

## Teaching Participant Observation

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As a technique of research, participant observation distinguishes itself by breaking down the barriers between observer and participant, between those who study and those who are studied. It shatters the glass box from which sociologists observe the world and puts them temporarily at the mercy of their subjects. Instead of watching respondents through two-way mirrors, reconstructing them from the traces they leave in archives, analyzing their responses to telephone interviews, or reducing them to demographic data points, the ethnographer confronts participants in their corporeal reality, in their concrete existence, in *their* time and space.

Conventional social science privileges its knowledge by first separating the observer from the participant and then placing the observer above the participant. It divides society into two parts, one of which is superior to the other. The subject becomes an object, a dupe of social forces, whereas the scientist lies beyond social determinism, exercising autonomy and rationality. Ethnographers challenge this bifurcation when they insist that they share a common world with those they study, when they believe that participant and observer are commensurate though not identical entities, and when they recognize that both sides have theories about the other as well as about themselves. Instead of standing above society, contemporary ethnographers veer toward absorption into the society. They are more likely to forsake the authority of science and enter into dialogue with their subjects. Ethnography becomes a collaborative enterprise of participant and observer.

### THE POWER OF PEDAGOGY

This view of participant observation as a collaborative enterprise has its counterpart in teaching. Instead of treating students as empty vessels waiting to be filled with knowledge ("here is the truth, swallow it") and instead of students regarding themselves as passive recipients of pedagogic wisdom ("just tell me what I've got to know"), we can try to construct learning as a mutual relationship between educator and the educated. This is a noble goal, but one that is neither easily pursued nor ever completely successful. Mutuality between graduate student and teacher depends on disrupting the structures of micropower. In a doctoral program discipline is exercised all the more ubiquitously and punitively because the authority of knowledge is so flimsy. Impressive syllabuses, impossible reading assignments, grading, interminable examinations, and letters of evaluation as well as the parade of experience and status turn education into a display of power.

How far can one go in disrupting the institutional foundations of a professor's claim to the monopoly of knowledge without undermining teaching altogether? How is it possible to institutionalize pedagogic ignorance, not just proclaim it? To break with an authoritarian order from above, an order, moreover, endowed with legitimacy, one must perhaps create a compulsory autonomy from below. This, at any rate, is how I have designed the structure of my seminars in participant observation. By sending students out into the field to undertake their own projects, I sought to endow them with a minimal independence. I insisted, as a condition of participation, that within one week they had to be in the field, writing field notes. They were now the ones with a monopoly of knowledge, at least on the topic of their own research. After a minimum of reading about ethnography in the first two weeks, we devoted the remainder of the semester to discussing each of the twelve studies in turn. We would learn through active participation and observation both in the seminar and in the field, that is, through doing and confronting the doing of others. Rather than working in isolation, students would participate in and learn from their classmates' projects.

With an independent base, a realm of unquestioned expertise, each student could somewhat confidently launch into a discussion with the others. And because they were all feeling similar tensions and frustrations, they could share those experiences. Precisely because I was not doing field research and did not have any privileged knowledge about their sites, my own participation in the seminars rapidly diminished to the point that I became embarrassed to even summarize the discussions.<sup>1</sup> In teaching, ignorance can be a virtue. Not just my own ignorance but that of eleven classmates compelled each student to translate

his or her experiences into a meaningful sociological account. Each student had to convince me and the entire class that something interesting was going on at the field site.

Students were on their own, but they also relied on each other as well as me to help them through the crises that continually erupted both in the field and in their attempts to make sense of the field. If presentations fell apart, and they often did, anxiety beset us all. We were all implicated in each study from the beginning, and we mobilized our energies to recover the broken pieces. As the seminar unfolded and the projects began to take shape, so the students also gained more self-confidence and experience in helping others. The development of each became the condition for the development of all.

But let's not romanticize these dialogues in the field and in the seminar. To be sure there was collaboration, but who set the terms? In the seminar, just as in the field, students had to accept strict ground rules not of their making. I demanded that they be in the field each week and that they submit at least five sets of field notes to me. I expected them to attend every meeting of the seminar, to come prepared to comment on the work of others, and on two occasions to distribute their own field notes and analysis for discussion. I expected a proposal or draft of the final paper halfway through the semester that would situate the study in some literature. There were no incompletes—the final paper had to be delivered by the end of the semester. I was demanding that they do the impossible: complete an ethnography in three months.

These ground rules had repercussions in the field. The intense schedule led students to choose sites with relative ease of access and where they were likely to be comfortable. So they tended to select groups with whom they had some sympathy, the underdogs rather than the overdogs. But the relative ease of entry created unexpected tensions when students found themselves torn between competing loyalties to the seminar and the field. The roles of participant and observer are inherently in conflict, and tension and anxiety are an intrinsic part of field work, I reassured them. Other things being equal, I advised, the greater the tension the better the product.

### FIELD WORK AND ITS ANALYSIS

Their anxiety was compounded by their inexperience. Only one student had ever completed a participant observation study before. They didn't know what to expect. Tensions crystallized around field notes: What are they? How much do you write? How often? About what? I tried to counter this fear of the unknown and to demystify field work by distributing some of my own field notes, the first set from the study

that became my doctoral dissertation and subsequently the book *Manufacturing Consent*. I know sociologists who write wonderful field notes, rich in observation, deep in insight, comprehensive in coverage—but I am not one of them. Students were surprised by my display of vulnerability in showing them my primitive notes. At the same time they were encouraged because they could see even in these tentative first notes of mine the outline of the book that emerged several years later. This was not going to be so hard after all.

But it *was* hard. I would not accept any field notes without analysis—a commentary on the significance of what they had experienced and observed. I wanted them to figure out what was interesting and important about their field observation right from the beginning. Before they could even enroll in the course, I demanded a short “prospectus” that described what they expected to find in their field site *before* they entered. Inevitably, what they actually found would violate their expectations, forcing them to ask why they had been so wrong. It is after all so easy to “normalize” the field, to see what is as natural and inevitable. The most difficult part of the ethnographic enterprise is to make the data sound abnormal, sound surprising. One has to make the reader say, “Wow! That’s interesting, I wonder why?” How students dragged their feet when it came to analysis!<sup>2</sup>

There is probably a trade-off here. Those who find field work most trying often feel more at home analyzing their material than those who manage to cope more effectively with the tensions of the field. The appeal of participant observation usually lies in the integrity it gives to those one studies. Therefore, students resisted violating the authenticity of their subjects by locating them within some explanatory framework. They preferred to think that the accumulation of data would by itself miraculously turn into sociology, that the final paper would be conceived immaculately during examination week.

Even though we read Howard Becker’s *Writing for Social Scientists*, students found it difficult to stomach my insistence that the writing of a major paper begins in the first week, that it is not something hurriedly put together at the end of the semester when all the data are in. Field work and writing proceed together as a process of continual reconstruction of the past in the light of the present with a view to anticipating the future. From the very beginning the field challenges our preconceptions, forcing us to reconstruct our images, our theories, and even what constitutes our questions. Initially, our understanding of what is going on—what is interesting—may oscillate wildly, but over time the oscillations diminish (if all goes well) as we converge toward a stable interpretation.

Field work becomes a series of self-conscious experiments or interventions in which one first tests out and then reformulates one’s hypotheses. It begins with what is often the most dramatic intervention, the initial entry itself. At this point how participants regard the observer reveals more than how the observer regards the participant. When Ann Ferguson introduced herself to the Wholly Grains Bakery, one of the members chuckled, “So you want to observe us, just like Margaret Mead . . . the sexual life of the baking collective.” Leslie Salzinger, on the other hand, was an object of suspicion. Was she stealing ideas to give to a competing agency? Subjects’ obstacles and resistances to being studied had to be carefully scrutinized, although these were often difficult to interpret before substantial field work had been completed. Similarly, moments of crisis, generated by oneself or others, reveal the true interests of the participants, moving us toward new refutations and conjectures.

#### OBSERVING THE OBSERVERS

The seminar itself was like any other field site with its own taken-for-granted culture, its own rituals and jokes, and its own crises and interventions. Perhaps it was different from the usual seminar, for it did not establish its content through agendas, sets of questions, or reading lists. It had an uncertain future, mirroring the flux in the field. To remind us that as scientists we were not outside or superior to society, that as observers we were not above being observed, I wanted someone to study the seminar. Charlie Kurzman volunteered. It was a thankless task since, unlike the others, he did not have a monopoly of knowledge. We knew as much about the seminar as he, or so we liked to think. Whether he desired it or not, we would evaluate his paper in the light of how we experienced the seminar. Like the others, he had to distribute his field notes, make a presentation, and give us his final paper. He was in an unusually weak position with respect to his subjects, who were friends and classmates. They were people with whom he spent time outside the seminar and who would (hopefully) be his colleagues for years to come. He could not afford to offend either them or me. But offend us he had to if he was going to say anything interesting, anything sociologically meaningful.

The turning point came when Charlie gave his first presentation. Until then the class had been very supportive of all the projects. That suddenly changed when Charlie revealed how his fellow students identified their subjects as positive and negative deviants, and how those judgments were shaped by professional interests and unexamined po-

litical assumptions. Even ethnographers like to think that their judgments have some objectivity. The class flew at Charlie, accusing him of objectification, insensitivity, distortion, and male bias. He had forced us to confront illusions we had about ourselves; he had challenged our political purity and objectivity. We returned in kind but with an unexpected display of passion and resentment. The session turned into a royal Berkeley cockfight that, like its Balinese model, dramatized cleavage and tension in our midst. By pecking away at Charlie's "knowledge claims," we provided grist for his theoretical mill. As sacrificial cock he suffered, but, as so often happens in participant observation, along with suffering came insight.

While some students unleashed pent-up anxieties about being surveilled, others began to worry about their own field work, what they were doing to *their* subjects. Ann Ferguson was so perturbed that she withdrew from the field. She just couldn't face her fellow workers. If they were to see her field notes, surely they would be as resentful as we were toward Charlie. She returned to the bakery only when one of her fellow workers phoned and asked why she hadn't been to visit.

Charlie's intervention had moved the seminar to a new level of consciousness. He opened up a range of questions concerning the possibilities and dangers of the sort of research that we, in principle, endorsed—research that was responsive to the interests of the participants. Students now had second thoughts about handing back their completed papers to the people they analyzed. Certainly this was an ideal way of validating, developing, and reconstructing our theories, but it also had risks that might not be worth taking. Equalizing power relations by allowing participants to contest one's explanations was fine in principle, but in practice it could turn friends into enemies and so compromise what one wanted to say as to make it worthless.

Each project, therefore, went through a life cycle. In order to gain entry students made commitments to those they were studying. But as the semester unfolded they became increasingly committed to one another at the same time that my demands for sociological significance became more insistent. The balance of power was now shifting from participant to observer. As surveillers we do become visible and subject to countersurveillance, but it is not symmetrical. Sooner or later we retreat behind the university walls, whereas our subjects remain to cope with the situation in which we found them. The very possibility of sociology, of partaking in a sociological community whose concern is to explain the particular as a manifestation of the general or the global, resides in this irreducible asymmetry of power. Only Charlie had no-

where to retreat, and so he did not waste much time rejecting his role as observer as soon as the semester was over.<sup>3</sup>

#### THE EDUCATOR, TOO, MUST BE EDUCATED

In his address to the Eastern Sociological Society in 1969, Everett Hughes took the parallel between teaching and field work in a slightly different direction.<sup>4</sup> He regarded the life experiences of his students as well as their own research projects as material for his own theorizing. Like field work, teaching was a way of interacting with "other" to broaden one's horizons, to move from the particular to the general, to emancipate oneself from parochialism. Hughes had a deep respect for his students—they were not empty vessels but fascinating subjects in their own right. He would question them about their families, their backgrounds, their histories, and how they saw the world. For him field work and teaching were transmission belts of knowledge from participant to observer and from student to teacher. Hughes had no qualms about using his students as informants, just as his students had few doubts about the benefits of ethnographic research. In the pioneering vision of Chicago sociology, knowledge was considered emancipatory.

Today we are more skeptical of the blessings of knowledge and more likely to see knowledge as a vehicle of domination. Counteracting this domination of "expert" over "neophyte" calls for an altered vision of teaching and field work, less as transmission belts of knowledge and more as dialogue. Instead of serving as the instructor's informants, students become participants in a collective process that begins on the first day of class. But that is easier said than done. I remember well how at the first meeting of this seminar, my heart sank as I listened to the vague, incoherent, and tentative proposals. In the seminar I could be a participant much like everyone else, but outside I would play a more directive role: writing comments on people's field notes; incessantly asking "so what?"; cajoling them into formulating a problem or persuading them that they had a problem if only they would recognize it; or directing them to this literature or that. There was mutual irritation, despair, exasperation, and incomprehension.

As the studies took shape, I was being forced to articulate what they were about. I tried again and again to work out the links between the micro and the macro, the particular and the general, and the process of generating anomalies. I was being forced to deepen my own methodological self-consciousness. The group had an extraordinary tenacity. Everyone seemed to be converging on some identifiable theme. Three weeks from the end I could see the dim outline of a possible book. We

agreed to continue into the next semester with a view to rewriting our papers while I would contribute chapters on the extended case method and theory construction.<sup>5</sup>

At this point the enterprise changed from being my project and my seminar to our collective project. Undoubtedly, the papers exhibited some sort of unity around the extended case method because of the power I exercised and the pressure I applied, nonverbally in the class and verbally in my office, as well as through the many sets of comments on field notes, literature reviews, and drafts of papers. But in the second semester, the unity having been more or less established and a common project defined, we were now on a more equal footing.

This is not to say that it was easy for graduate students, who are accustomed to moving from course to course and from paper to paper, to now move from draft to draft. Again there was foot dragging. Revisions appeared at the last minute. In the end it was less my influence and more the collective pressure that got things done—pressure that extended as far as Charlie Kurzman in Turkey studying the Iranian Revolution, Shiori Ui laboring in a Cambodian textile factory, and Leslie Hurst observing classrooms in Sri Lanka.

Although I had been doing participant observation research for twenty years and had run seminars on the topic before, I had never written about it. My contributions to the book were at a much more rudimentary stage than theirs, and I thought that if they saw me struggling to make sense of my own ideas, moving from one draft to the next, they would be encouraged to do the same. I was wrong. Rewriting papers and confronting the limitations of one's own work, particularly at the beginning of one's career, is always painful. That I looked forward to rewriting was seen as a mark of eccentricity but more deeply a measure of my security and self-confidence.

However convivial and collaborative we became, we remained professor and graduate students. Just as in participant observation, so in teaching there is always a fundamental asymmetry. There was no escaping the institutional framework of the university and its inscribed hierarchy. In this context too much responsiveness easily leads to anarchy. It works only if the teacher first establishes the terrain of the seminar, its grounds and its broad goals, and then pursues those goals with unflinching determination. The teacher must still teach. The point is not to strip the teacher of all power, but only of disabling power, that is, power that inhibits the development of individual capacities, both of educator and educated. We must aspire to direction without domination, a balance between autonomy and dependence, a shared process of learning.

#### A MATTER OF GENDER?

How exceptional was this group? I had observed similar dynamics in my other seminars on participant observation, similar engagement and reciprocity. But this one seemed to move that much more smoothly, and I felt less a coercive presence than on previous occasions. There was more mutual engagement, interdependence, and joint endeavor, despite cussedness and obduracy. Unlike previous classes, this group never broke down out of exhaustion, indeed never looked like it was breaking down. I often wondered why. The participants had diverse interests but nevertheless shared a common political culture. There was an inner core that had already established close friendship ties. But all this was not unusual. In terms of career, none of the sociology students had even begun to prepare for their qualifying examinations. That was unusual, but it didn't help me comprehend what was going on. In the end the most plausible explanation is the most obvious one. The earlier seminars had roughly equal numbers of men and women; this one had ten women and two men.

What difference did gender make? Here I can only speculate based on my experiences in other seminars and on well-known theories of gender differences.<sup>6</sup> Certainly it was not a matter of explicitly studying questions raised by feminism, although many of the participants regarded themselves as feminists. Rather I think it was the structure of the seminar, which demanded both autonomy and interdependence, separation and connection. It was sustained from the beginning to the end by students' commitment to their own projects (which involved collaboration with those they were studying) as well as commitment to the success of everyone else's project. It became a genuinely collective enterprise, which raised the level of everyone's contribution. I am obviously not saying that men never excel in this mode or that women always do. I simply think there is a tendency in that direction.

Women are also more accustomed to handling the tensions of moving between spheres, in this case between different field sites and between field and school. This is not to say they find it easier than men. Indeed, they may have more difficulty separating themselves from others with whom they have continuing interaction. It is probably significant, therefore, that in each of the three seminars in which someone has volunteered to study the seminar, it has been a man. On the one hand these volunteers knew they wouldn't have to go into the field and interact with others, while on the other hand, at least initially, they thought they could separate their role as observer from their roles as friend, classmate, and graduate student.<sup>7</sup>

But the structure of the seminar had another feature with which men might have greater difficulty: the presentation of unfinished work, the tentativeness of people's ideas, the tolerance of uncertainty. The seminar was never the arena for students to boast superior erudition and knowledge, to lay claim to some final truth. It was always in flux, in process. It never seemed to end.

## NOTES

### CHAPTER ONE

I thank Gail Kligman and Erik Wright for their helpful comments on this chapter as well as chapters 2 and 13.

1. See Jürgen Habermas, *The Theory of Communicative Action*, vol. 2, *Lifeworld and System*, particularly chaps. 6 and 8. For Habermas the lifeworld is primarily integrated through intersubjective communicative action, but it is not confined to the micro-world as it includes the "public sphere" where politically negotiated meaning occurs. However, our use of the lifeworld is confined to the distorted communicative action in the micro-contexts of everyday life.

2. See Herbert Blumer, *Symbolic Interactionism*, chap. 1, and Leonard Schatzman and Anselm Strauss, *Field Research*, chap. 1. I don't adopt the distinction between "natural" and "unnatural" sociology because I want to stress what distinguishes the social sciences from the natural sciences, namely the interaction of participant and observer. The idea of studying people in their natural setting connotes the zoologist studying animals or the botanist studying plants rather than people studying people.

3. See, for example, John Lofland and Lyn Lofland, *Analyzing Social Settings*, and Schatzman and Strauss, *Field Research*. For an interesting account of the relative merits of participant observation as compared to survey research and experiential analysis, see Shulamit Reinharz, *On Becoming a Social Scientist*. For a collection of standard readings, see George McCall and J. L. Simmons, eds., *Issues in Participant Observation*.

4. Typically participant observation is described as a method that has been surpassed by advanced scientific techniques of research. Morton Hunt, for example, writes of participant observation as one of a number of techniques that "once formerly dominated the field: today, while still used . . . they account for only a small part of total research effort" (*Profiles of Social Research*, p. 6). According to Hunt the mainstays of sociology are now surveys and experimental techniques.