

Editor's Note: *This paper, The Other Side of the Looking Glass: Problems Encountered in Fieldwork examines some of the difficulties that are likely to be encountered in participant observation research, and presents strategies for handling them. The general position of this paper and the specific examples presented grew out of a research study conducted by the author of the elderly tenants of a slum hotel in a large midwestern city.*

## INTRODUCTION

The current revival of interest in participant observational approaches to data collection and analysis has engendered, as an additional bonus, a growing concern with some of the problems attendant to this methodological orientation.<sup>1</sup> The literature contains several excellent treatments of this subject; we are indebted to such men and women as Robert Redfield (1941; 1948), William H. Whyte (1943; 1951; 1964), and Rosalie H. Wax (1957; 1960), to name but a few, who have told us in articles and books of their experiences in the field, of the difficulties they encountered, of the solutions they discovered.

While acknowledging the contributions that have aided us in our understanding and practicing of the process of fieldwork, we must concede that in general fieldworkers have been remiss in reporting back to us the contingencies of their fieldwork experience. We are confronted, as a consequence, with an impoverishment of systematic data on the difficulties that are likely to be encountered in this kind of research enterprise. Particularly, we lack formal knowledge as to preferable ways of dealing with recurrent barriers to the fulfillment of the research goals of participant observers.

That this omission is pronounced is related to the greater flexibility of fieldwork; participant observation techniques are characterized by their lack of rigidity, their emergent quality, and their responsiveness to in-coming data. Herbert Blumer (1969:1-60) has pointed out the compatibility of fieldwork with the conducting of "naturalistic" research. Unfortunately, the open-ended character of participant observation has sometimes seduced its practitioners into a belief in what we recognize as the

fieldwork "mystique", which includes such tenets as fieldwork is an "art", a unique experience not sharable with others, etc. Such beliefs wreak havoc with the research goals of replicability and generalization, and have contributed to the disrepute with which a sizable number of social scientists view such examples of "soft" science.

Those, on the other hand, interested in defending participant observation studies are correct in pointing out that survey research with its reliance upon the verbal report, differs substantially from fieldwork not in terms of greater precision or a higher degree of standardization, but rather by virtue of the *removal* of the researcher from the data that he or she is investigating.<sup>2</sup> They claim that survey techniques may be appropriate for the study of attitudes and sentiments easily verbalized, but for the study of the on-going process of human interaction, participant observation, with its built-in dynamic and responsive attributes, is recommended.

While in substantial agreement with those who argue the validity of the fieldwork approach, nevertheless, we are frustrated by what appears to be a kind of jealous guardianship that prohibits the revealing of the "trade secrets" of fieldworkers. Accounts of field experiences tend to range from abstract typologies (which frequently give us little idea as to the data from which they were derived) to the anecdotal episode (whose generality remains in question) (cf. Gold, 1958). Systematic reportings are missing and we, as fieldworkers, are handicapped in our research by the failure of other researchers to render accountable their strategies for resolving problems that arise in the course of the research. This is doubly unfortunate in that private insights do not become public knowledge upon which others could draw, and, further, this general failure to report the oftentimes checkered course of fieldwork increases the vulnerability of such studies to criticism from proponents of a more "rigorous" research protocol.

#### NEW WORK: OLD PROBLEMS

Toward the end of contributing to the working out of systematic methods of dealing with fieldwork problems and in the

interest of rendering us more accountable, we are going to consider some selected issues that arose in the course of a research study recently completed by the author (Stephens, 1976).

Let us preface our discussion by noting that there is one problem that the fieldworker has already solved, the fundamental problem of identifying the *kind of data* that he or she is going to study. By selecting a participant observation approach, we opted to study what is in fact the basic datum of sociology, i.e., social interaction. The participant observer does not settle for questionnaires administered from afar, which assume that people really do what they say they do (a contention difficult to accept, given the perversity of human beings who have often been observed behaving in ways that at best bear little relationship to verbalized attitudes and at worst flagrantly contradict them). The fieldworker has, by virtue of the research protocol chosen, elected to avoid isolating or compressing the process of social interaction into any artificial form, and, instead, has made a commitment to a direct examination of the empirical social world.

The problems that arise in the course of work in the field are fundamentally the productions which arise out of the process of interaction. Fieldwork, as are all research methodologies, is social interaction, and has its characteristic rules regarding what does and does not constitute adequate role performance, such as the "taken-for-granted meanings" that define the social reality of the interacting parties—in short, the common characteristics of social situations. The difficulties, misunderstandings, and frustrations that are endemic to all interpersonal transactions are to be encountered in the interactional process of fieldwork. We, as sociologists interested in the study of the vicissitudes of social interaction, ought, therefore, to turn our attention to the study of the research act in general and participant observation in particular.

As a participant observer, the fieldworker must play a dual role, that of social scientist with specific research goals and that of a member of a system of interacting others. We suspect that the more frequent pitfall lies in the overextension of the latter role. Certainly, we are cautioned continually against the danger of over-rapport or over-identification with certain individuals within

the target group. The researcher who over-identifies with the whole group produces serious threats to the external validity of his/her findings, rendering questionable the generalizability of findings to larger social groupings. The fieldworker who over-identifies with selected individuals, on the other hand, raises threats to the internal validity of his/her study. Not only may he/she be unable to develop propositions that may be generalized to other groups, but he/she runs the risk of misrepresenting the very group that he/she wishes to know about.

This latter case is probably the more common problem encountered in fieldwork. Reliable individuals must be found and groomed for their role as informants. But very often informants who are worth developing and educating to the goals of the researcher are a selective group. They are likely to be articulate, naive observers, and in various ways rather exceptional people. They may tend to be more analytical than other individuals in the group and more prone to scrutiny of the taken-for-granted meanings that define the social reality of their group experiences.

When such individuals speak for themselves, we can record it as such and no particular difficulty arises. However, we must rely upon the reports and observations of our informants in many cases where we were not there to observe the data first hand, and it is here that we may get into trouble. When informants speak for others, we are getting observations and data through the filter of their beliefs and interpretations which may not be representative of the group as a whole. We are to some extent "removed" from the data. To resolve this discrepancy, it is necessary to cultivate other informants who can "balance" the insights of these informants. A further reliance upon multiple measures will promote sensitivity to discrepancies between the accounts of informants. However, the fact of the matter is that there is relatively little in the literature that deals with this issue in a *practical* fashion.<sup>3</sup>

Additionally, we are liable to encounter difficulties in terms of our becoming associated with certain cliques, thereby alienating other cliques. The potentially devastating consequences of this fieldwork hazard should be obvious to all of us. In our research we found the best method of dealing with this to lie in what Blanche

Geer (1970) calls "touching base", that is, a deliberate strategy of frequent informal chats to keep important individuals informed and interested. Nevertheless, this strategy does not dispel entirely the temptation to gravitate toward those individuals with whom one feels a certain affinity.

Another problem stemming from the erosion of the researcher role is that the fieldworker will become so intimately involved in the routine affairs of his/her subjects that he/she begins to look for "important" things in conversations, behavior, events, and ignores or becomes unaware of the uninterrupted flow of people being themselves which is the real stuff of social relationships and the ultimate data of sociological research. When this begins to happen, and a reliable indication is the "just marking time" feeling, then we suggest that it's a good time to back off. If necessary, leave the site for a day or two and use this time to review research goals.

Of major concern to those of us interested in making accountable our methods of collecting data is the investigation of the interactional process of fieldwork. It is time to admit once and for all that in the interactional situation of fieldwork we expect both to give and to receive certain benefits that make possible and sustain social relationships. That we are scientists studying subjects does not change the fundamental nature of the situation: we are partners in an interpersonal transaction, i.e., we are interacting selves in relationship to other interacting selves.

A concern arising from the interactional nature of fieldwork is the necessity of the researcher to give up certain rights in order to maintain the relationship. Chief among these are the right to "be oneself" and the right to privacy. The following entry from our fieldnotes illustrates the strain on the researcher occasioned by the relinquishment, though only temporary, of such rights.

In this type of study the researcher flirts with exhaustion. You can't really be yourself. You're always public. Even when you're in your room, you're busy planning the next day's work. Leaving the site doesn't really remove one psychologically from a preoccupation with study. It's difficult, this stricture against being

my 'real' self. I always have to control my reactions and frequently find myself sacrificing spontaneity for strategy.<sup>4</sup>

Herein lies the major seduction of establishing close relationships with informants that might debilitate objectivity for these are the individuals with whom the fieldworker is likely to feel "at home." Being oneself does not occur in a vacuum, but involves being oneself in interaction with reacting others. The confirmation of selfhood depends so intimately on continuing feedback from other selves that one is never oneself alone. This axis where the self and interaction meet is not only a problem to be resolved by the fieldworker, but, indeed, goes to the very heart of the research enterprise for it is here that method and theory converge.

Another neglected problem involves the issue of reciprocity. There is an audacity shared by researchers, whether their site be a village in Columbia or a hospital in San Francisco: they enter into the lives of their subjects, knowing that theirs is not an enduring social commitment. They know that after a time they will abandon those relationships, whose primary function is the furtherance of research interests. How does the fieldworker reciprocate? How can he/she reimburse people for their acceptance, time, and efforts? An entry from our daily log indicates that at least some of our subjects were thinking about this issue, too:

'Aristotle' asked me today in the bar what I was going to do when the study was completed. He wanted to know if I planned to 'abandon' them. I found myself fumbling for adequate assurances that I hoped to maintain ties with a number of people in the hotel.<sup>5</sup>

One of the taken-for-granted meanings that underlies human sociality is that there will be mutual benefits to the parties in a relationship ranging from goods, sentiments, ego enhancements, etc. The benefits received by the fieldworker are manifest, but in many cases the benefits to those studied are not nearly so obvious. The question of reciprocity must be faced by every researcher, but

for some reason they have been coy in telling us how they wrestled with and resolved this matter. In our research we found ourselves uncomfortable with the traditional answer to this question—the familiar slogan about how by making a contribution to systematic knowledge these people will be helped "in the long run"—and wondered if it wasn't just a rationalization readily available to social scientists.

### CONCLUSION

These are just a few of the issues that arose in the course of our research. They by no means exhaust the potential sources of trouble to the participant observer. The exigencies of the research "bargain", reaching the socially invisible subject, problems of replicability—to name but a few—have not been included in our discussion. We have hoped, however, to call attention to some issues that fieldworkers are, in our judgment, overdue in addressing. If we are to hope for confidence in our findings on the part of the larger social scientific community, then we must begin to make accountable the specific research protocols utilized. This means that we have to make public the sometimes checkered course of our fieldwork experience; we must account for the strategies of which we made use to resolve problems that arose during the course of collecting data, and this must include reporting those efforts that failed. When we begin to do this, we shall have made a good start toward improving the replicability of participant observation studies.

Finally, we contend that a most fruitful means of coming to grips with these issues is to acknowledge that the "process of collecting sociological data is itself an interactional process, having features in common with other situations of human interaction" (Phillips, 1971:77). As social scientists we study interaction. Why, then, with a few notable exceptions, have we neglected to study the research act as social interaction?

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NOTES

1. This article is a revised version of a paper delivered to the Michigan Sociological Association, (Spring meeting, April, 1973).
2. For a particularly thorough discussion of the differences between survey research and participant observation, the reader is urged to read Derek L. Phillips' (1971) excellent book on this subject.
3. The case for the adoption of multiple measures which makes possible the "triangulation" of data has been argued convincingly in two recent works: Norman K. Denzin (1970) and Eugene J. Webb (1966).
4. Loss of privacy and intense management of the public self are, of course, not peculiar to the business of fieldwork; however, they do become occasional sources of grief to the researcher in his/her quest to gain understanding and the acceptance of his/her subjects. As subtle components of the research "bargain" and the developing mutual expectations in the fieldwork situation, they are in need of our systematic study.
5. The poignancy of this entry lies, of course, in the inevitable comparison the researcher made between the seriousness with which subjects asked this question, and the generally facile treatment given to it in the accounts of social scientists.

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