

# INTRODUCTION TO FIELDWORK

WILLIAM B. SHAFFIR

ROBERT A. STEBBINS

Fieldwork must certainly rank with the more disagreeable activities that humanity has fashioned for itself. It is usually inconvenient, to say the least, sometimes physically uncomfortable, frequently embarrassing, and, to a degree, always tense. Although anthropology and sociology still appear to have the largest proportion of field researchers among the social sciences, their number is growing significantly in such diverse disciplines as nursing, education, management, medicine, and social work. Field researchers have in common the tendency to immerse themselves for the sake of science in situations that all but a tiny minority of humankind goes to great lengths to avoid. Consider some examples: Raymond A. Friedman (1989) said that the first contact with his research subjects in a study of labor negotiations was a disaster. The plant manager was opposed to his presence. He entered the field in the middle of a rancorous intraunion fight over seniority. And he initially chose an inopportune place (a bar) to interview workers. Carol S. Wharton (1987) managed over a ten-month period the delicate observer-as-participant role of researcher and counselor-advocate in a shelter for battered

women. Peggy Golde (1986, pp. 67-96) struggled to learn about aesthetic values and practices in a Mexican village against the odds of physical isolation, a contaminated water supply, and a new language.

For most researchers the day-to-day demands of fieldwork are fraught regularly with feelings of uncertainty and anxiety. The process of leading a way of life over an extended period that is often both novel and strange exposes the researcher to situations and experiences that usually are accompanied by an intense concern with whether the research is conducted and managed properly. Researcher fieldwork accounts typically deal with such matters as how the hurdles blocking entry were overcome successfully and how the emergent relationships with subjects were cultivated and maintained during the course of the study; the emotional pains of this work rarely are mentioned. In discussing anthropologists' fieldwork accounts, Freilich (1970) writes:

Rarely mentioned are anthropologists' anxious attempts to act appropriately when they knew little of the native culture, the emotional pressures

to act in terms of the culture of orientation, when reason and training dictated that they act in terms of the native culture, the depressing time when the project seemed destined to fail, the loneliness when communication with the natives was at a low point, and the craving for familiar sights, sounds, and faces. (p. 27)

Despite the paucity of accounts describing the less happy moments of fieldwork, such moments are likely to be present in most, if not all, field studies. This is suggested in the discussions of membership roles in field research (Adler & Adler, 1987) and field relations (Hammersley & Atkinson, 1983), and by the frequency with which they become topics of conversation among researchers. Perhaps most unhappy moments in the field are not as painful as that of Gini Graham Scott (1983) whose position as covert observer of a black magic group (Church of Hu) was discovered by other members:

At first, as I walked in, I was delighted to finally have the chance to talk to some higher-ups, but in moments the elaborate plotting that had taken place behind my back became painfully obvious.

As I sat down on the bed beside Huf, Lare looked at me icily. "What are your motives?" she hissed.

At once I became aware of the current of hostility in the room, and this sudden realization, so unexpected, left me almost speechless.

"To grow," I answered lamely. "Are you concerned about the tapes [containing research data]?"

"Well, what about them?" she snapped.

"It's so I can remember things," I said.

"And the questions? Why have you been asking everyone about their backgrounds? What does that have to do with growth?"

I tried to explain. "But I always ask people about themselves when I meet them. What's wrong with that?"

However, Lare disregarded my explanation. "We don't believe you," she said.

Then Firth butted in. "We have several people in intelligence in the group . . . we've read your diary . . ."

At this point the elaborate plotting going on behind my back became clear, and I couldn't think of anything to say. It was apparent now they considered me some kind of undercover enemy or sensationalist journalist out to harm or expose the Church, and they had gathered their evidence to

prove this. Now they were trying the case, though it was obvious the decision had already been made. Later, Armat explained that they had fears about me or anyone else drawing attention to them because of the negative climate towards cults among "humans." So they were afraid that any outside attention might lead to the destruction of the Church before they could prepare for the coming annihilation. However, in the tense setting of a quickly convened trial, there was no way to explain my intentions or try to reconcile them with my expressed belief in learning magic. Once Firth said he read my diary, I realized there was nothing more to say.

"So now, get out," Lare snapped. "Take off your pentagram and get out."

As I removed it from my chain, I explained that I had driven up with several other people and had no way back.

"That's your problem," she said. "Just be gone by the time we get back." Then threateningly she added: "You should be glad that we aren't going to do anything else."

"There are buses," Huf remarked. (pp. 132-133)

Nonetheless, after hearing field researchers discuss their work, one has to conclude that there are exceptional payoffs that justify the accompanying hardships. And for these scientists, the payoffs go beyond the lengthy research reports that present sets of inductive generalizations based on direct contact with other ways of life. Fieldwork, its rigors notwithstanding, offers many rewarding personal experiences. Among them are the often warm relations to be had with subjects and the challenges of understanding a new culture and overcoming anxieties. In short, entering the research setting, learning the ropes once in, maintaining working relations with subjects, and making a smooth exit are difficult to achieve and sources of pride when done well.

From another perspective, the desire to do fieldwork is founded on motives that drive few other kinds of scientific investigation. To be sure, field researchers share with other scientists the goal of collecting valid, impartial data about some natural phenomenon. In addition, however, they gain satisfaction—perhaps better stated as a sense of accomplishment—from successfully managing the social side of their projects, which is more problematic than in any other form of inquiry. Though they raise

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questions of validity (Johnson, 1975, p. 161), gratifying relations between observers and subjects frequently emerge in the field. At the same time, being accepted by subjects as a group is crucial for conduct of the study (Cicourel, 1964, p. 42). Observers must be able to convince their subjects (and sometimes their professional colleagues) that they can satisfactorily do the research and that their interests are of enough importance to offset the frequent inconvenience, embarrassment, annoyance, and exposure that necessarily accompany unbiased scientific scrutiny of any group. It is in attempting to solve this basic problem, which recurs throughout every study, that many of the unforgettable experiences of fieldwork occur.

Completion of a fieldwork project is also an accomplishment because the “situation of social scientists” (Lofland, 1976, pp. 13-18) discourages it so. Lofland notes that many social scientists are unsuited temperamentally for the stressful activity of such an undertaking because they are rather asocial, reclusive, and sometimes even abrasive. Furthermore, all university-based research must be molded to the demands of the university as a large-scale organization. Getting acquainted with an essentially foreign way of life is complicated further when the research is pursued intermittently after classes, before committee meetings, between deadlines, and the like (e.g., Shaffir, Marshall, & Haas, 1980). To this is added the lack of procedural clarity that characterizes field research; there are few useful rules (unlike other forms of social research) available for transforming chaotic sets of observations into systematic generalizations about a way of life. Then there is the preference of funding agencies for quantitative investigations, which pushes the field-worker yet another step in the direction of marginality. Finally, the very style of reporting social scientific findings, abstruse and arcane as it tends to be, contrasts badly with the down-to-earth routines of the people under study. When they find it next to impossible to see themselves in the reports of the projects in which they have participated, they are rendered ineffective as critics of the accuracy of the research.

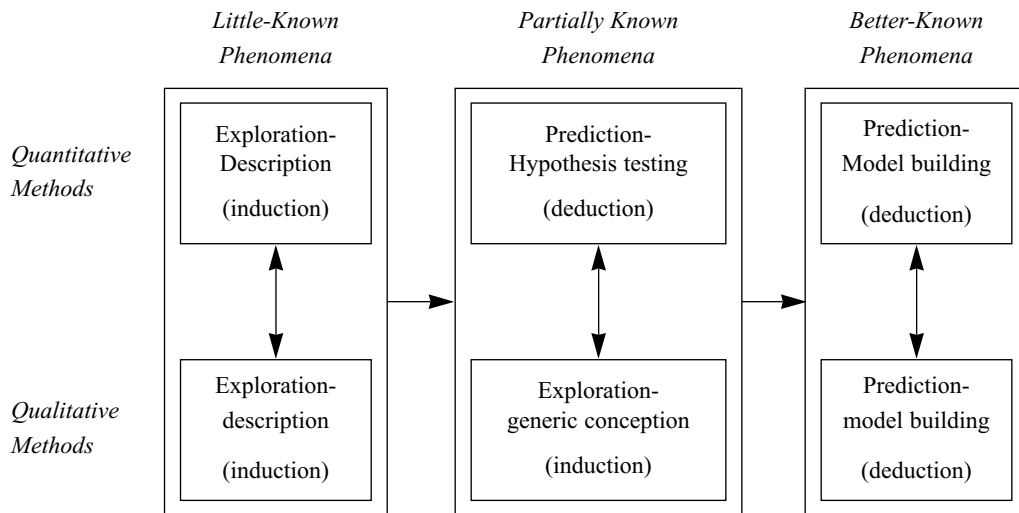
Fieldwork is carried out by immersing oneself in a collective way of life for the purpose of gaining firsthand knowledge about a major facet

of it. As Blumer (1986) puts it, field research on another way of life consists of:

getting closer to the people involved in it, seeing it in a variety of situations they meet, noting their problems and observing how they handle them, being party to their conversations and watching their way of life as it flows along. (p. 37)

Adopting mainly the methodology of participant observation—described as “research that involves social interaction between the researcher and informants in the milieu of the latter, during which data are systematically and unobtrusively collected” (Taylor & Bogdan, 1984, p. 15)—the researcher attempts to record the ongoing experiences of those observed in their symbolic world. This research strategy commits the observer to learning to define the world from the perspective of those studied and requires that he or she gain as intimate an understanding as possible of their way of perceiving life. To achieve this aim, the field researcher typically supplements participant observation with additional methodological techniques in field research, often including semistructured interviews, life histories, document analysis, and various nonreactive measures (Webb, Campbell, Schwartz, Sechrest, & Grove, 1981).

Most fieldwork projects are exploratory, this means that the researcher approaches the field with certain special orientations, among them *flexibility* in looking for data and *open-mindedness* about where to find them. These are needed to explore the phenomenon under study when relatively little is known about it. Following Max Weber’s model, the first step to be taken in the scientific study of social life is to acquire an intimate, firsthand understanding (*Verstehen*) of the human acts being observed. It follows that the most efficient approach is to search for this understanding wherever it may be found by any method that appears to bear fruit. The main goal of exploratory research is the generation of inductively obtained generalizations about the field. These generalizations are eventually woven into a “grounded theory” of the phenomenon under consideration, the procedure for which is found in a series of publications by Glaser (1978), Glaser and Strauss (1967), and Strauss (1987).



**Figure A.1** The Relationship of Qualitative and Quantitative Methods

The left side of Figure A.1 indicates that both quantitative and qualitative data may be gathered during exploration. Most exploratory studies, however, are predominantly qualitative, possibly augmented in a minor quantitative way by such descriptive statistics as indexes, percentages, and enumerations. As we come to know better the phenomenon we have chosen for examination, we move to the right across Figure A.1. That is, we come to rely less and less on exploration and more and more on prediction along the lines of hypotheses obtained deductively from the grounded theory. This process typically unfolds over the course of several studies. It is paralleled by an expansion of the grounded theory through its application to an ever-wider range of phenomena and through its internal development of an ever-growing number of generic concepts (Purs, 1987).

On the far right side of Figure A.1, we find a well-developed grounded theory about a reasonably known, broad range of related phenomena. At this point concern is chiefly with enhancing the precision of the theory, a process that is pursued commonly through prediction and quantification including a heavy reliance on inferential statistics. Even here, however, qualitative data can sometimes play a role. Such data, for example, may help confirm certain hypotheses or bring to our attention through exploration

important recent changes in social process and structure that the narrower focus of confirmation of hypotheses has led us to overlook.

Although descriptions and analyses of the various dimensions of the field research experience have become more plentiful recently, it is unfortunate that the social aspects both underlying and shaping this experience have received so little critical attention. These social aspects, which include feelings of self-doubt, uncertainty, and frustration, are both inherent in field research and the basic stuff of which this methodology consists.

Field research is accompanied by a set of experiences that are, for the most part, unavailable through other forms of social scientific research. These experiences are bound together with satisfactions, embarrassments, challenges, pains, triumphs, ambiguities, and agonies, all of which blend into what has been described as the field research adventure (Glazer, 1972). It is difficult to imagine a field project that does not include at least some of these features, however skilled and experienced the researcher. Anyone undertaking field study for the first time—usually an undergraduate or a graduate student—encounters a mix of these feelings but, unlike the seasoned investigator, blames him- or herself for the problem, seemingly a result solely of inadequate preparation and experience.

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### HISTORY OF FIELDWORK

The history of fieldwork as a set of research techniques, an approach to data collection, can be related, in good measure, to the issues of validity and reliability, ethics, and the study of the unfamiliar. In grappling with them, fieldwork procedure has gained in distinctiveness and respectability, and its place in the scientific process has been clarified.

Rosalie Wax (1971), who has written one of the most extensive histories of fieldwork, points out that descriptive reporting of the customs, inclinations, and accomplishments of other societies goes back almost to the origin of writing. From the Roman period on, travelers have been so fascinated with the cultural differences they experienced that they have recorded their observations as a matter of interest to themselves and their compatriots. And as world passage became easier in the late nineteenth century, so the accounts of “backward” peoples multiplied. Some of these “amateur” reports, as contemporary anthropologists refer to them, are accurate enough to be of scientific value.

While the travelers busied themselves writing biased accounts of foreigners, educated men and sometimes women (e.g., lawyers, physicians, physical scientists, administrative officials) were gathering firsthand information on certain sections of their own societies with which they were unacquainted initially. In the early twentieth century Charles Booth, for example, combined statistical data with extensive interviewing and participant observation to complete a vast study of the working people of London. In fact, several investigations were conducted in the late nineteenth and early twentieth centuries in England, France, and Germany that used participant observation and interviews (sometimes supplemented by questionnaires) to produce data.

Bronislaw Malinowski (1922) was perhaps the first social scientist to live in a preliterate community for an extended period and to record as objectively as possible what he saw. His intimate involvement in the daily lives of his subjects is regarded as a turning point in the history of fieldwork procedure. Malinowski also wrote detailed descriptions of how he gathered his data.

Several decades later, sociologist Robert Park (a former journalist) and anthropologist Robert Redfield turned the University of Chicago into a center for participant observer-based fieldwork that was without parallel anywhere in the world. During the 1920s, the first generation of sociologists here concentrated on subjects such as the hobo, the ghetto, the neighborhood, and the gang. Succeeding generations of students and faculty examined, among others, French Canada, an Italian slum, juvenile delinquents, and governmental agencies (Blau, 1955), a mental hospital (Goffman, 1961), and medical students (Becker, Geer, Hughes, & Strauss, 1961). Unlike Malinowski, however, those who came under the influence of Park and Redfield at Chicago were expected to integrate their field data with the ideas of Weber, Simmel, Dewey, and other prominent social theorists of the day. Furthermore, the fieldwork of this period maintained the stance of scientific objectivity, which started with the colonial period of world history and the dominance of natural science methods in world intellectual circles. As Barnes (1963) put it:

The ethnographer took for granted that the observations and records he made did not significantly disturb the behavior of the people studied. In the classical mechanics of the nineteenth century it was assumed that physical observations could be made without affecting the objects observed and in much the same way ethnographers assumed that in their researches there was no direct feedback from them to their informants. (p. 120)

Barnes goes on to state that times have changed. Modern field research is apt to deal with literate people who can read the researcher's reports, write letters to influential authorities, and perhaps sue. In response to this threat and others, ethics committees have emerged in many universities, where they assess proposed social research for its possible unfavorable impact on subjects. And modern field research frequently centers on topics within the investigator's society. Thus the possibility must be faced that some subjects, when in the researcher's presence, will not be candid in their behavior and conversations for fear that their actions and statements, which may be unacceptable to certain people, will become available to those persons.

“There may still be an exotic focus of study, but the group or institution being studied is now seen to be embedded in a network of social relations of which the observer is an integral if reluctant part” (Barnes, 1963, p. 121).

Meanwhile, certain trends have forced field researchers to clarify their position in the scientific process. The use of questionnaires gained widespread acceptance through the rising popularity of public-opinion polling and its close association with the ideas of Paul Lazarsfeld and Robert Merton (R. Wax, 1971, p. 40). Rigorous research designs, quantitative data, statistical techniques, and at first mechanical and later electronic information processing became hallmarks of social scientific procedure, while fieldwork was regarded more and more as an old-fashioned and “softheaded” approach.

In other words, whereas one of the widely acknowledged strengths of fieldwork has been its potential for generating seminal ideas, its capacity for effectively testing these ideas has been increasingly questioned. Field researchers helped to confuse its role by claiming Znaniecki’s (1934) method of “analytic induction” as a model of their procedure. In analytic induction, hypotheses are generated not only from raw data but also are tested by them. A lively debate sprang up some time later over the logical possibility of conducting both operations simultaneously (based on the same data) and over the general utility of analytic induction (see Robinson, 1951; Turner, 1953).

More than 30 years elapsed after the publication of Znaniecki’s book before social scientists sorted out the place of inductive hypothesis-generating procedure in the broader scientific process. Glaser and Strauss (1967) firmly established that no procedure can concurrently generate and test propositions. The first requires flexibility, unstructured research techniques, intuition, and detailed description; the second uses control, structured techniques, precisions, and logical movement from premises to conclusions. The “constant comparative method,” as Glaser and Strauss describe the approach, is an effective means for generating hypotheses and grounded theory.

We finally have learned that as our knowledge about an area grows—as hypotheses grounded in direct field study begin to coalesce

into a theory about it—fieldwork and its less structured techniques fade into the background. At the same time, more controlled techniques come into use (see Figure A.1). Today, some social science fields are largely or significantly exploratory in their procedural orientation, among them anthropology, community psychology, symbolic interactionism, and classroom studies.

Others, such as small group research and family sociology, generally appear to have passed beyond this stage.

Although initial exploration has been the chief role of qualitative research, we are not claiming that in its exploratory stage it is always “preliminary” in its import. Exploratory studies have been so effective as to have become classics—for example, Whyte’s (1943/1981) *StreetCorner Society* or Goffman’s (1959) *The Presentation of Self in Everyday Life*. In these instances and others, social science has subsequently learned through more controlled study that the initial observations were empirically sound and theoretically significant. Glaser and Strauss (1967, pp. 234-235) advance three reasons why exploratory investigations often turn out to be the final research on a particular topic. First, the qualitative findings are taken by social scientists to be definitive. Second, interest wanes in conducting further research on the phenomenon. Third, before researchers can mount more rigorous studies of it, the phenomenon has changed considerably.

A parallel history of growing self-consciousness about the special status of fieldwork is evident in two ways: in the organizational developments that have come to surround it, and in its literature. Every year since 1984, McMaster University, the University of Waterloo, and more recently York University have sponsored conferences on qualitative methods and research projects. Although more specialized, the annual University of Pennsylvania Ethnography in Education Research Forum has been around even longer (since 1980). The Qualitative Interest Group (QUIG) at the University of Georgia offered in early 1990 its third international conference on qualitative research in education. Educationists were also instrumental in starting in 1987 in Quebec the francophone *Association pour la recherche qualitative*;

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however, it embraces qualitative research in all fields. It is possibly the first association devoted exclusively to qualitative research.

Concerning self-consciousness about the special status of fieldwork as seen in the progression of its literature, Junker (1960, p. 160) notes that in the first two decades of this century the publications of field researchers contained little about their problems and experiences. He traces the tendency to discuss these matters in the final report, however briefly, to Robert and Helen Lynd's study of Middletown. But it was not until after 1940 that theoretical treatments of fieldwork issues and experiences began to occur with regularity. Even in 1960, Junker could write that "full accounts are still rare, considering the large number of field studies published and still coming off the presses" (pp 160-161).

Since then, however, candid descriptions of fieldwork that frequently touch on the issues of validity and ethics have become routine in monographic reports. Several texts and readers devoted exclusively to this method also have been produced. Some of the most recent of these have been written or edited by Berg (1989), Emerson (1983), Hammersley and Atkinson (1983), Lofland and Lofland (1984), and Taylor and Bogdan (1984). Since 1986 Sage Publications has published the *Qualitative Research Methods* series of short introductions to the methodological tools of qualitative studies, and since 1977 the *Sociological Observations* series of monographs. Approximately two decades ago John Lofland (1971, p. 131) urged researchers to present detailed accounts of the social relations and private feelings experienced in the field. Also of note are the two main journals for qualitative research, the *Journal of Contemporary Ethnography* (formerly *Urban Life and Culture* and then *Urban Life*), founded in 1972, and *Qualitative Sociology*, founded in 1978. Nonetheless, in sociology and especially anthropology, articles on qualitative methods and research projects frequently appear in many of the general and specialty journals.

### ISSUES IN FIELDWORK

Besides acquainting unseasoned field researchers with the nature of the experiences that

await them, there are three reasons why a collection of accounts is valuable. They are seen in the three issues traditionally considered in methodological discussions of the field approach that are affected by the nature of the researcher's experiences in gathering data: validity and reliability, ethics, and the study of the unfamiliar. Because field researchers are virtually part of the data-collection process, rather than its external directors, their experiences there are critical.

### Validity and Reliability

The problem of validity in field research concerns the difficulty of gaining an accurate or true impression of the phenomenon under study. The companion problem of reliability centers on the replicability of observations; it rests on the question of whether another researcher with similar methodological training, understanding of the field setting, and rapport with the subjects can make similar observations. In field research these two problems fall into the following categories:

- (1) reactive effects of the observer's presence or activities on the phenomena being observed;
- (2) distorting effects of selective perception and interpretation on the observer's part; and
- (3) limitations on the observer's ability to witness all relevant aspects of the phenomena in question (McCall & Simmons, 1969, p. 78).

The experiences of the researcher bear on all three.

Reactive effects are the special behavioral responses subjects make because the observer is in the setting, responses that are atypical for the occasion. Webb and his colleagues (1981, pp. 49-58) treat four reactive effects that frequently invalidate social science data, two of which—the guinea pig effect and role selection—are germane here. In the first, subjects are aware of being observed and react by putting their best foot forward; they strive to make a good impression. In the second, which is closely related, they choose to emphasize one of several selves that they sense is most appropriate given the observers presence.

Special reactive effects may take place when a researcher's rational appearances fail (Johnson, 1975, pp. 155-160). Observers are human, too. At times they get angry, become sympathetic, grow despondent, and are unable to hide these sentiments. Jean Briggs's (1986, pp. 34-38) indignation at the Kapluna (white) fisherman who damaged a canoe owned by the Eskimos she was studying resulted in subtle ostracism that lasted three months. The validity of the data is certainly jeopardized, in some measure, by such outbursts. Perhaps, too, maintaining rationality when emotion is conventionally expected produces the image of a researcher who is callous or lifeless and should be treated accordingly.

Another condition under which reactive effects could blemish the quality of data is the disintegration of trust between the observer and one or more subjects. Once the researcher has found or been placed in a role in the group being studied, whether that of observer or something more familiar to its members, a certain degree of trust develops whereby he or she is allowed to participate in many of their affairs. Behavior of the observer that contradicts the belief that he or she belongs in that role may break the bond of trust. Still more unfortunate, it may suddenly come to light that the observer is just that, an observer, rather than a pure member—one of the chief hazards in operating as a concealed researcher. This possibility haunted Kathryn Fox (1987) in her study of punk rockers. She knew that if her identity as a researcher became generally known, she would effectively be denied access to the local punk scene. This situation severely limited her participation there.

Observers also selectively perceive and interpret data in different ways that ultimately and sometimes seriously bias their investigations. The most celebrated of these is "going native," or so thoroughly embracing the customs and beliefs of the focal group that one becomes incapable of objective work. Certain other problems bear mention as well. Johnson (1975, pp. 151-155) points out that the observer's apprehension that commonly attends entry to the field can slant the perception of events during his or her early days there. Moreover, our commonsense assumptions about life are disturbed during these initial weeks, causing us to

see things that other people, with different presuppositions about everyday affairs, would miss.

Special orientations towards subjects, whether love, hate, friendship, admiration, respect, or dislike, also influence our views of these people and their behavior. One wonders what consequences the close bond between Doc and William F. Whyte (1943/1981) or Tally and Elliot Liebow (1967) had for the observers' perceptions of these informants and the informants' social involvements. Undoubtedly there were both advantages and disadvantages to these arrangements. Of equal importance are the desires felt by many field researchers to help their subjects in some way. They face a profound dilemma, however, when the need for help is so great that it threatens the study itself by monopolizing the time the researcher has to commit to the latter (Lofland & Lofland, 1984, p. 34).

The third category of validity and reliability problems deals with limitations on the observer's ability to witness all that is relevant to the study. For example, Richard V. Ericson (1981, pp. 25-26) found in his study of detective work that it appeared on certain occasions that cases were assigned intentionally to another team of detectives to which there was no field researcher attached. On other occasions the detective sergeant responsible for case assignments would give a case to a team with a researcher because he believed the case would be interesting to the researcher.

Taylor and Bogdan (1984, pp. 44-45) discuss a related fieldwork predicament that can cut off researchers from events of great importance to them. Observers, having become established as individuals who are knowledgeable about the local scene, may be called upon to mediate conflict or advise on how to solve a problem. Those who follow this lead and try to help may find themselves alienated from members of the group whose lives were affected unfavorably by or who were opposed to the suggested solution.

The status of the researcher may engender still another type of observational limitation. One thorny status problem, prevalent in anthropology and sociology, is the exclusiveness of sex. Being male, for instance, tends to bar one from direct observation of female activities, as Shaffir (1974, p. 43) discovered while studying



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the Lubavitcher Chassidim of Montreal. He was forced to accept different orders of data for males and females in that community. Rosalie Wax (1971, p. 46) describes the exclusiveness of certain age and sex categories she encountered as a woman scientist who has worked in several different groups.

All research is subject to the problem of reliability and validity. Although they are common to the experiment and the survey, they also are found in fieldwork. The critics of fieldwork lack confidence in the analysis of and conclusions drawn from field data. This sentiment hinges on their belief that the undisciplined procedures of fieldwork enable researchers, to a greater degree than practitioners of other methodologies, to influence the very situations they are studying, thereby flagrantly violating the canons of scientific objectivity.

A common feature of all social science research is the subjects' response to the "demand characteristics" of the investigation (Orne, 1962; Rosenthal, 1970; Sherman, 1967).

Researchers in the social sciences are faced with a unique methodological problem: the very conditions of their research constitute an important complex variable for what passes as the findings of their investigations. . . . The activities of the investigator play a crucial role in the data obtained. (Cicourel, 1964, p. 39)

In fieldwork, researchers' very attempts to establish rapport with the people they are studying may be achieved at the expense of a degree of accuracy as to how they normally behave or present themselves in the situations being observed. Indeed, Jack Douglas (1976) argues, for these reasons and others, that traditional methods of field research must be discarded in favor of more penetrating or "investigative" procedures that permit access to these private spheres of life. As Becker (1970, pp. 39-62) so persuasively argues, however, in contrast to the more controlled methods of laboratory experiment and survey interviews, fieldwork is least likely to permit researchers to bias results to correspond with their expectations.

First, the people the field worker observes are ordinarily constrained to act as they would have in

his absence, by the very social constraints whose effects interest him; he therefore has little chance, compared to practitioners of other methods, to influence what they do, for more potent forces are operating. Second, the field worker inevitably, by his continuous presence, gathers much more data and . . . makes and can make many more tests of his hypotheses than researchers who use more formal methods. (pp. 43-44)

One way to attack the validity problem is to play back one's observations to one's subjects in either verbal or written form. From the perspective of the experience of fieldwork, this practice tends to enhance rapport with subjects (assuming the observations are of interest to them and of little threat) by casting them in the roles of local experts and helpful participants in the research project. "Member validation" is one of several contributions that informants can make.

### Ethical Issues

Only part of the seemingly endless list of ethical issues that plague social scientists is germane to the social experience of fieldwork. These issues are of three kinds: ethics of concealment, changes in research interests, and violations of the researcher's moral code. The oft-discussed questions of what to write about the group under study, how to protect confidentiality against legal proceedings, and the like are of greatest concern after leaving the field. They appear to play no significant role in the actual collection of data.

Of the three kinds, the ethics of concealment has been examined most thoroughly in the professional literature. It has several facets, one being the issue of the covert observer, or social scientific investigator whose professional aims are unknown to the subjects. They take the individual for someone else, usually one of them. The majority of writers on this topic oppose concealment (e.g., Davis, 1961; Erikson, 1965; Gold, 1958, pp. 221-222), though some scholars argue the contrary (e.g., Douglas, 1976). One way in which this issue can affect the actual experience of fieldwork is recounted by Wallis (1977), who secretly observed a Scientology group:

At the Scientology lodging house the problem was equally difficult. The other residents with whom I dined and breakfasted were committed Scientologists and in a friendly way sought to draw me into their conversations. I found it difficult to participate without suggesting a commitment similar to their own, which I did not feel. Returning to the course material, I found as I progressed that I would shortly have to convey—either aloud or by my continued presence—assent to claims made by Ron Hubbard, the movement's founder, with which I could not agree and of which I could sometimes make little sense. (p. 155)

Even where the observer's true role is known, ethical considerations may arise when there is concealment of certain aspects of the project. Occasionally, the project's very aims are kept secret; Reece and Siegal (1986, pp. 104-108) present a fictive case along these lines and then explore its ethical implications. Or concealment may be more subtle, but no less questionable in the eyes of some scientists, when data are gathered by means of a hidden tape recorder, inadvertently overheard remarks, or intentional eavesdropping. These clandestine methods add tension to the conduct of fieldwork because there is always the risk that what is hidden will somehow be uncovered. Finally, individual researchers may question the propriety of concealing opinions that are diametrically opposed to those held by their subjects. Howard Newby (1977, p. 118), for example, suffered pangs of conscience of this sort when as a liberal university student he interviewed conservative farm workers about their political attitudes. On a related theme, Yablonsky (1965, p. 72) sees the researcher's failure to moralize, when accompanied by an intense interest in deviant life styles, as subtle encouragement of the subject's aberrant behavior.

Changes in research interests are inevitable in field study, where one of the central aims is to discover data capable of generating original theory. Johnson (1975, p. 58) notes how investigators may gain entry to a field setting by stating a particular set of interests, only to find themselves in a moral dilemma because their observations have spawned new interests, ones that the sponsors had no opportunity to consider. To make things worse, these new interests may touch on sensitive matters, where formal

permission for observation or interviewing might never be granted.

Then there is the question of the ethical conflict suffered by field researchers who feel compelled to engage in, or at least witness, illegal activities. Should such activities be reported? The agency whose permission has made the research possible may expect this will be done. Should researchers try to maintain or strengthen rapport with their subjects by participating in unlawful events when invited to do so? Polsky (1967/1985, p. 127), who systematically observed criminals in their natural habitat, says that this is up to the individual investigator but that, in the study of criminals anyway, one should make clear what one is prepared to do and see and not to do and see. William Whyte (1943/1981, pp. 313-317) got caught in the ethical dilemma of whether to vote in place of another man in a mayoralty election.

Although all field researchers are faced with ethical decisions in the course of their work, a review of the fieldwork literature reveals that there is no consensus concerning the researcher's duties and responsibilities either to those studied or to the discipline itself. As Roth (1960) has so aptly argued, the controversy between "secret research" and "nonsecret research" is largely misguided, for all research is secret in some way. A more profitable line of investigation is to focus on "how much secrecy shall there be with which people in which circumstances" (p. 283). Most field researchers hold that some measure of responsibility is owed the people under study, though the extent of this conviction and its application are left to each researcher's conscience.

### Studying the Unfamiliar

Dealing with the unfamiliar is bound to produce at least some formative experiences. In field investigations the use of unstructured procedures, the pursuit of new propositions, and the participation in strange (for the scientist) activities are the stuff of which memorable involvements are made.

Unlike controlled studies, such as surveys and experiments, field studies avoid prejudice of the nature of the problem and hence the use of rigid data-gathering devices and hypotheses based on a priori beliefs or hunches concerning

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the research setting and its participants. Rather, their mission is typically the discovery of new propositions that must be tested more rigorously in subsequent research specially designed for this purpose. Hence field researchers always live, to some extent, with the disquieting notion that they are gathering the wrong data (e.g., Gans, 1968, p. 312), that they should be observing or asking questions about another event or practice instead of the present one. Or they are bewildered by the complexity of the field setting and therefore are unable to identify significant dimensions and categories that can serve to channel their observations and questions. These feelings of uncertainty that accompany open-ended investigation tend, however, to diminish as the investigator grows more familiar with the group under study and those of its activities that bear on the research focus.

Blanche Geer (1964) describes the confusion of her first days in the field as she set out to examine the different perspectives on academic work held by a sample of undergraduates:

Our proposal seems forgotten. Of course, there were not enough premedical students at the previews (summer orientation for freshmen) for me to concentrate on them. To limit myself to our broader objective, the liberal-arts college student, was difficult. The previewers did not group themselves according to the school or college of the University they planned to enter. Out of ordinary politeness . . . I found myself talking to prefreshmen planning careers in engineering, pharmacy, business, and fine arts, as well as the liberal arts. Perhaps it is impossible to stick to a narrow objective in the field. If, as will always be the case, there are unanticipated data at hand, the field worker will broaden his operations to get them. Perhaps he includes such data because they will help him to understand his planned objectives, but he may very well go after them simply because, like the mountain, they are there. (p. 327)

The requirement that a researcher discover something only increases the anxiety of fieldwork. Nowhere does originality come easily. The field experience does offer a kaleidoscope of contrasts between the observer's routine world and that of the subjects. New patterns of thought and action flash before the observer's eyes. But the question is always: Are these of

any importance for science? Researchers note these contrasts and often virtually everything else they perceive. Nevertheless, the ultimate goal is conceptual. One strives to organize these novel perceptions into a grounded or inductive theory, which is done to some extent while still in the field. Some of the generalizations that constitute the emerging theory are born from on-the-spot flashes of insight. Herein lies the art of science. And these insightful moments can be among the most exciting of the fieldwork experience, while their absence can be exasperating, if not discouraging.

As if unstructured research procedure and obligatory discovery were not enough, field investigators must also be ready to cope with unfamiliar events. Furthermore, they must learn new ways of behaving and possibly new skills, the mastery of which are crucial for success in their projects. Powdermaker (1968) describes this problem for the field anthropologist:

During the first month or so the field worker proceeds very slowly, making use of all his sensory impressions and intuitions. He walks warily and attempts to learn as quickly as possible the most important forms of native etiquette and taboos. When in doubt he falls back on his own sense of politeness and sensitivity to the feelings of others. He likewise has to cope with his own emotional problems, for he often experiences anxieties in a strange situation. He may be overwhelmed by the difficulties of really getting "inside" an alien culture and of learning an unrecorded or other strange language. He may wonder whether he should intrude into the privacy of people's lives by asking them questions. Field workers vary in their degree of shyness, but most people of any sensitivity experience some feelings of this type when they first enter a new field situation. (p. 419)

### MARGINALITY

If there is one especially well-suited adjective that describes the social experiences of fieldwork, it is *marginality*. Field researchers and their activities are marginal in several ways. For one, field research—because of its emphasis on direct human contact, subjective understanding of others' motives and wants, and broad

participation in their daily affairs—is closer to the humanities than most forms of social scientific research. Although structured data collection is now the most traveled methodological route in many social sciences, field researchers are riding off in a different direction. For this they are scorned by many positivists, who see field research as a weak science. From the humanists' perspective, however, it is still too scientific, owing to its concern with validity, testable hypotheses, replicability, and the like. The public, who might be expected to take a neutral stand in this intellectual debate, sometimes appears to have embraced the stereotype that good social science is characterized only by quantitative rigor. Thus it occasionally happens that field researchers also have to convince even their subjects and sponsors that theirs is a legitimate approach for the problem at hand.

Yet the field researcher who is identified as an atypical social scientist has an advantage. For here is a scientist who is viewed by group members as interested enough in them and their activities to maintain extensive direct contact instead of relying solely or chiefly on such substitutes as questionnaires and measurement scales. Many subjects appreciate this special effort. They seem to know that their lives are too complicated to be studied accurately and adequately by structured means alone. By the same token, it is to be expected that field researchers will make some subjects uneasy by their tendency to plumb the group's dark secrets.

Field researchers are also marginal in their own professions (except in anthropology). Recall Lofland's (1976, p. 13) observation that many social scientists are unsuited for engaging in field study. Being rather asocial, reclusive, and occasionally abrasive, they would fail to gain entrance to the setting to be examined or, if they somehow succeeded, they would fail to maintain the level of rapport upon which good field research depends. Social scientists who have the requisite interpersonal skills to do fieldwork are a minority; providing they discover their talents, they find occupational fulfillment in ways most of their colleagues see as strange or exotic.

Once in the field, all participant observers, if they are known as such to their subjects, are more or less marginal to their subjects' world.

The former never quite belong, especially while the research is getting under way—a fact that is made amply clear time and again. As Hughes (1960) argues, even though the sociologist might report observations made as a member of the group under study, “the member becomes something of a stranger in the very act of objectifying and reporting his experiences” (p. ix). In a similar vein, Freilich (1970), writing about anthropologists, cautions against the common desire among researchers to become a native:

Irrespective of what role he assumes, the anthropologist remains a *marginal man* in the community, an outsider. No matter how skilled he is in the native tongue, how nimble in handling strange social relationships, how artistic in performing social and religious rituals, and how attached he is to local beliefs, goals, and values, the anthropologist rarely deludes himself into thinking that many community members really regard him as one of them. (p. 2)

From the researcher's standpoint there are humbling experiences inherent in this kind of marginality. Lofland (1976, p. 14) points out that one must admit to laymen that one is ignorant, though willing to learn. Such an admission is incongruent with the self-image of savant, of dignified university professor, of learned Ph.D. As a field researcher, one is a mere student in need of particular instruction or general socialization.

What is worse, subjects have been known to take advantage of this situation and put on or mislead the observer about aspects of their lives of interest to the study (e.g., Visano, 1987, p. 67). Field researchers must be alert to such deception but be prepared to take it in good stride. Nonetheless, being the object of a put-on, while it adds zest to the subjects' routine, frequently is embarrassing to the “mark” (Stebbins, 1975).

And fieldwork, even when conducted in the researcher's own community, has been known to become all-absorbing, leaving time only for absolutely mandatory family and work activities. Marginality is the best characterization of the committed social scientist, who spends practically every waking minute riding with police or observing juvenile gangs. As with

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professionals in any occupation, the line between work and leisure is sometimes erased for field researchers, which casts them in a strange light when viewed from the perspective of a leisure-oriented society.

On balance, marginality, despite its drawbacks, seems to breed a peculiar strain of motivation among committed field researchers. Being atypical in one's profession, to the extent that such a condition is free of stigma, has the potential appeal of salutary visibility, of being commendably different. Field researchers have stories to tell about their data-collection exploits that enchant students and colleagues, most of whom have no such accounts to swap. Field researchers have "been around" in a way seldom matched by the run-of-the-mill social scientist. They have gained in personal sophistication through contact with other cultures and lifestyles and through solving thorny interpersonal problems in the course of completing their projects.

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## CONCLUSION

As most field researchers would admit, the so-called rules and canons of fieldwork frequently are bent and twisted to accommodate the particular demands and requirements of the fieldwork situation and the personal characteristics of the researcher. The following observations reflect this view clearly and accurately:

As every researcher knows, there is more to doing research than is dreamt of in philosophies of science, and texts in methodology offer answers to only a fraction of the problems one encounters. The best laid research plans run up against unforeseen contingencies in the collection and analysis of data; the data one collects may prove to have little to do with the hypotheses one sets out to test; unexpected findings inspire new ideas. No matter how carefully one plans in advance, research is designed in the course of its execution. The finished monograph is the result of hundreds of decisions, large and small, made while the research is underway and our standard texts do not give us procedures and techniques for making these decisions. . . . I must take issue with one point . . . that social research being what it is, we can never escape the necessity to improvise, the surprise of

the unexpected, our dependence on inspiration. . . . It is possible, after all, to reflect on one's difficulties and inspirations and see how they could be handled more rationally the next time around. In short, one can be methodical about matters that earlier had been left to chance and improvisation and thus cut down the area of guess work. (Becker, 1965a, pp. 602-603)

The sociological enterprise of theory and research has been presented as an idealized process, immaculately conceived in design and elegantly executed in practice. My discussions of theory, measurement, instrumentation, sampling strategies, resolutions of issues of validity, and the generation of valid causal propositions by various methods proceeded on an assumption. This assumption was that once the proper rules were learned, adequate theory would be forthcoming. Unfortunately, of course, this is seldom the case. Each theorist or methodologist takes rules of method and inference and molds them to fit her particular problem—and personality. (Denzin, 1989, p. 249)

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