Sociological Work
Method and Substance

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reader, as do present modes of statistical presentation, opportunity to make his own judgment as to the adequacy of the proof and the degree of confidence to be assigned the conclusion.

Conclusion

I have tried to describe the analytic field work characteristic of participant observation, first, in order to bring out the fact that the technique consists of something more than merely immersing oneself in data and "having insights." The discussion may also serve to stimulate those who work with this and similar techniques to attempt greater formalization and systematization of the various operations they use, in order that qualitative research may become more a "scientific" and less an "artistic" kind of endeavor. Finally, I have proposed that new modes of reporting results be introduced, so that the reader is given greater access to the data and procedures on which conclusions are based.

CHAPTER THREE

Field Work Evidence

How credible are the conclusions derived from data gathered by field work? If we enter to some degree into the lives of the people we study, take part in their daily round of activity and observe the scenes and sites where it occurs; if we talk with them both informally and in relatively organized interviews; if we inspect organization records, official documents, public media, letters, diaries, and any other records and artifacts we can find; if we record systematically all the information we acquire in these ways; and if, finally, we assess that information systematically to see what evidence it provides for what conclusions—if we do all that, should people take our conclusions to be highly credible? Or should they find it risky to give much evidentiary value to conclusions so arrived at? 1

Anthropologists might find the question foolish, because they do so much of their work this way and because so many of their classics depend on such evidence; but a minority among them, possibly a growing minority, might see some sense in the question, feeling that anthropological field work techniques are too unstructured to produce reliable knowledge. Psychologists, on the other hand, find the question meaningful and, indeed, are forever raising

Social category: some elementary schools may resemble prisons, others support, and dozens of other ways that might easily account for the differences in our descriptions. We should never assume that two institutions looked very different from Robert Redfield's, the medical school my colleagues and I studied seemed very different from the one studied by Merton et al.; two studies uncover such differences the result is anomalous only if we insist that things called by the same name therefore are the same.

These disparities may occur simply because the organizations are in fact not the same. The passage of time may have changed Tepoztlan substantially; it would not be surprising. The University of Kansas Medical School we studied differs from the Cornell Medical School that Merton et al. studied in location, the recruitment of faculty and students, the sources of support, and dozens of other ways that might easily account for the differences in our descriptions. We should never assume that two institutions are alike simply because they belong to the same conventionally defined social category: some elementary schools may resemble prisons, others country clubs, while still others do indeed look like ordinary schools. If two studies uncover such differences the result is anomalous only if we insist that things called by the same name therefore are the same.

But suppose even that two researchers study the identical organization and yet describe it quite differently. In laboratory research, we think it very unlikely that people would give very different descriptions of what took place in the same experiment. The experimenter did this, the subjects did that; one may argue the interpretation, but one seldom disputes the description. So varying descriptions of the same organization distress people used to research modeled on the experimental paradigm.

But their distress may rest on an unwarranted assumption: that the two field workers in question set out to study the same thing, answer the same questions. People often study the same or similar organizations using field methods but have different theories and different questions in mind. When


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they ask different questions, they get different answers. The data in the two studies will indeed be different, but the difference does not indicate that the information is untrustworthy. It shows only that the observer is observing something different.

We mistakenly assume that observers must be studying the same thing because we suppose that only one social structure is present in an organization or community. That is true in some general sense. All the people who occupy a given geographical area or a particular building that houses a given organization do in some sense constitute one large social structure. But the overall structure contains smaller units and the difference between two field studies of the same thing may lie in their differential emphasis on one or another of these smaller units. Whyte and Gans described essentially similar low-income Italian districts in Boston, but their descriptions are vastly different. Whyte describes the characteristic activities of a group of young unmarried men and explores their relation to the political structure of the community and to a settlement house located there. Gans describes what seems to be a quite different society made up of married people and their families and containing, in addition to the settlement house, a host of other institutions.

But no one would argue seriously that the difference between Gans' description and Whyte's shows that one or the other or both are implausible or that their data are untrustworthy. They focused on different parts of the total community, and they asked different questions about them. There is no reason for their descriptions to be similar.

In the same way, two observers might study the same organization or organizational part, but with reference to different problems. When Renee Fox studied medical freshmen, she wanted to know how the social structure of the medical school trained them in qualities likely to be important for successful performance as physicians; when Blanche Geer studied medical freshmen she wanted to know how they organized themselves to deal with the everyday problems of being medical students. They studied the same thing, but they studied different aspects of it, and we should not expect their descriptions of the social structure of the freshman year of medical school to be identical.

In general, we should not expect identical results when two observers study the same organization from different points of view or when they study different substructures within a larger organization. What we have a right to expect is that the two descriptions be compatible, that the con-


4. See Merton et al., op. cit., and Becker et al., op. cit.
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clusions of one study do not implicitly or explicitly contradict those of the other. Thus, we can see that Whyte and Gans described essentially the same kind of community, for Whyte's description of the corner boys meshes perfectly with Gans' description of the family units in the community; the families Gans describes are just the families we expect the corner boys to come from and to create themselves when they have the chance.

We now approach the heart of the question, which has to do with the lack of strict procedural rules to guide a field worker's data-gathering activities. Suppose that two observers ask the same question of the field situation they observe and also use similar modes of analysis. Isn't it possible, even likely, that the lack of formalization in data-gathering techniques will allow whatever biases the investigator has to mold the data he gets? Won't he then, in effect, simply be studying his own prejudices, the data so perverted by his (probably unconscious) influence that we cannot use it as scientific evidence?

The question has often been raised about field work data but has added force because of the studies showing the effect of the investigator's bias in much more controlled situations. Studies by the staff of NORC and others demonstrate that the characteristics and biases of survey interviewers exert a considerable effect on the answers they get from respondents. Even more shocking, Rosenthal's studies of experimenter bias have shown that the experimenter's knowledge of the hypothesis he is testing and of the conclusion he expects to arrive at affect the responses of the subjects of social-psychological experiments. Rosenthal even reports that the experimenter's bias affects the outcome of animal experiments. If the researcher's bias can affect the data gathered in these more controlled styles of research, is it not much more likely in the unformalized techniques of field work, where the observer has infinitely more opportunity to emit cues that affect those he studies and to pick from all that is happening only the evidence that suits him?

There are good reasons to believe that the opposite is true. Field observation is less likely than the more controlled methods of laboratory experiment and survey interview to allow the researcher to bias the results he gets in directions suggested by his own expectations, beliefs, or desires. Almost every field worker believes that proposition, ordinarily because he has often had to sacrifice pet ideas and hypotheses to the recalcitrant facts in his field notes. Before I discuss the characteristics of data gathering in the field that produce those recalcitrant facts, I want to introduce the personal experience that convinced me on this score.

My dissertation dealt with the career patterns of Chicago public school teachers. I knew, before I started to interview teachers, that most teachers began their careers in Negro and slum schools and made strenuous, and usually successful, efforts to escape into middle-class schools. One of my major problems was why they did so. My advisor, Everett C. Hughes, had a theory about it. He expected that I would find what he and his students had found in other professions: that the highest professional prestige went to those who had clients of the highest social status. Teachers would prefer middle-class schools because they regarded them as the most prestigious places a teacher could work. I firmly believed Professor Hughes' theory correct; even if I had not, the wily wisdom of graduate students dictated that I pretend to believe it and make every effort to prove it. When I interviewed teachers, however, they refused to support my expectation. They gave a good many reasons for not liking Negro and slum schools, in answer to my more or less nondirective questions, but none of the reasons had anything to do with professional prestige. Even when at the end of the interview (where it could do no harm) I pursued teachers with direct and leading questions, they simply denied that prestige had anything to do with their actions. My expectation, firmly grounded in theory, belief, and what I took to be my own self-interest, had no effect on my data. (When, with some trepidation, I reported my results to Professor Hughes, he turned out to be much more ready than graduate-student culture had led me to believe to embrace the new findings and revise his theory; so much for the wily wisdom of graduate students. That my vision of my self-interest was incorrect, however, did not in the least moderate its influence on what I hoped to find, or the inability of my hopes to influence reality.)

But the firmest convictions of field workers, based on just such anecdotes, neither convince doubters nor explain analytically the reasons we should take field work data seriously as evidence. Those reasons fall into two categories, which I now propose to examine. 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 in-


7. The findings of that research are reported in Chapters 9-11 of this volume.
evitably, by his continuous presence, gathers much more data and, in a sense to be explained, makes and can make many more tests of his hypotheses than researchers who use more formal methods.

Constraints

Rosenthal, Friedman, Orne, Rosenberg, and others have shown, in my judgment quite conclusively, that the subjects of carefully controlled psychological experiments respond not only to the stimuli specified in the experimental design, but also to a variety of other stimuli found in their relationship to the experimenter. Believing in science and wanting to help the scientist, subjects respond to the "demand characteristics" of the experiment and do things they would not otherwise do—like giving the experimenter's stooges apparently lethal electric shocks—because that seems to be what they are "supposed" to do. Believing that psychologists can tell things about you from seemingly innocent bits of behavior and not wanting to appear "sick," " unintelligent" or whatever other bad thing a psychologist might be able to discern, they respond in ways they think will cause a psychologist to think well of them. For reasons not quite so clear, they alter their behavior so as to confirm the hypothesis the experimenter expects to be confirmed.

Since experimenters attempt to influence subjects only in the ways specified in their design, the findings from this research indicate that subjects respond to very slight cues, which not only are not intended to have an effect, but which no one even conceived as possibly having an effect. Minor variations in the wording of instructions, changes in the name of the organization allegedly sponsoring the research or in the name of the tests administered, differences in the number of times the experimenter smiled—it is trivia of this kind that confounded experimental results.


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Similarly, survey researchers have discovered that the social attributes and the attitudes and beliefs of survey interviewers affect the answers their respondents give. People answer questions about race differently when interviewers are of one color or another, and they similarly vary their answers on questions about sex and mental illness depending on the age and sex of the interviewer. Interviewers get the answers they expect, just as experimenters get the responses they expect.

It is not so clear that the influences on survey responses come from the trivial stimuli that influence experiments. For one thing, interviewers work in a less supervised situation; we cannot observe them going about their business and ordinarily rely on their own reports of what went on. So they are freer than experimenters to deviate from their instructions, and they might do so in grosser ways. Some of the variation may come from interviewer cheating. Still, interviewers' actions are constrained by their instructions and by the fixed wording and question order of the schedules they administer, so that their effect on responses must result from relatively small variations in behavior.

Field workers have a great deal more freedom than either experimenters or survey interviewers. They can ask anyone anything they want to ask, can use the most outrageously leading questions and the most biased wordings; they can take actions of all kinds, not just the minor variation in number of smiles that affects an experiment's results but much bolder interventions in an organization's workings; they can indicate not only an indirect and muted evaluation of another's actions but also the most direct kind of positive evaluation, as when they join in some controversial group activity. They can, in short, produce stimuli of a grossly biasing kind, much grosser than those that have been demonstrated to have serious effects in more controlled styles of research. How can we put any weight in conclusions based on data so produced?

So far, I have focused on the freedom of the researcher, in various techniques, to engage in potentially biasing forms of behavior vis-à-vis the people he studies. But to suppose that the subjects of field research are affected by the bias of the observer, and mold their acts and words to what they think he wants, supposes not only that they are willing to do so but that they are free to. It supposes that they are under no other constraints and so can follow their disposition to be pleasing, if they have such a disposition. But this freedom is found most characteristically in the laboratory experiment, where the ideal of control is precisely to remove all influences other than those the experimenter wants to operate. Experimenters neutralize external constraints by isolating the subjects of their experiments from their usual surroundings, by experimenting on topics unconnected with any strong be-
lies held by the subject, and by assuring the subject that his behavior in the experiment, however he performs, will have no influence on his life outside the experimental laboratory. To precisely the degree that these aims are achieved, subjects are free to shape their words and deeds in accordance with cues unwittingly given off by a biased experimenter.

Somewhat the same freedom is available to the respondent in a survey interview. He is approached by someone he has never seen before and never expects to see again who asks him a series of questions about his attitudes on a variety of topics, none of which he has any direct control over or responsibility for. His answers will not have the slightest effect (and so he is assured by the well-trained interviewer) on anything in his real life. Since he is not constrained by anything but the pressures arising in the immediate face-to-face situation of the interview, these pressures are most likely to have a biasing effect on what he says. Hence the findings of the studies on interviewer bias. Are whites embarrassed to admit prejudiced feelings to the interviewer? Do women hesitate to discuss sex with a young male interviewer? They are free to indulge these embarrassment and hesitations, for they are guaranteed that the frankness of their answers (or the lack thereof) will have no consequence beyond the immediate situation. Why not avoid embarrassment, since what you say makes no other difference?

Consider, in contrast, the people a field worker studies. They are enmeshed in social relationships important to them, at work, in community life, wherever. The events they participate in matter to them. The opinions and actions of the people they interact with must be taken into account, because they affect those events. All the constraints that affect them in their ordinary lives continue to operate while the observer observes.

Whether or not the person being observed knows what the observer expects of him, he dare not respond to that expectation. What he is involved in at the moment of observation is as a rule much more important to him than the observer is. If I observe a college student responding to a teacher in the classroom, I observe a person to whom my reactions are much less important than those of the teacher, who may give him a low grade, and even of other students, whose opinion of him has consequences long after he has seen the last of me. He may not care to have me think him stupid, naive, or deceitful; but better I should think so than those whose opinions are more fateful than mine.12 Similarly, when Skolnick observed police officers, they were busy doing things to earn them promotions (or at least prevent reprimands) from their superiors, things to maintain their position in the department and vis-a-vis lawbreakers, such as prostitutes, with whom they had repeated contact. They might like to have Skolnick think well of them, but they want much more to have their officers think well of them, to have whores respect them enough to do what they are told and so on.13

In short, the presence in the observational situation of the very social constraints the sociologist ordinarily studies makes it difficult for the people he observes to tailor their behavior to what they think he might want or expect. However much they want to, the real consequences of deviating from what they might otherwise have done are great enough—loss of a promotion or of reputation in the eyes of stable members of their community—that they cannot.

Two further remarks are in order. First, the analysis above does not apply, of course, when the people observed regard the observer in fact as important enough to constitute a threat and thus act as a real-life constraint on what they do. When they believe that, they will put on a show for him quite like the one they may for different reasons put on for an experimenter or survey interviewer. Industrial managers, for example, may suspect that a sociologist's findings presented to and interpreted by their superiors will uncover matters they would prefer kept secret and thus have untoward consequences for their immediate situations and future careers. Dalton suggests that a good many researchers have been led down the garden path by wary managers.14 Schoolteachers and their pupils often collaborate to put on demonstrations of efficiency, industry, and harmony for a visitor because they fear that he may give a bad report to the principal and cause them all grief. Any situation where participants may cast the observer in the role of Inspector General contains this difficulty.

One cure for the ill consists of convincing people that you are not important, that those who control their fate don't know you or, if they do, don't care much what you say. Miller recounts an amusing instance in which the medical interns he was studying lost their fear that he was spying for hospital administrators when one of those administrators indirectly accused him of being the intern who was allegedly stealing food from patient's trays. Miller earned the accusation by being of a size that made the accusation plausible, and the incident convinced interns that none of their superiors knew him and that he was therefore harmless.15 (I consider a second cure later.)

When a field worker convinces the people he studies that what he sees will have no further consequences, it paradoxically has the opposite effect

15. Stephen J. Miller, Prescription for Leadership (Chicago: Aldine Publishing Co., 1970). Morris Zeiditch, Jr., has pointed out to me that this is a special instance of a more general situation in which different groups or participants keep things secret from one another, so that secrets are kept from the researcher not because he may inform the outside world but because he may inform other factions or segments.
that the same success has in more controlled research situation. There, the
more that people believe it makes no difference what the observer sees them
do or say, the more open they are to being influenced by him; in field work,
the more that people believe the researcher is unimportant, the freer they
are to respond to the other pressing constraints that surround them.

The general principle, then, is that research subjects respond most to the
things in the research situation that seem most important to them. If you,
the researcher, are most important—either because you have carefully in-
sured that nothing more important will intrude or because they fear that your
findings will become known to people who can affect their fate—your data
will reflect that importance, as subjects politely shape their response to the
cues you give them of what you need to be told or shown. If you are less
important or not important at all from their point of view, they will do
what they might have done were you not there. Field work data tend to ap­
proach the latter pole, experimental and survey data the former; but those
connections, while not accidental, are not uniform, so that field workers
need to be cautious while users of more structured methods can take ad­
vantage of the comparison to devise appropriate safeguards.

My second remark concerns the differences between data secured from
people in the field when they are in the company of others and when they
are alone with the field worker. Observers report that people say one thing
and do another, or say one thing in one setting and something else in an­
other. In particular, they may voice a "public" opinion in public, whether
they act on it or not, and quite another opinion when they speak privately
to the researcher and indicate a disbelief in the common culture. Gorden,
for example, showed that the attitudes toward Russia of the members of a
cooperative college living unit varied depending on whether they were ex­
pressed in group discussions with other members, in private interviews, or
in a supposedly anonymous questionnaire; the more public the situation,
the more the person conformed to the house "line."16

We would err if we interpreted one or the other of these expressions as
the "real" one, dismissing the other as mere cover-up. I take the liberty of
reproducing here an earlier discussion of this point, which arose from my
experience studying medical students. It indicates when and how the ob­
server can note or even provoke these variations, and how they may be inter­
preted.17

16. Raymond L. Gorden, "Interaction Between Attitude and the Definition
of the Situation in the Expression of Opinion," American Sociological Review 17 (1952),
59-58.
17. The following material first appeared, in slightly different form, as Howard S.
Becker, "Interviewing Medical Students," American Journal of Sociology 62 (1956),
198-201. The study of medical students referred to was eventually reported in Becker
et al., Boys in White, op cit.

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The values of any social group are an ideal actual behavior may some­
times approximate but seldom fully embodies. To deal with the tension be­
tween ideal and reality conceptually, there are two possible polar attitudes
toward values. Individuals may be idealistic, accepting the values warmly
and wholeheartedly, feeling that everyone can and should live up to them
and that they are both "right" and "practical." Or they may be cynical, con­
ceiving the values as impossibly impractical and incapable of being lived up
and that they are both "right" and "practical." Or they may be cynical, con­
ceiving the values as impossibly impractical and incapable of being lived up
to; they may feel that anyone who accepts these values wholeheartedly de­
seves himself and that one must compromise in meeting the exigencies of
daily life.

Probably most commonly, individuals feel both ways about the values
of their group at the same time; or one way in some situations, the other
way in others. In which of these moods do they respond to the interviewer
seeking sociological information? Or to turn attention to the interviewer
himself: Which of these is he looking for in the people he talks to? Which
response does he want to elicit?

Sociologists have had a penchant for the exposed since the days of muck­
raking. The interviewer is typically out to get "the real story" he conceives
to be lying hidden beneath the platitudes of any group and discounts heav­
y any expressions of the "official" ideology. The search for the informal or­
ganization of a group reflects this, and Merton's dictum that sociology's dis­
tinctive contribution lies in the discovery and analysis of latent rather than
manifest functions is a theoretical statement of this position.18

The interviewer must always remember that cynicism may underlie a
perfunctory idealism. In many situations, interviewees perceive him as a
potentially dangerous person and, fearing he will discover secrets better kept
from the outside world, resort to the "official line" to keep his inquisitiveness
politely at bay. The interviewer may circumvent such tactics by affecting
the interviewee is lulled into believing that the
former accepts his own publicly disreputable view of things, or by confront­
ing him with the evidence of his own words or reported deeds which do not
jibe with the views he has presented. There may, perhaps, be other ways, for
this area has not been well explored.

Convinced that idealistic talk is probably not sincere but merely a cover-up
for less respectable cynicism, the interviewer strives to get beneath it to the
"real thing." If he is using a schedule, he may be instructed or feel it necessary
to use a "probe." An interview is frequently judged successful precisely to the
degree that it elicits cynical rather than idealistic attitudes. A person inter­
viewing married couples with an eye to assessing their adjustment would prob­
ably place less credence in an interview in which both partners insisted that
their was the perfect marriage than he would in one in which he was told
the honeymoon is over."

Important and justified as is the interviewer's preoccupation with the prob-

18. Robert K. Merton, Social Theory and Social Structure (Glencoe: Free Press,
1949), 68.
I describe this procedure below.

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My data give a quite different picture from that arrived at by our hypothetical "idealistic" researcher. I finally became aware that I had been systematically underestimating the idealism of the men I was studying by finding evidences of it in my own field notes. Some men made almost continual implicit reference, in their comments about practicing physicians they had seen at work, to an extremely high and "impractical" standard of medical practice best typified by their clinical teachers. Others went to great lengths to acquire knowledge on specific topics required neither by their immediate practical interests as students nor by the more long-range material interests related to their medical futures. Particular patients seen on the hospital wards typified certain difficult dilemmas of medical idealism, and, faced with a concrete example, some students brought up their own heavily idealistic worries about what they might do if confronted with a similar dilemma when they became doctors.

Seeing this, I began deliberately encouraging the expression of such thoughts. I spent more time with students engaged in activities carried out alone, raising questions in a sympathetic fashion quite different from the manner I used in groups. I kidded them less, asked interestingly about topics in which they had an "impractical" interest, and so on. Not every student displayed strong "idealism"; a few, indeed, did not respond idealistically at all, no matter how hard I searched or what situations I attempted to search in. But I had now looked; if I missed it where it was in fact present, it was not because my own actions suppressed its expression.

In the long run, I got both kinds of data on students. I had long enough contact with them to get by another means the idealism I missed at first and so ended with a picture of them which included both aspects of their selves. The technical moral to be drawn is perhaps that one should assume that people have both varieties of feelings about the values underlying the social relationships under study and be aware of and consciously manipulate those elements of role and situation which give promise of eliciting one sentiment or the other.

The technical moral forces a theoretical moral as well. We may assume too readily that the people we study will be easily classified as "attitude types" and that they will be more or less consistent in their view of things germane to our study. It is, after all, such a theoretical assumption that accounts for the exposé, with its emphasis on uncovering the "real" attitudes, as well as for the opposite "Pollyanna" attitude, with its unquestioning belief that people are as good as they say they are. It may be more useful to start with the hypothesis that people may entertain such attitude, at one time or another, and let this notion inform a more flexible interviewing style.

Rich Data

We often say that field work data are "rich," meaning thereby to find some saving grace in our failure to gather them systematically or use precise measurements. We think of rich data as containing great detail and speci-
licity about the events studied, as much as a historian might want were he interested in the same events. The adjective also suggests that, like a sauce, it may be too much of a good thing, more than anyone needs or can put to good use.

But the rich detailed data produced by field work have an important use. They counter the twin dangers of respondent duplicity and observer bias by making it difficult for respondents to produce data that uniformly support a mistaken conclusion, just as they make it difficult for the observer to restrict his observations so that he sees only what supports his prejudices and expectations. I have already suggested that ordinarily we observe people subject to all the structural constraints of everyday life, so that they cannot mold their actions to please us. But suppose now that an occasion arises in the field, as it sometimes does, in which people are momentarily freed from those constraints. And suppose that at just such a moment the observer appears, makes his observations, and departs before things get back to normal. Observer bias could then influence what the observer sees, for the subjects of his study would be free to respond to his cues. If the observer observed only on such occasions, or on a few occasions that happened to be of this kind, he would be in the position of the experimenter who makes a limited series of observations when his subjects are free of external constraint.

But the field worker typically gathers his data over an extended period of time, in a variety of situations, using several ways of getting at the questions he is interested in, all of these reducing the danger of bias. Because he observes over a long time, he finds it hard to ignore the mass of information supporting an appropriate hypothesis he may have expected or desired, just as the people he studies would find it hard, if they wanted to deceive him, to manipulate such a mass of impressions in order to affect his assessment of the situation. Because he does not constrain himself with inflexible and detailed procedural rules, he can use a variety of devices to elicit talk and action from his subjects.

In short, the very large number of observations and kinds of data an observer can collect, and the resulting possibility of experimenting with a variety of procedures for collecting them, means that his final conclusions can be tested more often and in more ways than is common in other forms of research. We therefore act correctly when we place great reliance on field work evidence.

Numerous Observations. Field workers ordinarily spend a long time gathering their data. Students of a community usually reckon their stay in years: Whyte spent four years in Cornerville, Gans spent two years in Levittown, Suttles three years on Chicago's Near West Side, all of them living in the area so that their observations went on twenty-four hours a day.20

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Students of organizations spend somewhat less time, the decrease often resulting from the simple fact that people do not live in a factory or school, from the use of multiple observers, or both. Still, Dalton21 spent several years collecting material on industrial organization; my colleagues and I spent three-plus man-years studying a medical school and more than seven man-years studying an undergraduate college.22 Field work reports sometimes attempts to convey the bulk of the data gathered by announcing the number of pages of field notes eventually subjected to analysis; in large studies the figure can run well into the thousands.

All these numbers serve simply to indicate that there is a sufficiency of data gathered over a substantial period of time. Any conclusion based on these data has therefore been subjected to hundreds and thousands of tests. Not only has the observer seen many actions and heard many statements that support his conclusion, but he has seen and heard many, many more actions and statements that serve as evidence negating alternative likely hypotheses. Thus, we not only hear college students talk about the importance of grades and saw them do things that reflected that importance, we also saw and heard things that indicated that they did not use other likely alternative perspectives, such as a liberal arts or a vocational perspective. Perhaps more important, we failed to hear and failed to see those things that would have signaled the existence and importance of alternative perspectives—all the variety of interconnected ideas and actions that might have constituted a liberal arts perspective, for instance—and that failure occurred over and over, day in and day out, through our entire time in the field. (The failure finds its evidentiary use in the field worker's consideration of the number and explanation of items of negative evidence.)23

Insofar as many items in those data support the same conclusion, one can dismiss some other important objections to research results. For example, a college student might say, in a moment of pique, that he did not care what happened in any of his courses or what his grades would be. If we have only one expression of his perspective, the one expressed during that moment, we would properly worry about how accurately it represented the perspective he used day in and day out through the school year. If we have thirty expressions of varying kinds—talk, actions, things done or said privately, things done and said in the company of others—that reveal the same perspective, we worry less about this threat to the validity of our conclusion.

22. Becker et al., Boys in White, op. cit.; and Making the Grade, op. cit.
23. The example is based on Making the Grade, op. cit., especially pages 76-79 and 121-128.
In general, multiple observations convince us that our conclusion is not based on some momentary or fleeting expression of the people we study, subject to ephemeral and unusual circumstances.

Similarly, the circumstances surrounding people's actions sometimes change according to a regular temporal schedule: college students take examinations at the end of the quarter or semester, industries have busy times and slow times, and so on. People may not be aware of the temporality of their behavior, but the researcher must be, for data gathered at different times reflect different realities. The nursing students Davis and Olesen studied had a new notion about school, their profession, and their careers after they returned from their first vacation, which revealed to them just how isolated from the world of men and marriage they now were. If one used pre-vacation data only, he would implicitly assume that student perspectives did not vary meaningfully over time; having both allows you to see that they may be occurring.


25. For a discussion of the utility of observations over a period of time, see Zachary Gusow, "The Observer-Observed Relationship as Information About Structure in Small-Group Research," Psychiatry 27 (August 1964), 230-247.

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cause of eligibility rules and other factors, likewise take student grades into account in making decisions about their own activities. Each aspect of the collective action that makes up the life of a college campus has connections to the other parts, and an emphasis on grades usually forms one of the major connections.

Suppose that, for whatever reason, some students wish to make a field worker believe that they do not place much importance on grades, although in fact they do. They tell him they don't care about grades and even spend the evening they might otherwise have spent studying drinking beer with him as though they hadn't anything else to do. If he talks with them casually over several hours, they will find it necessary to lie about many other things: how they recruit members of their fraternity, what they do with their time, how active they have been in campus politics, and all the other matters where they base their day-to-day activity on the premise they now wish to deny, that grades are in fact important to them. They can lie about all these things, but it is a difficult job requiring a quick mind and intense concentration; one must see the possible ramifications of every remark and tailor what is said to take account of them.

26. If the students know that the observer has not come just for the evening but proposes to spend the next year observing them and their kind, they can easily see that he will soon discover that they have been lying to him. Say what they will, he will come by some night to drink beer only to be told that they must study for an exam; if he asks why, they will have to say that they need better grades to stay in school (or because they want to get into campus politics or law school or whatever). Furthermore, other people will probably describe to him a system in which their position will seem bizarre and unusual, so that he will be back with more questions. Eventually, he will uncover their lies, in their further words, in their deeds, and in the words and deeds of others.

People can, of course, construct Potemkin Villages for inspection by outsiders such as sociologists, in which all these problems have been analyzed and social life so arranged that exactly the desired impression emanates from it and nothing else. But Potemkin Village must have for its primary business fooling outsiders, for everything that would belie the intended impression must be suppressed at the risk of defeating the goal of the entire enterprise. The communities, schools, and factories we study, however, always have some other primary business. They are places to live, places where people try to teach other people something, places where goods are supposed to be produced. That other business creates the external constraints—the necessity of adjusting to other members of the community or organization and to important others elsewhere—that make it impossible
for our subjects to put on a continual show for us. Though fooling us may, on occasion, become quite important, it is never the first and only order of business.

If, then, we make numerous and lengthy observations over a long period of time, we will see, if not everything, most things, and will be able to make some pretty good guesses about the rest. Furthermore, as our intention to do this becomes known, people will see they cannot hide things from us forever without paying some very large price in organizational and personal efficiency. They foresee that they will eventually be found out and then be revealed as having been uncivil enough to have lied or dissembled earlier. Some people will not mind being thought uncivil, but very many people do mind. We may say, in general, that (given an interconnectedness of activities of the kind described and a sensitivity to notions of civility) numerous observations give us good reason to suppose that little will have been hidden from the observer; and therefore that his conclusions are warranted.

For much the same reasons, many observations made over a substantial period of time helps the observer guard against his own conscious or unconscious biases, against “seeing only what he wants to see.” For it is equally difficult to lie to yourself. Contradictory evidence appears and it appears not in subtle forms but in very gross ways. The more observations one makes and the more different kinds of observations one makes, the more difficult it becomes to ignore or explain away evidence that runs counter to one’s expectation or bias. One has the same problem of making a coherent picture of what he sees that the person studied has when he confronts the observer.

We often conceive of observer bias as a subtle process, involving a failure to attend to subtle cues, an unconscious ignoring of unemphasized remarks and events, an unintentional distortion of ambiguous or equivocal stimuli. But the major features of a social organization, and their ramifications and interconnections, have no such subtle and equivocal character. The college students we observed did not remark casually and ambiguously on their interest in grades; they talked about it much of the time, placed great emphasis on the point, explained much of what they did with reference to the grading system, and in general kept it before us constantly. Had we observed and talked with them only a few times we might have been able to ignore the point. But one could not ignore or fail to record a matter students paraded so incessantly and even obsessively without consciously cheating. (Cheating, however, is a problem that affects the validity of data gathered by all research methods and I do not consider it here.)

In addition, the interconnectedness of an organization’s parts already referred to means that our attempt to understand any particular aspect of what we see probably requires us to have some grasp of its other major aspects. The observations we record require us to attend to other observables, however unexpected or distasteful to us, in order to make sense of them.

In general, then, by making numerous observations we confront ourselves with the major features of the collective activity we study in a gross and repeated way such that it is unlikely that we will unconsciously avoid recording some important matters. For this reason, we correctly place confidence in the field worker’s evidence.

Flexible procedures. The field worker, because he has continuing contact with those he studies, can gather data from them by multiple procedures, in several settings and in many moods. This variety allows him to crosscheck his conclusions and retest them repeatedly, so that he may be sure his data are not an artifact of some one way of proceeding or some one situation or relationship. He is not limited to what can be gathered in one interview (even if it is eight hours long!) nor is he limited in what he asks about by his knowledge and understanding as of that time; since he can interview repeatedly, he can inquire about different matters on different occasions. He can change his relation to people, dealing with them differently as they come to know one another better. He can take chances with words and actions that may annoy or anger people, because he knows he will probably have a chance to repair the damage.

I want to comment on three of the possibilities created by this flexibility of procedures for gathering evidence to test research conclusions: (1) using unconventional measures suggested by experience in the situation; (2) making evidentiary use of one’s own experience; and (3) using aggressive and deceitful styles of interviewing to provoke people into saying things they might otherwise keep to themselves. The availability of such procedures gives us further reason to trust conclusions based in field work.

(1) Sociologists treat concrete items of data as instances of general theoretical classes, as an embodiment of some abstractly conceived variable most conveniently measured in that way. When we use standardized items of data to measure those abstract variables, we assume that the specific concrete datum we measure will serve as an adequate embodiment across a variety of times, places, and people. If we ask for father’s occupation, intending the datum to indicate something about parental social class, we assume that it has roughly the same relationship to social class in one time and place as in another, and that the people we ask will all understand roughly the same thing by the question. Naturally, these assumptions sometimes fail, but we put faith in them anyway, as a way of achieving convenience and

comparability. Our faith accounts for the persistent use of the variety of scales and items that form the standard tools of research.  

We can, however, take another approach to the problem of finding embodiments of our theoretically defined variables. We can look for the specific local variants, the way that variable finds expression under all the local and peculiar characteristics of the immediate situation. This procedure makes comparability somewhat more complicated to achieve, but it maximizes the fit between concept and datum. Roth, for example, wanted to study how people complied with institutional rules. Instead of using some general measure of compliance, or some measure specific to the medical settings he was particularly interested in, he noted that in the tuberculosis hospital he was observing people sometimes obeyed and sometimes violated strictly formulated rules about wearing protective masks and gowns. He counted the occasions on which various categories of hospital personnel wore or did not wear protective garb and thus found evidence that compliance is inversely related to rank: doctors conformed least, aides most.

The field work literature contains many examples of such locally restricted measures of abstract variables. Thus Blau measured patterns of influence and deference by observing the frequency with which coworkers asked advice of each other. Whyte used seating patterns as a measure of clique structure. My colleagues and I used a variety of items to demonstrate the existence among college students of a "grade point average" perspective on their academic work: the kinds of questions students asked in class, their methods of studying (both individual and collective), patterns of residential group prestige, and so on.

Each such newly proposed measure must be explained and justified, a bother compared with the ease of using well-known standardized measures. But an ingenious field worker can usually invent several useful ones and thus make it easy to adopt multimethod triangulation as a way of warranting his conclusions, a substantial gain well worth the bother.

27. See the attack on this kind of "measurement by fiat" in Aaron V. Cicourel, Method and Measurement in Sociology (New York: Free Press, 1964).
30. Whyte, op. cit. See also Becker et al., Boys in White, op. cit.
31. Becker et al., Making the Grade, op. cit.

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(2) The field worker can sometimes take advantage of his presence in the situation to produce evidence based on his own experiences. Most obviously, we can make a datum of the way the people we study respond to us as observers. Gussow reports using his varying reception in several schools as a way of understanding their structure. Teachers in a traditional school, for example, were less aware of which child he was observing than teachers in a modern school (the difference reflecting their relative indifference to children's individuality) and more interested in using the researcher as an authority side (the difference reflecting their greater concern over hierarchy and control).

We can also, if we actually engage in the same activities as the people we study, make use of our responses to the events, tasks, and troubles of that style of life. In a remarkable table, Roy used his own production record in a machine shop to demonstrate the amount workers restricted production, and the degree to which they lowered their own earnings in the process.  

(3) Interviewers frequently inhibit themselves by adopting a bland, polite style of conversation designed to create rapport with their respondents and to avoid "leading" them. A more flexible procedure often elicits much fuller data, as the interviewer himself takes positions on some issues and as he uses more aggressive conversational tactics. This flexibility can also characterize the one-shot interview, but it is probably true that you feel freer to use it when you are working with people over a period of time and can make use of their knowing that you know a good deal about what is going on. (On the other hand, field workers sometimes worry more over souring a relationship that must persist than do interviewers who will not see their respondent again.)

I once again take the liberty of reproducing, as an extended example of this point, an earlier discussion based on my own study of schoolteachers.

Arnold Rose once proposed that sociological interviewers be more experimental in dealing with their informants. He pointed out that use of the questionnaire or schedule is appropriate only in certain research situations, particularly where information as to the nature of a given attitude is desired. However, where information as to the nature of a given attitude is desired, "where the subject's attitude must be fully known . . . ," the interviewer must take an active role. The interviewer

33. Gussow, op. cit.
34. Donald Roy, "Quota Restriction and Goldbricking in a Machine Shop," American Journal of Sociology 57 (March 1957), 427-442.
35. The following material first appeared, in slightly different form, as Howard S. Becker, "A Note on Interviewing Tactics," Human Organization 12 (Winter 1954), 31-32.
must experiment, using those tactics which seem most likely to elicit the desired kind of information. This note presents tactics that proved successful in a study designed to get information about the role problems of Chicago public school teachers.37

Chicago public school teachers, like functionaries of many institutions, feel that they have a good deal to hide from what they regard as a prying, misunderstanding, and potentially dangerous public. They have certain problems whose existence, if admitted, would provoke unfavorable public comment. Further, they are afraid to make statements about their superiors and colleagues which might get them into trouble with and provoke retaliation from those people. This makes interviewing around the basic relationships of a teacher's role difficult. Fear prevents them from being frank and giving an undistorted picture of the reality as they know it. To overcome this, I developed certain techniques in the course of the research which enabled me to get franker statements than would normally be forthcoming. These can best be described in the context of particular problems in connection with which they were used.

The interview ordinarily started with questions at a high level of generality: "What are the problems of being a schoolteacher? What kinds of things can make your work tough or unpleasant?" Most teachers could talk about these relationships at this abstract level of discussion; they could say that a principal might make their work difficult by being too "bossy" and interfering, that parents might get "out of their place" in a variety of ways, things they would have been reluctant to state at the outset about specific persons. When a number of such statements had been made and we were well launched on our conversation, I would assume a skeptical air and ask the teacher if she could give me any evidence for these statements, in the form of examples from her own experience. (Obviously, one cannot build up a description of a social structure from such general statements alone; material of a more specific nature is needed to check the way these general attitudes are expressed in behavior.) This somewhat suddenly put the interviewee in the position of having to put up or shut up, substantiate what she said or admit that it was only hearsay. In most cases these abstract statements were generalized from experiences the teacher had had and, in the face of a direct challenge, she ordinarily came across with descriptions of specific situations in which these generalities were embodied.

Once the interview area had been shifted in this way to personal experience, I used another strategy to elicit further information that was being withheld. I played dumb and pretended not to understand certain relationships and attitudes which were implicit in the description the teacher gave, but which she preferred not to state openly. In so doing, I forced her to state these things in order to present a coherent description. For example, these teachers ordinarily differentiate students, among themselves and to themselves, according to racial and social-class criteria, on the basis of observed differences in the way children of each type act in school. They prefer not to say this in any public way, particularly to a person coming from the University of Chicago as I did,38 for fear of being accused of discriminatory and undemocratic thinking and activity. I was extremely interested in getting attitudes around just this point. To take a particular instance, in describing a given experience with a principal, the notion of such differences would be implicit in what was being said, and it was part of the unspoken etiquette of the situation that I was to accept this implication without stating it. I would refuse to do this, however, and play dumb. If, for instance, I was told that she had taught in a "colored" school where the principal wasn't tough enough with the children, I was meant to understand that such children needed firmer treatment than others. But I refused to understand this and would say, "Why did he have to be tough? What do you mean?" To make her judgment of the principal's action plausible and reasonable, the teacher then had to explain to me that it was unfortunately true that colored children seemed to misbehave more than the other varieties. The same tactic was used in a variety of contexts.

By continually moving the area of discussion to the level of concrete personal experience, and by playing dumb about the implied descriptions of relationships involved in accounts of such experiences, I coerced many interviewees into being considerably more frank than they had originally intended. I was quite aggressive, often expressing open disbelief in the face of statements that seemed evasive, implausible, or inconsistent with what had already been said or with my general knowledge of the particular topic, and equally open curiosity about the things that were left out. It is certain that such tactics, used in just this way, would not work with all kinds of people. Some of the success attained with schoolteachers must be attributed to the professional politeness and courtesy they felt obliged to extend to me. Once the interview had gotten under way and the teacher had committed herself to accepting me and my questions, she found it difficult to be so insulting as abruptly to refuse to discuss certain issues or to make statements she knew would appear implausible or inconsistent to me. Where failure to tell the truth about her feelings made her statements obviously false or evasive she found it necessary, in the logic of the developing situation, to tell the truth in order to avoid being unpleasant to me.39

Such tactics will not prove effective in all situations, nor would one want to use them indiscriminately. Where, for example, your research places you in continuous contact with those being studied, as in a long-term community study, it might be wisest to avoid the possibility of antagonizing informants which lies in this stratagem, especially since the information which might be elicited could undoubtedly be picked up more tactfully in the course of an

37. The research is reported in Chapters 9-11 of this volume.

38. This research constituted my dissertation at the University of Chicago and I identified myself to teachers as a graduate student at the University and an employee of its Committee on Race Relations.

39. I relied here on the tenet of everyday civility described earlier, according to which it is uncivil to be openly implausible or evasive.
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extended series of interviews and observations. Further, not all interviewer-informant relationships carry such a ready-made bond of courtesy as the one here described, and many informants would undoubtedly simply ignore the situational pressure to be consistent plausible. I believe, however, that similar pressures to which informants of diverse kinds will be sensitive can be built up if the interviewer is willing to experiment. Finally, the situation can be complicated, and often is, by the fact of the informant being of higher social-class position that the interviewer. The unspoken etiquette of such a relationship leaves the informant free to be rude through evasiveness or implausibility, free to ignore the demands of a questioner who is stepping out of the confines of his deference role.40

Field workers experimenting with this device may find ways to adapt it for use in the more difficult situations, ways of creating a bond between interviewer and informant of such a character that the informant can be coerced into stating things he would otherwise leave unsaid.

Conclusion

We ought not to decide that only field work can provide trustworthy evidence for sociological conclusions. Many people have argued cogently that we should use methods appropriate to the form of our problem and to the character of the world we are studying.41 Where field work is the appropriate method, however, the burden of my argument has been that we can use the evidence it produces without undue worry. Because it gives us information on people acting under the very social constraints whose operation we are interested in, and because its numerous items of information and flexible procedures allow us to test our conclusions repeatedly and in a variety of ways, we need not fear that its unsystematic character will distort our findings in ways that we, our readers, or the people we study find convenient, congenial, or expectable.

CHAPTER FOUR

The Life History
and the Scientific Mosaic

Thomas and Znaniecki published the first sociological life history document to receive wide attention in The Polish Peasant.1 Clifford Shaw and his associates published several others in the years following: The Jack-Roller, The Natural History of a Delinquent Career, and Brothers in Crime. During the same period Edwin Sutherland published the still popular Professional Thief. And similar documents have appeared occasionally since, most recently The Fantastic Lodge and Hustler.2 When The Jack-Roller was reissued a few years ago, I was asked to write an introduction and made that the occasion for some thoughts on the place of the life history in contemporary sociology.

The life history is not conventional social science “data,” although it has some of the features of that kind of fact, being an attempt to gather material useful in the formulation of general sociological theory. Nor is it a conventional autobiography, although it shares with autobiography its narrative form, its first-person point of view and its frankly subjective


2. Clifford R. Shaw, The Jack-Roller (Chicago, 1930), The Natural History of a Delinquent Career (Chicago, 1931), and Brothers in Crime (Chicago, 1936); Chic Conwell and Edwin H. Sutherland, The Professional Thief (Chicago, 1937); Helen MacGill Hughes (ed.), The Fantastic Lodge (Boston, 1961); Henry Williamson, Hustler, edited by R. Lincoln Keiser (Garden City, N.Y., 1965).

40. Erving Goffman pointed out this possibility to me.