

treatment, and possibilities for the mentally ill in our society? Is it an "alternative" and, if so, of what sort, for whom, and for how many?

—Juniper Wiley
University of California, Los Angeles

Journal of Contemporary Ethnography

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ON FIELDWORK

ERVING GOFFMAN

(transcribed and edited by Lyn H. Lofland)

EDITOR'S INTRODUCTION

What follows is a transcription of a tape-recorded talk given by Erving Goffman during the 1974 Pacific Sociological Association Meetings, where he was a member of a panel of successful fieldworkers discussing their data collection and analysis procedures. John Lofland, who organized the session, had invited Sherri Cavan, Fred Davis, and Jacqueline Wiseman, as well as Goffman, to talk candidly and informally about how they went about doing their work. Somewhat revised versions of the Cavan, Davis, and Wiseman talks, along with an additional piece by Julius Roth, were published in this journal in a special section of the October 1974 issue. Claiming that his own remarks had been too informal to warrant publication, Goffman asked not to be included.

Erving Goffman liked neither to be photographed nor to be "taped" and, very much in keeping with his usual practice, at the beginning of his talk he asked that no recordings be made. However, appropriate to an overflow audience composed heavily of enthusiastic, if not totally ethical, fieldworkers, surreptitious recordings were, in fact, made and the transcription that follows is one result.

While Goffman was alive, there was every reason to hope he might eventually turn these informal remarks into a published piece. With his premature death in 1982, however, this hope was shattered. For oddly enough, despite the many students whose fieldwork he supervised and despite his own numerous experiences, Goffman never published anything on the topic. He had a great deal to say about the matter, as his many students can certainly attest, but what

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he had to say was communicated orally and remains only in the memories of a small number of social scientists. What he said that day in March 1974 may not be earthshaking. He was, in this instance, a creative carrier of a tradition, not its inventor. But what he had to say was, as one would expect from Goffman, thoughtful, uniquely insightful, and, in places, eloquent. I am grateful to his widow, Gillian Sankoff, who agreed with us that the value of this "oral essay" overrode Goffman's expressed wish that it not be preserved, and who gave permission for its publication.

A final word about the accuracy and editing of the transcription. Unsurprisingly, given the "undercover" manner in which it was recorded, the quality of the tape is poor. Despite the use of techniques that improved that quality somewhat, portions of the talk are not sufficiently intelligible to include. Fortunately, it is clear from the tape that these portions consist entirely of "asides," brief forays into topics that are mentioned and then dropped, and their loss does not detract from the substance of what Goffman had to say. As I hope will be apparent to people who knew him well and/or who were present at the panel session, my editing of the transcription has been light. I have certainly not attempted to translate "spoken Goffman" into written prose (in fact, I have tried, with punctuation, to convey the cadence of his speech), but I have, for purposes of clarity, dropped an extraneous word here and there, added an occasional word (in brackets), and, in one or two instances, slightly altered sentence structure.

* * *

I am going to report on what I conclude from studies of this kind that I've done. And I can only begin by repeating John Lofland's remarks that what you get in all of this [attempt to articulate techniques] is rationalizations,¹ and we're in the precarious position of providing them. The only qualification of that precariousness is that ordinarily people

go into the field without any discussion at all, so we can't be damaging the situation too much.

I think there are different kinds of fieldwork: going on digs, experiments, observational work, interviewing work, and the like, and these all have their own characters. I only want to talk about one kind and that's one that features participant observation—observation that's done by two kinds of "finks": the police on the one hand and us on the other. It's us that I want to largely talk about, although I think in many cases they do a quicker and better job than we do.

By participant observation, I mean a technique that wouldn't be the only technique a study would employ, it wouldn't be a technique that would be useful for any study, but it's a technique that you *can* feature in some studies. It's one of getting data, it seems to me, by subjecting yourself, your own body and your own personality, and your own social situation, to the set of contingencies that play upon a set of individuals, so that you can physically and ecologically penetrate their circle of response to their social situation, or their work situation, or their ethnic situation, or whatever. So that you are close to them while they are responding to what life does to them. I feel that the way this is done is to not, of course, just listen to what they talk about, but to pick up on their minor grunts and groans as they respond to their situation. When you do that, it seems to me, the standard technique is to try to subject yourself, hopefully, to their life circumstances, which means that although, in fact, you can leave at any time, you act as if you can't and you try to accept all of the desirable and undesirable things that are a feature of their life. That "tunes your body up" and with your "tuned-up" body and with the ecological right to be close to them (which you've obtained by one sneaky means or another), you are in a position to note their gestural, visual, bodily response to what's going on around them and you're empathetic enough—because you've been taking the same crap they've been taking—to

sense what it is that they're responding to. To me, that's the core of observation. If you don't get yourself in that situation, I don't think you can do a piece of serious work. (Although, if you've got a short period of time, there would be all kinds of reasons why you wouldn't be able to get in that situation.) But that's the name of the game. You're artificially forcing yourself to be tuned into something that you then pick up as a witness—not as an interviewer, not as a listener, but as a witness to how they react to what gets done to and around them.

Now there are two main issues following from that. What you do after you get the data, which Jackie [Wiseman] has addressed herself to. And the other is, how you go about acquiring the data. And I think that, in turn, divides up into two general problems, that of *getting into place* so that you're in a position to [acquire data] and the second is the *exploitation of that place*. There's a minor phase of getting out—of "getting out" in your head—which we could look at later on if you want to.

[GETTING INTO PLACE]

I want to talk very briefly—a few minutes each—on those two major phases: that of getting into place and that of exploiting place once you get into it. There are certain rules in the trade about getting into place: You do a survey, you mess up some field situations that you're not going to use to find a little bit out about their life, you develop rationales for why you should be there. You have to anticipate being questioned by the people whom you study so you engage in providing a story that will hold up should the facts be brought to their attention. So you engage in what are sometimes called "telling" practices. (In the early years of this business, we frowned upon total participant observation, that is total passing in the field, because people had very fancy notions about what it would be like to be discredited. I don't mean moral issues, I mean concerns

about the fact that they would be discovered and be humiliated. I think, at least in my experience, it's proven to be a fact that that's much exaggerated and that you can act as though you're somebody you're not and get away with it for a year or two. Whether you want to do that, of course, is another issue, one that bears on the ethical and professional issues attached to participant observation. I would be happy to talk about that, but I'm not talking about that right now.) So you have to get some story that will be—I like a story such that if they find out what you are doing, the story you presented could not be an absolute lie. If they don't find out what you're doing, the story you presented doesn't get in your way.

Now the next thing you have to do is cut your life to the bone, as much as you can afford to cut it down. Except for a few murder mysteries or something you can bring along in case you get really depressed, remove yourself from all resources. One of the problems of going in with a spouse, of course, is that while you can get more material on members of the opposite sex (especially if you go in with a kid), it does give you a way out. You can talk to that person, and all that, and that's no way to make a world. The way to make a world is to be naked to the bone, to have as few resources as you can get by with. Because you can argue—just as Jackie argued that every world makes sense to people—you can argue that every world provides substance for the people, provides a life. And that's what you're about, [that's what you're] trying to get quickly, you see. So, the way to get it is to need it. And the only way to need it is to not have anything of your own. So you should be in a position to cut yourself to the bone. But lots of people don't do that too much—partly because of the contingencies of getting a degree and all that.

Then there is the other issue, which I'll only remark on briefly and then go on to a little bit on note taking and the like, and that's the self-discipline required. As graduate students, we're only interested in being smart, and raising

our hands, and being defensive—as people usually are—and forming the right associations, and all that. And if you're going to do good fieldwork, it seems to me, that's got to go by the board. You've got to really change your relationship to the way you manage [the] anxieties and stresses of the social networks around you. For one thing, you have to open yourself up to any overture. Now, you can't follow up these overtures because you may early associate yourself with the wrong person. You've got to be disciplined enough with the people to find out what the various classes of individuals are that are involved in the place. You've got to then decide which class you're going to study. Once you do that you've got to find out about the internal cleavages within the class, and then decide which internal cleavage you're going to accept as your own. So, you shouldn't get too friendly. But you have to open yourself up in ways you're not in ordinary life. You have to open yourself up to being snubbed. You have to stop making points to show how "smart assed" you are. And that's extremely difficult for graduate students (especially on the East Coast, especially in the East!). Then you have to be willing to be a horse's ass. In these little groups, the world consists of becoming very good at doing some stupid little things, like running a boat, or dealing, or something like that, you see. And you're going to be an ass at that sort of thing. And that's one reason why you have to be young to do fieldwork. It's harder to be an ass when you're old. And you have to engage in a strategy with respect to costume. People don't like to cut their hair, for example,² so they retain something of their own self, which is nonsense. On the other hand, some people try to mimic the accents of the people they're studying. People don't like to have their accents mimicked. So you have to get a mix of changing costume, which the natives will accept as a reasonable thing, that isn't complete mimicry on the one hand, and that isn't completely retaining your own identity either.

Then, there's the issue, again, as part of the way in which you discipline yourself, of what you do with confidants.

people like to find a friend where [they're doing their study] and tell the friend the "true things" and discuss with their friend what's going on. Unless that friend is in a structural position of not being able to retell the stories—and there are ways in which you could find such a friend—then I don't think you should talk to anybody.

Now there are also tests that you can run on whether you've really penetrated the society that you're supposed to be studying and I'll mention some of these briefly in passing. The sights and sounds around you should get to be normal. You should be able to even play with the people, and make jokes back and forth, although that's not too good a test. People sometimes assume that if they're told strategic secrets, that's a sign that they're "in." I don't think that's too good a sign. One thing is, you should feel you could settle down and forget about being a sociologist. The members of the opposite sex should become attractive to you. You should be able to engage in the same body rhythms, rate of movement, tapping of the feet, that sort of thing, as the people around you. Those are the real tests of penetrating a group.

[EXPLOITING PLACE]

Let me talk for one minute before I quit on what you do after you get in the situation. First, I'll review this business of "getting in." Remember, your job is to get as close to some set of individuals as possible. So you've got to see that they're aligned against some others that are around. There's no way in which, if you're dealing with a lower group, you can start from a higher group, or be associated with a higher group. You've got to control your associations. If you get seen in any formal or informal conversation with members of a superordinate group, you're dead as far as the subordinate group is concerned. So you've got to really be strategic and militant about the way you handle these social relationships.

Now about exploitation of the place you're in. I think you should spend at least a year in the field. Otherwise you don't get the random sample, you don't get a range of unanticipated events, you don't get deep familiarity. It's deep familiarity that is the rationale—that, plus getting material on a tissue of events—that gives the justification and warrant for such an apparently "loose" thing as fieldwork.

Then there is the affiliation issue. You can't move down a social system. You can only move up a social system. So, if you've got to be with a range of people, be with the lowest people first. The higher people will "understand," later on, that you were "really" just studying them. But you can't start at the top and move down because then the people at the bottom will know that all along you really were a fink—which is what you are.

Note taking: two minutes or a minute on note taking. There is a freshness cycle when moving into the field. The first day you'll see more than you'll ever see again. And you'll see things that you won't see again. So, the first day you should take notes all the time. By the way, about note taking, obviously you find corners in the day when you can take notes. And every night you should type up your fieldnotes. [And] you have to do it every night because you have too much work to do and you'll begin to forget. Then there are various devices you can use. You can start penetrating by going to open socials where, indeed, people might allow you to take open notes. If you put your notebook on a larger piece of paper, people won't see your notebook. It's masked. They won't be disturbed by it. [Learn to] fake off-phase note taking. That is, don't write your notes on the act you're observing because then people will know what it is you're recording. Try to discipline yourself to write your notes before an act has begun, or after it has started so that people won't be able to detect from when you start taking notes and when you stop taking notes what act you're taking notes about.

There's an issue about when to stop taking notes. Usually when you are merely duplicating what you've already got.

Remember, you'll get, in a year, between 500 and 1,000 pages of single-spaced typed notes and this will be too much to read more than once or twice in your lifetime. So don't take too many notes.

Then there's [the matter of] what to do with information. Jackie takes seriously what people say. I don't give hardly any weight to what people say, but I try to triangulate what they're saying with events.

There's the issue of seeking multi-person situations. Two-person situations are not good because people can lie to you while they're with you. But with three people there, then they have to maintain their ties across those two other persons (other than yourself), and there's a limit to how they can do that. So that if you're in a multi-person situation, you've got a better chance of seeing things the way they ordinarily are.

Now, a point that I think is very important is this. We tend, because of our peculiar training, to try to write defensible statements, which is language written in Hemingway-type prose, defensible prose. That's the worst possible thing you can do. Write [your fieldnotes] as lushly as you can, as loosely as you can, as long as you put yourself into it, where you say, "I felt that." (Though not to too great a degree.) And as loose as that lush adverbialized prose is, it's still a richer matrix to start from than stuff that gets reduced into a few words of "sensible sentences." I'm now not [supporting] unscientific [practices] or anything like that. I'm just saying that to be scientific in this area, you've got to start by trusting yourself and writing as fully and as lushly as you can. That's part of the discipline itself, too. I believe that [other] people shouldn't read [your] fieldnotes, partly because it's a bore for them. But if they are going to read your fieldnotes, you'll tend not to write about yourself. Now don't just write about yourself, but put yourself into situations that you write about so that later on you will see how to qualify what it is you've said. You say, "I felt that," "my feeling was," "I had a feeling that"—that kind of thing. This is part of the self-discipline.

Now, these are comments on note taking. There are issues about getting out, about leaving the field so you can come back to it, [but] I think we can leave that, and—I'm going to stop right now.

NOTES

1. In his introductory remarks, John Lofland had commented that it was likely difficult for fieldworkers to "know" exactly what it is they do to generate their analyses.
2. Recall that the year is 1974 and, especially for younger men, long hair was still both stylish and a symbol of distance from the "establishment."

The question of the relationship between concepts and data is central to sociological methodology. It is also an issue that divides quantitative and ethnographic approaches. The most influential ethnographic treatment of this problem is that provided by Herbert Blumer. This article offers a detailed account and a critical assessment of Blumer's position. Its conclusion is that while his discussion of the problem is instructive he fails to provide a convincing solution.

**THE PROBLEM OF THE CONCEPT:
Herbert Blumer on the Relationship
Between Concepts and Data**

MARTYN HAMMERSLEY

A recurring methodological problem in sociology is the relationship between theory and empirical research. Central here is the question of how theoretical concepts are to be linked to empirical data. Different research traditions give this issue varying salience and adopt different solutions. In one form or another, though, it has been at the heart of many debates, particularly between advocates of "quantitative" and "qualitative" methods.

Quantitative researchers have typically argued that if the application of concepts is to be rigorous they must be specified in terms of standardized, concrete indicators, preferably structured so as to form a scale. Much effort has been expended in seeking to tie down social science concepts in this way through the use of interview and observational schedules, attitude inventories, and so on,

AUTHOR'S NOTE: This article has been in process for a number of years, and I would like to thank several colleagues for their comments on it at various stages: John Scarth, Donald Mackinnon, Stephen Ball, Jennifer Platt, Peter Adler, Patricia Adler, David Maines, and two anonymous referees.

I completed a draft of this article in early 1987 and sent a copy to Herbert Blumer. A month or so later I learned of his death. If he had had the chance to read and respond to it, no doubt there would have been a characteristically forthright reply. Unfortunately, we are all now deprived of future contributions from him.