at least insofar as actually *doing* the research and disseminating its results are concerned. The researcher probably does not want to retreat to the objective cold of the classic observer role, but neither does he or she want to shirk the responsibility for doing everything possible to avoid hurting or embarrassing people who have been trusting partners in the research endeavor. As Fluehr-Lobban (1994) concludes:

> Openness and disclosure; reference in social studies to participants instead of informants; models of collaborative research that incorporate informed consent; all are components of [ethnographic] research, whether academic or applied, federally or privately funded, that is fully current with developments taking place in the world we study and the professions that study it. Informed consent may only be a convenient summary term for what has taken place in biomedical and social science research, but when its spirit is implemented it results in better researchers and better research. (p. 8)

An Interim Solution

This ethical dilemma would seem to be the pivot on which further developments in observational research will turn, although there have been only provisional efforts to resolve it. One example of such an attempt is Angrosino's *Opportunity House* (1998), his summative report on the study, discussed above, of nearly two decades' duration of community-based agencies serving adult clients with mental disabilities. Informed consent was secured from those clients who were classified as "legally competent" and from the legal guardians of those who were not. Nevertheless, Angrosino never felt confident that the people with whom he worked fully understood the ramifications of their consent, particularly given that much of the ethnographic research was conducted in the form of extended life history interviews that often went off in directions that could not have been predicted at the time the original study was proposed and approved. Various interim publications about the project were written in the standard authoritative voice of the objective scientist, with aggregated observations and limited excerpts from interview data (attributed to pseudonymous informants) as illustrations. When it came time to write an overall analysis of the entire project, Angrosino found that such a strategy seemed inadequate. It was necessary to draw the reader into both the experiences of people with mental disabilities (people who are so much like us, and yet with a critical difference somewhat beyond our capacities to imagine) and the experiences of a researcher trying to figure out the patterns of the communities in which those people interact. But doing so by means of an implicitly distancing language of expository scientific writing and a blurring of individual differences was not an attractive option.

Angrosino therefore decided to try a form of "alternative ethnographic writing" and to present his material in the form of fictionalized stories that preserved the truth of individual experience without making explicit identifications of particular people with specific situations. There are many valid reasons for experimenting with nonexpository presentations of ethnographic material, but it may also be useful to think of such alternative genres as one response to the ethical quandary of observational research in transition.

Steps Toward an Ethic of Proportionate Reason in Observational Research

Because observational research, as it has evolved in recent times, is essentially a matter of interpersonal interaction and not a matter of objective hypothesis testing, it would seem that a standard for the making of ethical judgments appropriate to the analysis of "the morality of human action" (Gula, 1989, p. 272) is in order. Human action must always be interpreted in situational context, and not in terms of objective "codes." As Gula (1989) has pointed out, "No one enjoys an ahistorical vantage point which will give absolute certitude on moral matters" (p. 275). The notion of "proportionate reason"

is the key to such an interpretation (Cahill, 1981; Curran, 1979; Hoose, 1987; Walter, 1984). *Proportionalism* can sometimes refer to a strictly utilitarian cost-benefit analysis, but it is more properly thought of in this context as that which gives an action its moral meaning. In that sense, " 'proportionate' refers to the relation between the specific value at stake and the . . . limitations, the harm, or the inconvenience which will inevitably come about in trying to achieve that value" (Gula, 1989, p. 273). In other words, it is certainly important to "weigh the consequences" of an action, but consequences are only one part of the total meaning of the action. From this perspective, proportionate reason defines what a person is doing in an action (as an ethnographer engaged in an observational context) and not something merely added to the action already defined (i.e., the old notion of the ethnographic observer as extrinsic to the "action" he or she was recording).

There are three criteria that help us decide whether a proper relationship exists between the specific value and the other elements of the act (McCormick, 1973; McCormick & Ramsey, 1978). First, *the means used will not cause more harm than necessary to achieve the value.* In traditional moral terms, the ends cannot be said to justify the mans. If we take "the value" to refer to the production of some form of ethnography, then we must be careful to assure that "the means used" (e.g., inserting oneself into a social network, using photographs or other personal records) do not cause disproportionate harm. We might all agree that serving as *comadre* to an "informant's" child is sufficiently proportionate; we might well argue about whether becoming the lover of an "informant" (particularly if that sexual liaison is not intended to last beyond the time of the research) does more harm than an ethnographic book, paper, or presentation might be worth. Volunteering as a classroom tutor in a program serving adults with mental retardation whom one is interested in observing and interviewing is probably sufficiently proportionate; becoming a bill-paying benefactor to induce cooperation would, by contrast, be morally questionable.

The second criterion is that *no less harmful way exists at present to protect the value.* Some might argue that observational research always and inevitably compromises personal privacy, such that no form of research can ethically protect that cherished value. But most researchers (and others) would probably reject such an extreme view and take the position that there is real value in disseminating the fruits of ethnographic research so as to increase our knowledge and understanding of cultural diversity, or the nature of coping strategies, or any number of currently salient social justice issues. Granted that *all* methods have the potential to harm, we must be sure to choose those that do the *least* amount of harm, but that still enable us to come up with the sort of product that will be effective in communicating the valuable message. The strategy of writing ethnographic fiction, for example, is certainly not foolproof, as anyone with a knowledge of the population with which the ethnographer worked would be able to identify the "characters." But there is far less chance that an outside reader would be able to do so than would be the case with a report based on "objective" materials that are on the public record.

The third criterion is that *the means used to achieve the value will not undermine it.* If one sets out, for example, to use research in order to promote the dignity of people defined as mentally disabled, one must make sure that the research techniques do not subject such people to ridicule. Videotaping a group of people with mental retardation as they play a game of softball might conceivably result in viewers' concluding that such people are gallantly trying their best, but more likely it will result in confirming the popular stereotypes of such people as clumsy and inept, objects of pity (at best) or of scorn (at worst) rather than dignified individuals. Videotaping as an ethnographic method is ethically neutral; its appropriateness must be evaluated in this proportionate context.

McCormick (1973) suggests three modes of knowing whether there is a proportionate reason to carry out a suggested action. First, we know that a proper relation exists between a specific value and all other elements of an act through *experience,* which sometimes amounts to plain

common sense. For example, although we may think that it is important to encourage individual expression, we know from experience that doing so in the context of a community (such as a monastery) in which the individual is, by tradition, subordinate to the group will do real violence to the precepts by which the people we are intent on studying have historically formed themselves into a community. Experience might suggest that we rethink a decision to collect personal life histories of people in such groups in favor of focusing on the collective reconstruction of remembered common activities or events.

Second, we may know that a proper relationship exists through our own *intuition* that some actions are inherently disproportionate, even if we do not have personal experience of their being so. Janssens (1979, p. 63) asserts that we can discover disproportion through "feelings of disunity" within the self. For example, we should intuitively know that publishing personal material collected from people living in an oppressive, totalitarian society might ultimately result in that material being used against them, even in the absence of a direct or explicit threat. Our righteous goal of exposing the tyrannical regime might well backfire on the very people we are trying to help. Our intuition might warn us that an otherwise praiseworthy research proposal (e.g., to collect life histories or genealogies, or to observe the daily activities at the local market) could have harmful consequences if the product of the research were to fall into the wrong hands. A perception of what *could* happen (the result of intuition) is, of course, different from a perception of what *will* happen (the result of experience), and we are clearly not well served by dreaming up every conceivable disaster. It serves no purpose to allow ourselves to be paralyzed beforehand by overactive guilty consciences. But there is certainly a commonsensical hierarchy of plausibility that obtains in such cases—some things that *could* happen are more likely to come about than others.

Third, we know through *trial and error.* This is a mode of knowing that would be completely impossible under current institutional ethical guidelines. But the fact is that we do not and cannot know all possible elements in any given human social interaction, and the idea that we can predict—and thereby forestall—all harm is naïve in the extreme. An ethical research design would omit (or seek to modify) that which experience and intuition tell us is most likely to do harm; we can then proceed, but only on the understanding that the plan will be modified in the midst of the action when it becomes clear what is feasible and desirable in the real-life situation. For those uncomfortable with the indeterminacy of the term *trial and error,* Walter (1984) suggests "rational analysis and argument" (p. 38). By gathering evidence and formulating logical arguments, we try to give reasons to support our choices for certain actions over others. But the plain fact is that this way of knowing does, indeed, involve the possibility of committing an "error," perhaps one that may have unexpected harmful consequences. It is nonetheless disingenuous to hold that all possibility of harm can be anticipated and that any human action (including a research project based on interpersonal interaction) can be made risk-free. The moral advantage of the proportionate reasoning strategy is that it encourages the researcher to admit to an error once it has occurred, to correct it as far as possible, and to move on; the "objective" mode of research ethics, by contrast, encourages researchers to believe that they have eliminated all such problems, so that they are disinclined to own up to those problems that (perhaps inevitably) crop up and hence are less capable of repairing the damage. Those who work with people with developmental disabilities are familiar with the expression "the dignity of risk"; it is used to describe the "habilitation" of clients for full participation in the community. To deny the clients the possibility of making mistakes (by assuming that all risk can be eliminated beforehand and by failing to provide training in reasonable problem-solving techniques) is to deny them one of the fundamental characteristics of responsible adult living. One either lives in a shelter, protected from risk by objectified "codes," or one lives real life. The ethical paradigm suggested here does nothing more than allow the observational researcher the dignity of risk.

The logic of proportionate reason as a foundation for an ethical practice of social research might seem, at first glance, to slide into subjec-

tive relativism. Indeed, the conscience of the individual researcher plays a very large part in determining the morality of a given interaction. But proper proportionalism cannot be reduced to a proposition that an action can mean anything an individual wants it to mean, or that ethics is simply a matter of personal soul-searching. The strategy, rather, is based on a sense of community—the individual making the ethical decision must ultimately be guided by a kind of "communal discernment" (Gula, 1989, p. 278). When we speak of "experience," for example, we refer not just to personal experience, but to the "wisdom of the past" as it is embodied in the community's traditions. As such, it "demands broad consultation to seek the experience and reflection of others in order to prevent the influence of self interest from biasing perception and judgment. Using proportionalism requires more moral consultation with the community than would ever be required if the morality of actions were based on only one aspect . . . apart from its relation to all the . . . features of the action" (Gula, 1989, p. 278). That being the case, the ideal IRB would not be content with a utilitarian checklist of presumed consequences; it would constitute a circle of "wise" peers with whom the researcher could discuss and work out the sometimes conflicting demands of experience, intuition, and the potential for rational analysis and argument. The essential problem with current ethical codes, from the standpoint of qualitative observational researchers, is that they set up an arbitrary—and quite unnecessary—adversarial relationship between researchers and the rest of the scholarly community. The framework of proportionate reason implies that ethical research is the product of shared discourse, not of a species of prosecutorial inquisition.

◆ *Prospects for Observational Research*

As Adler and Adler (1994) remark in their chapter appearing in the first edition of this *Handbook,* "Forecasting the wax and wane of social science research methods is always uncertain" (p. 389), although they were able to do so by extrapolating from existing trends. In that same cautious spirit, we suggest that the future of observational research will most likely be in the direction of what Barrett (1996) refers to as "qualitative investigation with a difference" (p. 237). Barrett refers to the "demystification" of methodology; whereas once ethnographers spoke in a vague way about "rapport" or "empathy," they now publish and lecture extensively in the soul-baring manner suggested by the preceding literature review. One important result of that demystification is that observation can no longer be said to be a key to those grand, but somewhat opaque, units of analysis, "culture" and "society." Abu-Lughod (1991) has, indeed, urged qualitative researchers to use their techniques to undermine those concepts, which, she feels, have become the contemporary equivalents of "race"—categories that separate people, arrange them into hierarchies, and freeze the system so that institutionalized inequality prevails. To speak in such terms reifies the treatment of difference and hierarchy as somehow "natural." Observational research, by contrast, has the potential to turn our attention to what Abu-Lughod (1991) calls "the ethnography of the particular" (p. 154). Rather than attempting to describe the composite culture of a group or analyze the full range of institutions that supposedly constitute the society, the observational ethnographer will be able to provide a rounded account of the lives of particular people, the focus being on individuals and their ever-changing relationships rather than on the supposedly homogeneous, coherent, patterned, and (particularly in the case of traditional anthropologists) timeless nature of the supposed "group."

Abu-Lughod's position was foreshadowed by Geertz (1973) more than two decades ago (an indication of how slowly it takes some predictions to come to pass). Geertz advocated setting aside the traditional social science concern for "complexes of concrete behavior patterns" in favor of a "concern with the particular" based on the interpretation of "significant symbols" (p. 44). The above literature review clearly indicates that this shift is already taking place, in the interest of feminists and postmodernists of all persuasions in life

history and "meaning." At present, the type of social science represented by this approach to the observation of the particular coexists uneasily with more quantitative and positivistic schools of sociology, anthropology, and social psychology. There is, however, considerable doubt as to how long that link can survive, given the very different aims and approaches of the diverging branches of the once epistemologically unified social sciences. It seems not unlikely that observational techniques will find a home in a redefined genre of cultural studies (composed of the qualitative elements of the older disciplines), leaving their positivist colleagues to carry on in a redefined social science discipline.

Humanists though they may be, observational researchers are as dependent on the evolution of technology as their quantitative colleagues. *Observation* once implied a notebook and pencil, and perhaps a sketch pad; first still and then motion pictures were later added to the ethnographer's resources. Tape recorders have been supplemented (and, in some cases, even supplanted) by video recorders. Note taking has been enhanced by the advent of the laptop computer, and computer programs for the analysis of narrative data are being developed at a brisk pace. Observation-based ethnographers are, as a consequence, being pulled in two directions. On the one hand, they speak the theoretical language of "situatedness," indeterminacy, and relativism; but on the other hand, they rely more and more on technology that suggests the capturing of "reality" in ways that could be said to transcend the individual researcher's relatively limited capacity to interpret. The technology makes it possible for the ethnographer to record and analyze people and events with a degree of particularity that would have been impossible just a decade ago, but it also has the potential to privilege what is captured on the record at the expense of the lived experience as the ethnographer has personally known it. It would be foolish to suggest that for the sake of consistency, observation-based ethnographers should eschew further traffic with sophisticated recording and analyzing technology. But it would be equally foolish to assume that the current very strong trend in the direction of individualized particularization can continue without significant modification in the face of technology that has the perceived power to objectify and turn into "data" everything it encounters. Perhaps it will become necessary for us to turn our observational powers on the very process of observation, to understand ourselves not only as psychosocial creatures (which is the current tendency) but as users of technology. As Postman (1993) has pointed out, technological change is never merely additive or subtractive, never simply an aid to doing what has always been done. It is, rather, "ecological" in the sense that a change in one aspect of behavior has ramifications throughout the entire system of which that behavior is a part. "Surrounding every technology are institutions whose organization . . . reflects the world-view promoted by that technology" (p. 18). Under those circumstances, perhaps the most effective use we can make of observational techniques in the near future will be to discern the ethos of the technology that we can no longer afford to think of as a neutral adjunct to our business-as-usual. It is a technology that itself has the capacity to define our business. We need to turn our observational powers to what happens not just when "we" encounter "them," but when "we" do so with a particular kind of totalizing technology.

Nevertheless, it seems quite clear that the once-unquestioned hegemony of positivistic epistemology that encompassed even so fundamentally humanistic a research technique as observation has now been shaken to its roots. One telling indication of the power of that transition—and a challenging indication of things to come—is a recent comment by Stephen Jay Gould (1998), the renowned paleontologist and historian of science, who has ruefully admitted:

> No faith can be more misleading than an unquestioned personal conviction that the apparent testimony of one's eyes must provide a purely objective account, scarcely requiring any validation beyond the claim itself. Utterly unbiased observation must rank as a primary myth and shibboleth of science, for we can only see what fits into our mental space, and all description includes interpretation as well as sensory reporting. (p. 72)

■ *Notes*

1. The critique of objectivity was certainly not invented by the postmodernists. Indeed, a good case can be made that the prescient, discipline-spanning scholar Gregory Bateson (1972) was contributing rigorous and systematic analyses of the place of observers in the field long before it was considered possible to question the rationale of striving to eliminate "observer bias." His "cybernetic" theory suggested that the observer is inevitably tied to what is observed. Nevertheless, the debate that currently commands the attention of ethnographic researchers was jump-started by critics of the cultural studies persuasion, and the debate is most deeply informed by their vocabulary as well as by their specific epistemological concerns.

2. *Dialogue* need not be taken literally to mean a conversation between two parties; in practice, it often consists of multiple—even contradictory—voices.

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