

Politics and Ethics in Qualitative Research

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"Just Do It!"

FIELDWORK is fun; it is easy; anyone can do it; is salutary for young academics to flee the nest; and they should be able to take any moral or political dilemmas encountered in their stride. There has always been a somewhat pragmatic, if not reductionist, tradition in qualitative research that was exemplified by Everett Hughes's "fly on your own" strategy for students at Chicago (Gans, 1967, p. 301). Of course, the classical anthropologists engaged in long and lonely involvement in distant settings and had to solve their problems individually and on site (Clarke, 1975, p. 105), and something of this tradition—geared to the solo researcher, absent for a considerable period of time, and cut off from the university—was conveyed by the precepts of the Chicago school. This style of qualitative research holds that it is healthy and wholesome for students and aspiring social scientists to get "the seats of their pants dirty by *real* research" (Park, quoted in Burgess, 1982, p. 6; emphasis in original). They should abandon the classroom in order to knock on doors, troop the streets, and join groups; they should just "get in there and see what is going on" (as Howard Becker advised a bemused British student asking what "paradigm" he should employ in the field; Atkinson, 1977, p. 32).

In contrast, there are voices that alert us to the inherent moral pitfalls of participant observation and that warn us of the essentially "political" nature of all field research. In this model, qualitative research is seen as potentially volatile, even hazardous, requiring careful consideration and preparation before someone should be *allowed* to enter the field. Without adequate training and supervision, the neophyte researcher can unwittingly become an unguided projectile bringing turbulence to the field, fostering personal traumas (for researcher and researched), and even causing damage to the discipline. This position was powerfully argued by John Lofland at an ASA seminar on participant observation, where he virtually demanded a certification of competence before the researcher be let loose in the field. During the past decade, moreover, these two divergent stances have been challenged by the impact of feminist, racial, and ethnic discourse that has not only made visible new research areas but also has raised critical issues related to a politically engaged research dialectic (Welch, 1991). These have profound implications for the ethics and politics of research (Fonow & Cook, 1991; Grossberg, Nelson, & Treichler, 1992; Reinhartz, 1992).

AUTHOR'S NOTE: I would like to thank the editors of this volume, and the two readers for this chapter, for their valuable comments on my first draft. I also wish to extend my gratitude to Derek Phillips, Peter K. Manning, Hans Wermolder and John Van Maanen for their critical advice while I was preparing this chapter.

My position in this chapter will be to argue forcibly for the "get out and do it" perspective. Understandably, no one in his or her right mind would support a carefree, amateuristic, and unduly naive approach to qualitative research. But, at the same time, I would warn against leaning too far toward a highly restrictive model for research that serves to prevent academics from exploring complex social realities that are not always amenable to more formal methods. My sympathies for this view have been powerfully shaped by my own background as a sociologist who engaged in research that painfully raised a whole range of largely unexpected political and ethical issues (Punch, 1986, 1989), related to stress in the field situation, research fatigue, confidentiality, harm, privacy and identification, and spoiling the field. In two projects that commenced with supportive sponsors, I encountered an accumulation of unanticipated difficulties, such as varying interpretations of the research bargain over time, disputes about contractual obligations, restrictions on secondary access, intimidation via the law, disagreement on publication, and even an (in my view unethical) appeal to professional ethics in an attempt to limit my research. Those issues are not exclusive to projects employing observation, but perhaps they are most likely to occur in an acute way there than in other styles of work.

Furthermore, I trust that many of the views presented in this chapter are also applicable to other styles of qualitative research. Qualitative research covers a spectrum of techniques—but central are observation, interviewing, and documentary analysis—and these may be used in a broad range of disciplines. Indeed, contemporary researchers are to be found within an extensive spectrum of groups and institutions involving differing time spans and types of personal engagement (Burgess, 1982; Hammersley & Atkinson, 1983; Shaffir & Stebbins, 1991). It is probably the case, however, that in Anglo-American universities (with an "apprenticeship" model of graduate education unlike that in most continental European institutes), most researchers will first encounter fieldwork while engaged on a dissertation that is mostly a solo enterprise with relatively unstructured observation, deep involvement in the setting, and a strong identification with the researched. This can mean that the researcher is unavoidably vulnerable and that there is a considerably larger element of risk and uncertainty than with more formal methods.

There is here too an absolutely central point that much field research is dependent on one person's perception of the field situation at a given point in time, that that perception is shaped both by personality-and by the nature of the interaction with the researched, and that this makes the researcher his or her own "research instrument."

This is fundamentally different from more formal models of research, and it also bedevils our evaluation of what "really" happened because we are almost totally reliant on one person's portrayal of events. This is amplified if we further accept that there are a number of potentially distorting filters at work that militate against full authenticity on methods, and that censor material on the relationships with the human "subjects" concerned.

Here I am assuming that qualitative fieldwork employs participant observation as its central technique and that this involves the researcher in prolonged immersion in the life of a group, community, or organization in order to discern people's habits and thoughts as well as to decipher the social structure that binds them together (McCall & Simmons, 1969; Van Maanen, 1979). Far more than with other styles of social research, then, this implies that the investigator engages in a close, if not intimate, relationship with those he or she observes. Crucial to that relationship is access and acceptance, and elsewhere I have spoken of "infiltration" as a key technique in fieldwork (Punch, 1986, p. 11) even though the concept is negatively associated with spying and deception (Erikson, quoted in Bulmer, 1982, p. 150). Entry and departure, distrust and confidence, elation and despondency, commitment and betrayal, friendship and abandonment—all are as fundamental here as are dry discussions on the techniques of observation, taking field notes, analyzing the data, and writing the report. Furthermore, acute moral and ethical dilemmas may be encountered while a semiconscious political process of negotiation pervades all fieldwork. And both elements, political and ethical, often have to be resolved *situationally*, and even spontaneously, without the luxury of being able to turn first to consult a more experienced colleague. The dynamics and dilemmas associated with this area of fieldwork can be summarized crudely in terms of getting in and getting out, and of one's social and moral conduct in relation to the political constraints of the field.

On the Politics of Fieldwork

To a greater or lesser extent, politics suffuses all social scientific research (Guba & Lincoln, 1989, p. 125). By *politics* I mean everything from the micropolitics of personal relations to the cultures and resources of research units and universities, the powers and policies of government research departments, and ultimately even the hand (heavy or otherwise) of the central state itself (Bell & Newby, 1977; Hammond, 1964): All of these contexts and constraints crucially influence the -design, implementation, and out-

comes of research (Gubrium & Silverman, 1989). This is important to convey to fledgling researchers, who may imbibe a false view of the research process as smooth and unproblematic ("The unchanging researcher makes a unilinear journey through a static setting"; Hunt, 1984, p. 285), whereas we should be drawing their attention to the political perils and ethical pitfalls of actually carrying out research. An additional motive for doing this is to espouse the view that fieldwork is definitely not a soft option, but, rather, represents a *demanding* craft that involves both coping with multiple negotiations and continually dealing with ethical dilemmas.

But perhaps collectively we are ourselves largely responsible for the "conspiracy" in selling the neat, packaged, unilinear view of research. Successful studies attract the limelight; failures are often neglected. Dilemmas in the field are glossed over in an anodyne appendix, and it may even be deemed inappropriate for the "scientist" to abandon objectivity and detachment in recounting descriptions of personal involvement and political battles in the field setting. This can be reinforced by the strictures of publishers, who may find personal accounts anecdotal, trivial, and scarcely worthy of space (Punch, 1989, p. 203). As Clarke (1975) observes, "A large area of knowledge is systematically suppressed as 'non-scientific' by the limitations of prevailing research methodologies" (p. 96).

In contrast, some accounts of field research touch on the stress, the deep personal involvement, the role conflicts, the physical and mental effort, the drudgery and discomfort—and even the danger—of observational studies for the researcher. Yablonsky (1968) was threatened with violence in a commune, and Thompson (1967) was beaten up by Hell's Angels; Schwartz (1964) was attacked verbally and physically during his study in a mental hospital, where he was seen as a "spy" by both patients and staff; and Vidich and Bensman (1968) were caricatured, in a Fourth of July procession in the town they had studied, by an effigy bending over a manure spreader. Wax (1971) was involved in dangerous and stressful situations in Japanese relocation camps, and she was denounced as a "communist agitator" during research on Native American reservations. Burns (1977) was refused publication for his study of the BBC; Wallis (1977) was tailed and harassed by members of the Scientology movement; and, in a project within a police department, a researcher "literally had to block a file-cabinet with his body to keep two armed internal affairs officers from taking observers' records. Meanwhile the principal investigator was frantically contacting the chief of police to get internal affairs called off" (Florez & Kelling, 1979, p. 17).

These examples could be multiplied by horror stories gleaned from the academic circuit, where "tales of the field" (Van Maanen, 1988) abound of obstructionist gatekeepers, vacillating sponsors, factionalism in the field setting that forces the research to choose sides, organizational resistance, respondents subverting the research role, sexual shenanigans, and disputes about publication and the veracity of findings. Such pitfalls and predicaments can rarely be anticipated, yet they may fundamentally alter the whole nature and purpose of the research.

These personal and anecdotal accounts form an oral culture of moral and practical warnings; they are not widely written of, according to John Van Maanen (personal communication, 1993), largely because we have failed to develop a "genre or narrative convention within our standard works" that would shape a taken-for-granted imperative that field-workers own up to the manner in which they solved such issues during their research (but see Sanjek, 1990, on "fieldnotes"). In contrast, there is a stream of thought that does make exposure of affectivity and of the research process central and that is represented by feminist research (Roberts, 1981). This not only attacks traditional methodology as an instrument of repression but also in some cases, argues for "total immersion" in the field; this new "epistemology of insiderness" (Reinharz, 1992, p. 259) has led feminist scholars to an attempt "to rescue emotion from its discarded role in the creation of knowledge" (Fonow & Cook, 1991, p. 11). This powerful and significant contribution to the recent debate on the politics of research is in reaction to the patriarchal nature of academic life and the "research infrastructure" allied to an effort to construct a feminist epistemology and methodology. Fonow and Cook (1991) focus on a number of themes in the literature on feminist methods: "reflexivity; an action orientation; attention to the affective components of research; and use of the situation-in-hand" (pp. 1-5).

In essence, much research is informed by the experience of oppression owing to sexism, and the research process may well contain an element of "consciousness-raising," of emotional catharsis, and of increased politicization and activism. As the aim of certain strands of feminist research is praxis leading to liberation (Mies, 1991), this has profound implications for "the statement of purpose, topic selection, theoretical orientation, choice of method, view of human nature, and definitions of the researcher's role" (Fonow & Cook, 1991, p. 5). This action component is shared with black studies, Marxism, and gay and lesbian studies and permeates research with an explicitly *political* agenda. Research by women on women to assist women has undoubtedly opened up fresh new arenas largely inaccessible to males and this enrichment has frequently been embedded within qualitative research precisely because this is held

to be more compatible than formal, quantitative methods with feminist scholarship (Hammersley, 1993; Jayaratne & Stewart, 1991; Reinharz, 1992). Feminist research has, for instance, fostered studies of obscene telephone calls, violence against women (shelters for battered females), single-gender college residences, sexual harassment, pornography, AIDS clinics, abortion, and discrimination in the workplace. In effect, the impact of feminist research has been to awaken the whole issue of gender in research activities and to politicize the debate on the conduct of research; similar arguments have been raised about race and ethnicity.

In some cases there is an openness to "complete transformation" through total participation and a belief that consciousness-raising will become the "ground work for friendship, shared struggle and identity change" (Reinharz 1992, p. 68). This has aided in bringing affectivity into accounts of research and has also exposed the reality that much qualitative, observational work was conducted by privileged white males. There are profound epistemological and methodological issues here that I cannot possibly tackle within the confines of this chapter, but I suspect that many traditional ethnographers, brought up in a scholarly convention of "openness" to the field setting and "objectivity" with regard to data, would be concerned that explicitly ideological and political research would overly predetermine the material gleaned in observational studies. This, in turn, would doubtless lead to a riposte about the disingenuousness of believing in objectivity through the eyes of white male academics. My point is that the traditionalists tended to eschew "politics," to avoid "total immersion," and to be wary of "going native," all of which, in contrast, are elements of feminist methods. This debate has illuminated certain research dilemmas in an acute and fresh way that needs to be taken into account in all that follows below. Rather than enter that debate, which poses issues at the ideological and institutional levels, I shall focus here on those practical and mundane elements that continually influence the "politics" of fieldwork in many research projects.

Hence I wish simply to focus on certain features that are not always clearly articulated in accounts but that have a material impact on qualitative research in general and fieldwork in particular and that shape the politics of research.

Researcher personality. The personality of the researcher helps to determine his or her selection of topics, his or her intellectual approach, and his or her ability in the field (Clarke, 1975, p. 104). But often we are left in the dark as to the personal and intellectual path that led researchers to drop one line of inquiry or to pursue another topic. We require more intellectual autobiographies to clarify why academics end up studying what they do (Okely & Callaway, 1992). Family circumstances can be important in terms of absences and travel, and spouse's support, or lack of it, can prove crucial to the continuation of a field project.

Geographic proximity. One simple factor that is often glossed over in terms of selecting topics and field settings is geographic proximity. There may be something romantic about Evans-Pritchard, Malinowski, and Boas setting off stoically into the bush, where they lived in relative isolation and virtuous celibacy, but some researchers just travel conveniently down the road to the nearest morgue, mental hospital, or action group.

Nature of the research object. The nature of the research object—be it a community, a formal organization, or an informal group—is of significance for access, research bargains, funding, and the likelihood of polarity and conflict in the research setting (Punch, 1989; Spencer, 1973).

Researcher's institutional background. The reputation of the researcher's institutional background can be of considerable importance in opening or closing doors. The backing of prestigious academic institutions and figureheads may be vital to access in some settings but irrelevant, or even harmful, in others. For instance, Platt (1976, p. 45) records a case in which researchers in Britain were able to get a member of Parliament to organize a speech in the House of Commons that led to certain doors being opened for them.

Gatekeepers. Gatekeepers can be crucial in terms of access and funding (Argyris, 1969). The determination of some watchdogs to protect their institutions may ironically be almost inversely related to the willingness of members to accept research. Klein (1976) remarks, "Social science is not engaged by 'industry' or organizations, but by individuals in gatekeeping or sponsorship or client roles. The outcome, therefore, is always mediated through the needs, resources, and roles of such individuals" (p. 225). Researchers may suffer by being continually seen as extensions of their political sponsors within the setting despite their denials to the contrary. Furthermore, gatekeepers need not be construed only in terms of government agencies and corporate representatives, but can also be found in scientific funding bodies, among publishers, and within academia. The intellectual development of the discipline, academic imperialism, the institutional division of labor, the selection and availability of specific supervisors, backstage, bargaining, precontract lobbying, departmental distribution of perks (research assistance, travel money, -typing support), and patronage can all play roles in determining the status of, and resources for, field research, and in speci-

fying why some projects are launched and others buried (Dingwall, Payne, & Payne, 1980, Sharrock & Anderson, 1980; Shils, 1982). It is somewhat encouraging to read that even Whyte had difficulty in publishing his now-classic 1943 book *Street Corner Society*, in having it reviewed and taken seriously, and fluctuating sales have reflected the fads and fashions of postwar sociology. The acceptance of his research for a Ph.D. at Chicago was also contingent on Hughes' s championing of him against a critical Wirth (Whyte, 1981, p. 356).

Status of field-workers. The impact that the presence of researchers has on the setting is related to the status and visibility of the field-workers. The "lone wolf often requires no funding, gains easy access, and melts away into the field. The "hired hand," in contrast, may come with a team of people, be highly visible, be tied to contractual obligations, and be expected to deliver the goods within a specified period of time (Wycoff & Kelling, 1978).

Expectations in team research. A feature of research that has rarely been examined is the variety of expectations and roles in *team* research that can hinder behavior in the field and lead to conflict about outcomes. In team research, leadership, supervision, discipline, morale, status, salaries, career prospects, and the intellectual division of labor can promote unexpected tensions in the field and lead to disputes about publication. Junior assistants may fear that a senior researcher will prematurely publish to increase his or her academic status while cynically exploiting their data, spoiling the field, and ruining their chances of collecting separate data for a dissertation. A love affair breaking up between team members can also spell disaster and undermine timetables and deadlines. Workloads, ownership of data, rights of publication, and career and status issues are all affected by the constraints of team research. Al Reiss, Jr., in operating a team investigating police behavior, had to make it clear that serious "deviance" by a team member might threaten the whole project, and that he also had an employer-employee relationship with them that meant he was prepared to dismiss people if necessary (statements made at an ASA seminar on field research). Bell (1977) presents a graphic portrait of the problems that beset the restudy of the community of Banbury in Britain. The project leader was rarely present, the team never really jelled as a unit, the field supervisor left early to take up an academic appointment, and the two research assistants wanted to collect data for their dissertations as well as for the project; further, data were withheld from the supervisor because the others were worried that he "would in some way run off with the data and publish separately" (p. 55).

The structural and status frustrations of the hired hand (particularly the temporary research assistant virtually abandoned to the field) may mean that he or she suffers from poor morale becomes estranged from the parent organization is strongly tempted toward co-optation, becomes secretive toward supervisors, and is a "bother" requiring "unusually intense and patient supervision" (Florez & Kelling, 1979. p. 12). He or she is particularly in danger of "going native."

Other factors affecting research in the field. The actual conduct of research and success in the field can be affected by myriad factors, including age, gender, status, ethnic background, overidentification, rejection, factionalism, bureaucratic obstacles, accidents, and good fortune. But, again, we rarely hear of failures, although Diamond (1964) recounts how he was ejected from the field in Nigeria, and Clarke (1975) speaks of field-workers who nearly went insane, panicked, or got cold feet and never actually got to the field, "but we are systematically denied public information on what happens" (p. 106). Observational studies are often associated with young people (graduate students, research assistants), and some settings may require a youthful appearance and even physical stamina (as in Reimer's 1979 study of construction workers).

Gender and race close some avenues of inquiry but clearly open up others. Martin (1980), in her study of women in policing, could not penetrate the world of the policemen's locker room or out-of-work socializing. In masculine worlds the female researcher may have to adopt various ploys to deal with prejudice, sexual innuendo, and unwelcome advances. Hunt (1984) realized that she was operating in a culture where several features of her identity—white, female, educated outsider—were impediments to developing rapport and trust with different categories within the police and had to engage in a transformation from "untrustworthy feminine spy" to "street woman researcher" whereby she renegotiated gender-to combine elements of masculinity and femininity. The compromises this involved would doubtless enrage many contemporary feminists, but they force the female field-worker to get out or else accept a measure of "interactional shitwork" (Reinharz, 1992). The limitations associated with views on race and gender mean that it is impossible in many police forces for a white female to patrol alone with a black male officer. Women often have to cope too with the conflict between their desire and need to continue research (e.g., for career purposes) and their encountering "sexual harassment, physical danger, and sexual stereotyping"; furthermore, in a society that is "ageist, sexist, and hetero-sexist, the young, female researcher may be defined as a sexual object to be seduced by heterosexual males" (Reinharz, 1992, p. 58)..

A young student, however, may be perceived as nonthreatening and may even elicit a considerable measure of sympathy from respondents. But rather than concluding that fieldwork is not for

ing age and increased status can open doors to fruitful areas of inquiry, such as senior management in business. Personality, appearance, and luck may all play roles in exploiting unexpected avenues or overcoming unanticipated obstacles in the field.

Publishing. A harmonious relationship in the field may come unstuck at the moment of writing an impending publication where the researcher's material appears in cold print. The subjects of research suddenly see themselves summarized and interpreted in ways that may not match up with their own partial perspectives on the natural setting. Where the research bargain includes an implicit or explicit obligation to consult the group or institution on publication, severe differences of opinion can arise. These may be almost completely unanticipated by the researcher, in the sense that it is difficult to predict what organizational representatives will find objectionable (Bums, 1977). Vidich and Bensman's (1968) study of "Springdale" provoked a scandalized reaction that raised fundamental issues related to invasion of privacy, the ethics of research (on identity, harm, ownership of data, and so on), and responsibilities to Cornell University, which had sponsored the research (and which proved unduly sensitive to the outcry from the community). There were also protests from other academics. Progressive and radical institutions, highly critical of the establishment and ideologically committed to openness and publication, may themselves be highly sensitive to criticism because of their marginality, susceptibility to discrediting, and desire for legitimacy (Punch, 1986, pp. 49-70).

Social and moral obligations. Finally, what social and moral obligations are generated by fieldwork? This issue forms a major part of what follows in this chapter and can be viewed as having two central parts. On the one hand, there is the nature of the researcher's personal relationships with people he or she encounters in the field. On the other hand, there are the moral and ethical aspects related to the purpose and conduct of research itself. In effect, how far can you go?

Ethical Features of Qualitative Research

Issues

The view that science is intrinsically neutral and essentially beneficial disappeared with the revelations at the Nuremberg trials (recounting *tion* camp inmates) and with the role of leading scientists in the Manhattan Project, which led to the dropping of atomic bombs on Japan in 1945. Controlling science, however, raises resilient practical, ethical, and legal issues that are a matter of constant debate. The questions involved confront us with fundamental dilemmas, such as the protection of the subjects versus the freedoms to conduct research and to publish research findings. An understanding of this area needs to be rooted in knowledge of a number of studies that have given rise to moral and ethical questions.

In medical research, for instance, actual physical harm can be done to subjects, as in the Tuskegee Syphilis Study and in the Willowbrook Hepatitis Experiment, and patients' rights can be violated, as when live cancer cells were injected beneath the skin of nonconsenting geriatrics (Barber, 1976; Brandt, 1978; Katz, 1972). This background is important because, for a number of reasons, the attempt to control *biomedical* research, and to protect its subjects, has also become the model for the social sciences (Reiss, 1979). In social science, frequent reference is made to a number of studies that have raised blood pressures on ethical aspects of research. The revelations of Vidich and Bensman (1968) about the community of "Springdale" caused a furor among the townspeople and also fellow academics in relation to identification, harm, sponsorship, and professional ethics. Festinger, Riecken, and Schachter's (1956) work on membership in a sect involved a measure of deception and also implicit if not explicit affirmation for the group that could scarcely be described as nondirective. In the 1960s, American academics were shocked on discovering CIA involvement in the source of funding for "Project Camelot" (Horowitz, 1970). The CIA was also responsible for secretly distributing LSD to visitors to a brothel and then filming the results using a hidden camera; one person committed suicide while under the influence of the LSD (Sieber, 1992, p. 68).

In the Wichita Jury Study, microphones were hidden to record juries' deliberations. Milgram's (1963) renowned psychological experiment on authority required unwitting subjects to think that they were causing "pain" to others in a laboratory situation. Disguise and deception were used in La Pierre's (1934) pioneering study of prejudice, when he entered restaurants and hotels accompanied by a Chinese couple, and also in Lofland and Lejeune's (1960) study of reactions of aspiring members of Alcoholics Anonymous, in which students posed as alcoholics. There is also the well-known, if not now notorious, research of Laud Humphreys (1970, 1972) on homosexuals, whom he observed in a public toilet and later questioned in their homes under the guise of a

different project. (He recorded their car license plate numbers on first encounter and traced them to their homes; he then changed his hairstyle, clothes, and car and claimed he was conducting a "social health survey.") On the one hand, he received the coveted C. Wright Mills Award from the SSSP, but on the other hand there were efforts undertaken to revoke his Ph.D. (and an irate Alvin Gouldner socked him on the jaw!). For more details and further debate on these studies, the reader is referred to Klockars and O'Connor (1979) and Bulmer (1982), and also to texts dealing with ethical issues in research, such as Sjoberg (1968), Bames (1979), Diener and Crandall (1978), Boruch and Cecil (1983), Rynkiewich and Spradley (1976), and special issues of *American Sociologist* (1978) and *Social Problems* (1973, 1980).

In essence, most concern revolves around issues of harm, consent, deception, privacy, and confidentiality of data. And, in a sense, we are all still suffering for the sins of Milgram. His controversial research methods in laboratory experiments, allied to the negative reactions to revelations about medical tests on captive, vulnerable, and nonconsenting populations, led to the construction of various restrictions on social research. Academic associations have formulated codes of professional conduct and of ethics, and some research funding is dependent on researchers' ascribing to ethical guidelines. This codification presents a number of dilemmas, particularly for researchers who engage in fieldwork. For instance, the concept of consent would seem to rule out covert research, but how "honest" do you actually have to be about your research purpose? And the conflict orientation of some scholars—in terms of Becker's (1967) call to take sides or Douglas's (1979) demand that we deceive the establishment in order to expose it—seems to force moral choices upon us. There is a further dimension related to research on "deviants" who may engage in criminal and violent behavior: Does conscience allow us to witness this? Would we be prepared to protect people engaged in illegality from the authorities? The generality of codes often does not help us to make the fine distinctions that arise at the *interactional* level in participant observation studies, where the reality of the field setting may feel far removed from the refinements of scholarly debate and ethical niceties.

These issues have raised fundamental debate about the very nature of the academic enterprise and about the relationships among social science and research ethics, bureaucratic protection and secrecy, political control and individual rights and obligations (Wilkins, 1979, p. 113). Does the end of seeking knowledge justify the scientific means (Homan & Bulmer, 1982, p. 114)? What is public and what is private? When can research be said to be "harming" people? Does the researcher enjoy any immunity from the law when he or she refuses to disclose information? In what way can one institutionalize ethical norms—such as respect, beneficence, and justice (Reiss, 1979)—to ensure accountability and responsibility in the use and control of information on human subjects? And to what extent do betrayal of trust, deception, and invasion of privacy damage field relationships, make the researcher cynical and devious, enrage the "participants" in research, harm the reputation of social scientific research, and lead to malpractice in the wider society? All of these points generate ethical, moral, legal, professional, and practical problems and positions that continue to reverberate at conferences, during discussions, and in print. Here I intend to examine these issues in terms of a number of practical problems encountered, particularly in fieldwork situations that generate an ethical component. Again, I wish to clarify that my focus is predominantly sociological and anthropological and that I have in mind largely the lone researcher engaged in an observational study, where a personal involvement with the "subjects" in the field continually poses moral and ethical dilemmas.

At a more ideological, methodological, and institutional level, however, I wish first to touch on three developments that have materially affected the ethical dimension in research. First, the women's movement has brought forth a scholarship that emphasizes identification, trust, empathy, and non-exploitive relationships. Feminist research by women on women implies a "standpoint epistemology" that not only colors the ethical and moral component of research related to the power imbalances in a sexist and racist environment, but also inhibits deception of the research "subjects." Indeed, the gender and ethnic solidarity between researcher and researched welds that relationship into one of cooperation and collaboration that represents a personal commitment and also a contribution to the interests of women in general (e.g., in giving voice to "hidden women," in generating the "emancipatory praxis," and in seeing the field settings as "sites of resistance"). In this sense the personal is related to the ethical, the moral, and the political standpoint. And you do not rip off your sisters.

Second, the stream of evolutionist and interventionist work, or "action" research, has developed to a phase where "subjects" are seen as partners in the research process. To dupe them in any way would be to undermine the very processes one wants to examine. Rather, they are seen as "respondents, participants, stakeholders" in a constructivist paradigm that is based on avoidance of harm, fully informed consent, and the need for privacy and confidentiality. If "action research" actually seeks to empower participants, then one must be open and honest with them; as two leading proponents of "fourth-generation evaluation" research put it:

If evaluators cannot be clear, direct, and undeceptive regarding their wish to know how stakeholders make sense of their contexts, then stakeholders will be unclear, indirect, and probably misleading regarding how they do engage in sense-making and what their basic values are. Thus deception is not only counter to the posture of a constructivist evaluator, in that it destroys dignity, respect, and agency, but it also is counterproductive to the major goals of a fourth generation evaluation. Deception is worse than useless to a nonconventional evaluator; it is destructive of the effort's ultimate intent. (Guba & Lincoln, 1989, p. 122)

Third, and last, the concern with harm, consent, confidentiality, and so on has led some government agencies to insist that financing of research be contingent upon an ethical statement in the research proposal and that academic departments set up review and monitoring bodies to oversee the ethical component in funded research (Kimmel, 1988; Sieber, 1992). In brief, these three forces have had a powerful impact on consciousness about ethics in research and have, in particular, argued against deception and for taking the interests of the research "subjects" into account.

Codes and Consent

One significant element in such codes is the concept of "informed consent," by which the subjects of research have the right to be informed that they are being researched and also about the nature of the research. Federal agencies in the United States follow the rule for sponsored research "that the potential research subject understand the intention of the research and sign an 'informed consent' form, which incidentally must specify that the subject may withdraw from the research project at any time" (Weppner, 1977, p. 41). The key question here is. To what extent is this appropriate to much participant observation research? As Weppner (1977) observes, this threatens the continued existence of much "street-style" ethnography. When Powdermaker (1966), for instance, came face-to-face with a lynch mob in the Deep South, was she supposed to flash an academic identity card at the crowd and coolly outline her presence? In these and comparable circumstances, gaining consent is quite inappropriate, because activity is taking place that cannot be interrupted. In much fieldwork there seems to be no way around the predicament that informed consent—divulging one's identity and research purpose to all and sundry—will kill many a project stone dead.

And there are simply no easy answers provided by general codes to these situational ethics in fieldwork. For instance, researchers often confess to professional "misdemeanors" while in the field (Wax, 1971, p. 168), Malinowski (1967) socked a recalcitrant informant on the jaw; Powdermaker (1966) ceased to concern herself with the ethics of recording events in Hollywood unknown to the participants; Dalton (1964) fed information on salaries to a secretary in exchange for information on her male friend that was necessary for his research; and Bowen (1964) deliberately manipulated the research situation when it became impossible for her to maintain personal objectivity. The doyen of qualitative researchers, Whyte (1955, pp. 333-336), broke the law by "repeating" at elections, engaged in "retrospective falsification," and admits to having violated professional ethics (see also Whyte, 1984). What sanctions should we impose for these breaches of "professional" standards? Should we ignominiously drum these miscreants out of the profession? That seems a rather severe punishment for coming clean on their predicaments in the field.

My position is that a professional code of ethics is beneficial as a *guideline* that alerts researchers to the ethical dimensions of their work, particularly *prior* to entry. I am not arguing that the field-worker should abandon all ethical considerations once he or she has gotten in, but rather that informed consent is unworkable in some sorts of observational research. Furthermore, Reiss (1979, pp. 72, 77) notes that consent often serves to *reduce* participation and, although "definitive evidence is lacking," refusals seem more frequent from high-status, powerful people than from low-status, less powerful individuals. The ethicist might rail at my placing practical handicaps above ethical ideals, but I am seriously concerned that a strict application of codes will restrain and restrict a great deal of informal, innocuous research in which students and others study groups and activities that are unproblematic but where explicitly enforcing rules concerning informed consent will make the research role simply untenable.

Deception

What is plain is that codes and consent are opposed to deception. In contrast, the proponents of conflict methodology, which sometimes closely resembles investigative journalism (Wallraff, 1979), would argue that it is perfectly legitimate to expose nefarious institutions by using a measure of deceit. However, a number of studies that were not inspired by conflict methodology have employed some element of deception. In a neglected classic titled *Men Who Manage*, Dalton (1959) recounts how he investigated management in a number of firms by working covertly as a manager over a period of years. He used secretaries to gain information, employed out-of-work socializing to observe the-significance of club member-

ship for managers, utilized malcontents for their grievances against the organization, and manipulated intimates as "catalytic agents" to gain data (Dalton, 1964). In other projects, a researcher has joined a Pentecostal sect as if a novice; used plastic surgery, lost weight, lied about age, and adopted a "new personality" in order to study Air Force recruits (Sullivan, Queen, & Patrick, 1958); and entered a mental hospital as if a patient (Caudill, 1958). In other words, researchers have been prepared to use disguise, deception, and dissimulation in order to conduct research.

And perhaps *some* measure of deception is acceptable in *some* areas where the benefits of knowledge outweigh the harms that have been minimized by following convention on confidentiality and identity (and I fully acknowledge the sort of rationalizations this could lead to). One need not always be brutally honest, direct, and explicit about one's research purpose, but one should not normally engage in disguise. One should not steal documents. One should not directly lie to people. And, although one may disguise identity to a certain extent, one should not break promises made to people. Academics, in weighing up the balancing edge between overt-covert, and between openness-less than open, should take into account the consequences for the subjects, the profession, and, not least, for themselves.

I base this position on the view that subjects should not be harmed but also the pragmatic perspective that some dissimulation is intrinsic to social life and, therefore, also to fieldwork. Gans (1962) expresses this latter view neatly: "If the researcher is completely honest with people about his activities, they will try to hide actions and attitudes they consider undesirable, and so will be dishonest. Consequently, the researcher must be dishonest to get honest data" (p. 46). The crux of the matter is that some deception, passive or active, enables you to get at data not obtainable by other means. There are frequent references in the literature to field-workers as "spies" or "voyeurs," and an experienced researcher advises us to enter the field with a nebulous explanation of our purpose, to be careful that our deception is not found out until after we have left, and states that it is not "ethically necessary, nor methodologically sound, to make known specific hypotheses, background assumptions, or particular areas of interest" (Van Maanen, 1978, p. 334). So much for informed consent or, as a senior American academic at an ASA seminar on field methods put it bluntly, "You do lie through your teeth."

This is an extremely knotty area, because some academics argue precisely that researchers should be concerned with documenting abuses in public and business life. This is because they feel that convention on privacy, harm, and confidentiality should be waived when an institution is seen to be evading its public accountability (Holdaway, 1980, p. 324). Marx (1980) echoes this view when he suggests that perhaps different standards apply with respect to deception, privacy, informed consent, and avoiding harm to the researched against organizations that themselves engage in "deceitful, coercive and illegal activities" and are publicly accountable (p. 41). Can we salve our academic conscience by arguing that certain institutions deserve what they get? There seems to be no answer to this issue because it is impossible to establish a priori which institutions are "pernicious." One could visualize endless and fruitless debate as to which organizations should be included, particularly as many public bureaucracies of a relatively mundane sort are secretive and protective. The argument that they are also accountable is a telling one. But using covert research methods against them is likely only to close doors rather than to open them. The balance on this matter is ultimately a question for the individual researcher and his or her conscience in relation to feelings of responsibility to the profession and to "subjects." And it seems to be somewhat specious that academics can employ deception with high moral purpose against those they accuse of deception.

It is interesting, and even ironic, that social scientists espouse some of the techniques normally associated with morally polluted professions, such as policing and spying, and enjoy some of the moral ambivalence surrounding those occupations. The ironies and ambivalences are magnified when researchers study "deviants" and run the danger of what Klockars (1979) calls getting, "dirty hands" (p. 269). In getting at the dirt, one may get dirty oneself (Marx, 1980, p. 27). Klockars (1979) is clear on this; in research on deviants the academic promises *not* to blow the whistle and maintains "the immediate, morally unquestionable, and compelling good end of keeping one's promise to one's subjects" (p. 275; see also Polsky, 1969). His argument is that researchers *must* be prepared to get their hands dirty, but also that they protect themselves by approaching subjects as "decent human beings," and by engaging in *talk*. By discussing moral dilemmas openly, researchers can avoid the danger of concealing dirty means for "good" ends. Here I would fully support Klockars's (1979) standpoint:

The implication for field-work is to be most wary of any and all attempts to fashion rules and regulations, general guidelines, codes of ethics, or standards, of professional conduct which would allow well-meaning bureaucrats and concerned colleagues to mobilize punishments for morally dubious behavior. Doing so will, I think, only have the effect of forcing decent fieldworkers to lie, deceive, wear masks, misrepresent themselves,

hide the methods of their work, and otherwise dirty their hands more than their vocation now makes morally necessary, (p. 279)

In short, my position is to reject "conflict methodology" as a generally inappropriate model for social science. At the same time, I would accept some moderate measure of field-related deception providing the interests of the subjects are protected. A number of academics, however, take a very strong line on this area. Douglas's claim that basically "anything goes" is firmly opposed by Kai Erikson (cited in Bulmer, 1982). Among others, Erikson argues that it is unethical to misrepresent one's identity deliberately to gain entry into private domains one would otherwise be denied. It is also unethical to misrepresent deliberately the character of one's research. Bulmer (1982) supports the contention that the use of covert observation as a method is "neither ethically justified, nor practically necessary, nor in the best interest of sociology as an academic pursuit" (p. 217). This does not mean that it is *never* justified, but "its use requires most careful consideration in the light of ethical and practical considerations."

Bulmer (1982) then goes on to summarize his position in this debate usefully by arguing that the rights of subjects override the rights of science; that anonymity and confidentiality are necessary but not sufficient for subjects of research (we cannot predict the consequences of publication); and that covert observation is harmful to subjects, researchers, and the discipline. He adds that the need for covert research is exaggerated and that more attention should be paid to access as "overt insider." Also, for Bulmer, the role of "covert outsider" is less reprehensible than that of "covert insider and masquerading as a true participant." And, finally, social scientists should look outside their own profession for ethical guidance and should consider carefully the ethical implications of research before embarking on it. Much of this is sound advice, but it does mean closing avenues to certain types of research. And who is to perform the moral calculus that tells us what to research and what to leave alone?

Privacy, Harm, Identification, and Confidentiality

Conventional practice and ethical codes espouse the view that various safeguards should protect the privacy and identity of research subjects as Bulmer (1982) puts it, "Identities, locations of individuals and places are concealed in published results, data collected are held in anonymized form, and all data kept securely confidential" (p. 225). The last of these may require considerable ingenuity in these days of computer hackers. In general, there is a strong feeling among field-workers that settings and respondents should not be identifiable in print and that they should not suffer harm or embarrassment as a consequence of research. There are powerful arguments for respecting persons (see the "Belmont Report" on ethical principles governing research, discussed in O'Connor, 1979) and their dignity, and also for not invading their privacy. Exposing people's private domains to academics raises imagery of "Peeping Toms" and "Big Brother" (Mead, 1961). It does seem to be going a bit far to lie under beds in order to eavesdrop on conversations (Bulmer, 1982, p. 116). But what about attending meetings of Alcoholics Anonymous? Can we assume that alcoholics are too distressed to worry about someone observing their predicament (or that their appearance at A.A. meetings signals their willingness to be open about their problem in the company of others)? To a large extent, I feel that we can become too sensitive on this issue. There is no simple distinction between "public" and "private" while observation in many public and semi-public places is tolerable even when the subjects are not aware of being observed. Some areas are nonproblematic, such as observing the work of flight attendants while one is traveling, and others may be related to serious social problems, where some benefit may emerge from focusing on the issue (Weppner, 1977).

The major safeguard to place against the invasion of privacy is the assurance of confidentiality. But even such assurances are not watertight, and "sociologists themselves have often flagrantly betrayed confidence, undoing all the work of covers, pseudonyms, and deletions" (Rock, 1979). I mentioned earlier the tendency to choose sites close to one's university; pseudonyms can often be punctured by looking up the researcher's institutional affiliation at the time of the project. Everyone now knows that "Middletown" was Muncie, Indiana; that "Rainfall West" was Seattle; and that "Westville" was Oakland, California. Holdaway (1982) painstakingly uses a pseudonym for his research police station, but then refers in his bibliography to publications that make it plain that he studied the Metropolitan Police of London. And how do you disguise research conducted in readily identifiable cities such as London, New York, or Amsterdam? In addition, the cloak of anonymity for characters may not work with insiders who can easily locate the individuals concerned or, what is even worse, *claim* that they can recognize them when they are, in fact, wrong. Many institutions and public figures are almost impossible to disguise, and, if they cooperate in research, may have to accept a considerable measure of exposure, particularly if the popular media pick up on the research.

This makes it sometimes precarious to assert that no harm or embarrassment will come to the

researched (Reiss, 1979, p. 70). In the Cambridge -Somerville Youth Study there were apparently long-term negative consequences that emerged only when an evaluation study was conducted *30 years* after the original project (Kimmel, 1988, pp. 18-20). It is extremely difficult to predict to what uses one's research will be put; Wallis (1977) states that we must not cause "undeserved harm," but who is to define "deserved" and "undeserved" harm? Even people who have cooperated in research may feel hurt or embarrassed when the findings appear in print (e.g., the reactions in "Cornerville"—i.e., the North Side of Boston—to the publication of *Street Corner Society*; Whyte, 1955, p. 346). Indeed, Whyte has recently faced more controversy about his research, some 50 years after the fieldwork; he has been accused, among other things, of misleading respondents about publication (Boelen, 1992). Whyte (1992) has convincingly defended himself and has been supported by some of the original participants in the research (see Orlandella, 1992; Orlandella was "Sam Franco" in *Street Corner Society*). If there has been some element of betrayal on reading or learning of the publication, then the respondents will feel that "they have been cheated and misled by someone in whom they reposed trust, and confidence" (Bulmer, 1982, p. 15). Respondents may not be fully aware at the time of the research that its findings may be published. Graduate students who speak vaguely of a dissertation may not make it clear that this is also a public document lodged in a library and open to all (Wallis, 1977, p. 159). The more "deviant" and secretive the activity, the more likely it is that subjects will fear consequences, and "the single most likely source of harm in social science inquiry is that the disclosure of private knowledge can be damaging" (Reiss, 1979, p. 73).

Trust and Betrayal

One major theme running through the ethical debate on research is that academics should not spoil the field for others. This is reflected among field-workers, where there are strong norms not to "foul the nest." But given that replications are rare in social science, that field-workers continually seek new and more esoteric settings, and that institutions frequently find one piece of research-enough, there is a general tendency to hop from topic to topic. This makes spoiling the field less problematic for prospective researchers who look elsewhere rather than follow in someone's footsteps. It may well be problematic, however, for the researched. They may be left seething with rage-and determined to skin alive the next aspiring ethnographer who seeks access. In fact, I would be curious to know how many of us have actually made it *easier* for colleagues to gain access to institutions or groups. It is already the case that anthropologists are not welcome in some Third World countries because they are associated with espionage, which is why some have turned to urban anthropology as opportunities abroad diminish. Indeed, one of the most fundamental objections to conflict methodology is that it will effectively close doors to further research.

This is particularly the case in qualitative research, compared with more formal and socially distant methods, because the academic enters into a relatively close relationship with the researched. First, in order to conduct research the field-worker has to break through to some form of social acceptance with a group. Second, full or near full involvement in the setting may bring an almost total identification with the group. This may be reinforced in deviant subcultures, where the illegal nature of the group's activities necessarily cements a close relationship, both as a necessary mechanism of entry and as a continued guarantee of collusion and of silence to outsiders. In a number of studies use was made of involvement in the role as a full participant (during employment, on vacations, as a student, or in early career employment, e.g., Becker as a jazz musician, Ditton as a bread salesman, and Van Maanen at Disney land). Indeed, the actual or pretended full commitment to the role may be essential to gaining legitimacy and acceptance from the researched. But, third, and crucially, the researcher is essentially a transient who at some stage will abandon the field and will reenter an alternative social reality that is generally far more comforting and supportive. Anderson "became" a hobo, but he did not *remain* one; in fact, he posed as one and, like many researchers, acted out a role. In the end, we leave the researched behind in the field setting, and this can lead to acute feelings of abandonment and betrayal.

For instance, I conducted research with the Amsterdam police, and over a period of six years I became increasingly conscious of the social processes involved that gradually began to contain a covert element. Elsewhere, I wrote of my growing unease as I began to see through the pretense that I shared a common experience with ordinary policemen while I became uncomfortably aware of the manipulative element in the relationships built in the field. This brought me to the notion of *mutual deceit* as virtually inherent to the deeply engaged fieldwork role:

If a latent aim of field-work is to create trust in the researcher then what was the aim of that trust? And did not the relationship involve a double betrayal: first by them of me but then by me of them? In short, I felt that in field-work the subjects are conning you until you can gain their trust

escape the realization that deceit and dissemblance were part of the research role and I did not feel ethically comfortable with that insight. Lies, deceit, concealment, and bending the truth are mentioned in many reports of field-work. Indeed, Berreman (1964: 18) states that "participant observation, as a form of social interaction, always involves impression management. Therefore, as a research technique, it inevitably entails some secrecy and some dissimulation." At the time I found this all genuinely distressing and confusing. (Punch, 1979, p. 189)

My experiences and views on the mutual conning in the field are perhaps more generally true of research involving deep and long commitment to the setting and close, if not intimate, relationships with the research subjects. And it is precisely in such research that the departure of the researcher, and the subsequent publications of his or her findings, may lead to painful feelings of abandonment and betrayal. There may also be an emotional rejection of the published portrait of the research setting and interaction. In using one's personality to enter the field situation, and in acting out a transient role, one has to face the personal and emotionally charged accusations that not infrequently accompany this style of work. This can, for instance, prove an especially painful dilemma for feminists when they feel caught between solidarity in the field and the professional need to depart and start writing up their experiences for academic consumption (see Fonow & Cook, 1991, p. 9).

Conclusion

I have endeavored here to sketch an overview of those elements that researchers need to consider in pondering the ethics and politics of qualitative research. I recognize, however, that this area is a swamp and that I have provided no map. Each individual will have to trace his or her own path. This is because there is no consensus or unanimity on what is public and private, what constitutes harm, and what the benefits of knowledge are. Also, at the individual level there is no effective control to prevent a new Laud Humphreys from employing devious methods to conduct research. Indeed, the conflict methodologists would actually encourage us to use murky means in order to expose powerful institutions (while arguing that professional ethics are "scientific suicide"; Douglas, 1979, p. 32); feminists would condemn with passion and with anger certain quently we have been enjoined to "take sides" (with such spokespersons as Becker and Goffman, when the latter was president of the American Sociological Association, arguing for an engaged and committed profession that unmasks the forces of power and oppression); for where you stand will doubtless help to determine not only what you will research but also how you will research it.

In the past, particularly in medical research and psychological experimentation, there was a considerable amount of deception and, in some cases, a demonstrable element of harm. Attempts to control this have also had an impact on social science in general. Some federally funded research in the United States, for instance, must conform to ethical standards and to auditing by review boards, and professional associations have espoused codes of conduct. A number of disparate forces, including feminism and action research, have emphasized that deceptive and/or exploitive research is inimical to treating "subjects" as partners, collaborators, and stakeholders. Feminists express solidarity with the researched, reach a highly emotive empathy with them, and are committed to emancipating the oppressed; deception and exploitation would be diametrically opposed to their ideology and methods. Here the personal is both political and ethical. In other styles of research, such as interventionist or community-based research, any attempt to dupe or mislead the researched would prove counterproductive because it would undermine the very purpose of the project. In essence, there is a strong argument, reinforced from disparate but powerful forces, that "sound ethics and sound methodology go hand in hand" (Sieber, 1992, p. 4).

Finally, it is possible to examine these issues at the societal, institutional, and professional levels. I have preferred to focus more on how certain aspects of politics and ethics impinge on the individual researcher approaching fieldwork as a relative newcomer. But that does not occur in a vacuum and, fortunately, there are available experienced and wise mentors, academic debates on moral and ethical dilemmas in the field, and professional publications and guidelines on good research practice. In general, serious academics in a sound academic community will espouse trust, reject deception, and abhor harm. They will be wary of spoiling the field, of closing doors to research, and of damaging the reputation of their profession—both as a matter of principle and out of self-interest. In practice, however, professional codes and sound advice—may not be all that clear and unambiguous in the field setting, in all its complexity and fluidity. This is because participant observation, as Ditton (1977) notes, is *inevi-*

tably unethical "by virtue of being interactionally deceitful" (p. 10). At the situational and interactional level, then, it may be unavoidable that there is a degree of impression management, manipulation, concealment, economy with the truth, and even deception. I would maintain that we have to accept much of this as being in good faith, providing the researchers come clean about their "muddy boots" (Fielding, 1982, p. 96) and "grubby hands" (Marx, 1980, p. 27). Not to do so would unduly restrict observational and qualitative studies. In essence, I echo Hughes's and Becker's summons to "simply go out and do it." But I would add that before you go you should stop and reflect on the political and ethical dimensions of what you are about to experience. Just do it by all means, but think a bit first.

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