Journeys Through Ethnography

Realistic Accounts of Fieldwork

edited by
Annette Lareau
Temple University

and
Jeffrey Shultz
Beaver College

WestviewPress
A Division of HarperCollinsPublishers
had balancing competing personal and professional responsibilities. At the time, "the classic, exotic, other-cultural experience" was still the standard in the discipline (Messerschmidt 1981:3), and we were sensitive to the potential for devaluation of our work. In the end, having the field be "here" rather than "there" (cf. Geertz 1988) set the stage for the ongoing relationships with the women in the community that have enriched our lives.

Introduction to Chapter 5

Objectivity is often defined as a goal of social scientific research and writing. Researchers are expected to maintain a certain amount of distance or detachment from both the subject matter and the individuals they are studying. In effect, they are expected to remove personal issues and concerns from their work. As we noted earlier, Alma Gottlieb's early field notes (see Chapter 2) were devoid of personal content; she thought that was what her dissertation advisors expected. As a result, the notes were not a resource for writing the chapter in this volume; they lacked the emotional depth that the authors felt was necessary to recapture their first impressions of life in a Beng village.

Susan Krieger faced a similar dilemma as she sat down to write about the research she had done in a lesbian community in the midwestern United States during the late 1970s. Having been a participant in that community prior to being a researcher, she found it difficult to define the appropriate approach to take to her many pages of notes. As she describes, even having the notes laying on the kitchen table day after day (where she was more likely to see them) did not move her into action. Her initial attempts at sorting through and analyzing her data proved to be unproductive and frustrating.

In the chapter that follows, Krieger describes the process she went through. In the end, she found it necessary to reinject her self into the research and to understand her emotions and reactions as she was interviewing members of the community, some of whom she knew personally prior to knowing them as subjects in her study. Her book The Mirror Dance: Identity in a Women's Community ultimately offered a "birds-eye view" of a midwestern lesbian community that while known at one level is frequently "unobtainable" and "that in fiction and social science can be offered as a gift" (Krieger 1983:192). She describes her study in the following way:

The study conveyed the central excitement of a community's gossip, of everybody talking about themselves and each other, who was going with whom, and why. It allowed a reader to follow "the hotline that went around all the time" in the community, to know the community from its center to its margins, to go to parties, visit bars, share in the breakups of couples and the raising of children, become immersed in the many internal dialogues and private social and sexual entanglements that knit the community together, and hear opinions about commu
Krieger found that the sorting through of her own emotional reaction to interviewees was an essential element of her research process. In the end, however, the book offers an unusual approach to writing in that the authorial presence is not clearly visible in the book. Krieger reports (1983) that some readers saw her book as a “mere presentation” of the voices or experiences of members of the community. They longed for a more explicit analysis by the author.

This chapter, taken from her book Social Science and the Self. Personal Essays on an Art Form (1991), presents an honest, reflective, and insightful account of how she went about beginning to reflect on the data for her book. As she struggled to progress, she reflected on the role of the self in research. In what follows, she discusses why distance, objectivity, and convention were inappropriate to her unique analysis. These issues have taken on new urgency in the academy, particularly in the humanities and the social sciences. For some, the role of the self is simply part of the methodological steps in research; clarification of one’s self can help the researcher to understand issues of bias, limitations on data collection, and the “vision” of the research as guided by the researcher. For others, however, the role of the self is part of a more formidable intellectual critique of the nature of knowledge or what Krieger terms the “fiction/social science ambivalence.” As she states, “Social scientists often do what novelists do: they invent, they use illusion and inner vision, they focus on the unique and the particular. . . . Novelists, similarly, are often discoverers, testing ideas against evidence, developing generalizations, and seeking to be faithful to the details of external experience” (Krieger 1983: 176). Here, some ethnographers argue (drawing from the philosophical roots of Derrida 1982 and other “deconstructionists”) that the fundamental nature of the intellectual enterprise is not what those interested in social science believe.

In the discussion that follows, Krieger asserts the importance of ethnographers engaging in critical reflection of the role of the self in research. Although some ethnographers are aware of the need for concern with the issue of “representation in ethnography” (Van Maanen 1995), others complain that the “over-abundance of concern with the reflexivity of their work and the task of writing ethnography . . . can lose the phenomenon of interest” (Mehan 1992, see also Mehan 1995). Krieger, however, argues that efforts to avoid the role of the self are, essentially, a form of self-deception. She and others charge that ethnographers need to face up to the role of the researcher’s self, as well as to the researcher’s imagination (Atkinson 1990), in ethnography.

In earlier chapters, I have referred to an article titled “Beyond ‘Subjectivity’: The Use of the Self in Social Science.” I have mentioned that the article meant more to me than the book whose research and writing process it comments upon because the article speaks in a first-person voice and feels closer to me than the book. I have mentioned, too, that when I began writing Social Science and the Self, “Beyond ‘Subjectivity’” was very much on my mind. It represented a kind of personal writing that I wanted my new work to live up to. I was afraid I would not be able to be as candid in my new work as I had been in that earlier article.

“Beyond ‘Subjectivity,’” presented here, provides a specific discussion of research and writing in the case of The Mirror Dance. It also extends themes raised in previous chapters of this book. It is concerned with acknowledging one’s involvement in one’s work and with achieving some level of honesty in writing about that involvement. The article argues for use of insights about the self to help one understand others, and it advocates the development of a full enough sense of self, so that understandings of others will not be stilted, artificial, and unreal. Even more important in terms of the present work, “Beyond ‘Subjectivity’” takes as central the problem of asserting oneself, and one’s own vision, or voice, in the face of other voices that often seem to overwhelm and discourage the social scientific author. The most important feeling I arrived at after going through the exercise described in this article, which was aimed at helping me deal with my data, was that “I had a right to say something that was mine.” I think that often we feel we do not have that right in social science, or we feel it is a right we have to earn. I think such attitudes toward the individual authorial perspective, while appealingly modest, are not very helpful. They encourage us to deny that we will speak of things in terms that reflect how we see them. The more important question, is How will we mold these terms? What resources will we use to make our language fit our experiences? Will we draw fully on our internal and individual sense of things? Will we learn how to make good use of ourselves,
or will we primarily apply commonly held views because we know these are acceptable?

In part because it draws on notes about personal feelings that were not originally intended for publication, "Beyond "Subjectivity"" gives a sense of an inner authorial voice. It is an inside story about a book and an author, however brief and well rationalized. (See chapter 5 on the "success" storyline.) The tradition for writing up personal accounts in social science says that our studies are about others, and that our methodological statements should describe how we came to know what we did about them. Dutifully, "Beyond "Subjectivity"" does this, but the article interests me now less as an explanation of how I came to understand a lesbian community than as a statement in its own right that presents aspects of the private world of an author.

To take the internal life and make it external is important, in my view. The challenge of making a true portrait of one's experience remains, but at least the self is acknowledged. All of our statements about others are, very significantly, also about ourselves. We tend to provide little in terms of direct personal discussion when we write our studies, and I should like to see that little become a great deal more. Only with many stories will we get a good picture, since we each can speak only of our experience, and often we do this timidly, afraid of the outside world's tendency to deny us. This general tendency is well fueled by the many specific prohibitions against self-expression within social science. These prohibitions are particularly strong in their effects on the self-expression of women and of anyone not speaking a standard truth. The following article is not intended as a model of originality or correct personal expression, but it is a piece I found helpful, and I hope it suggests that there is more to do. Not only do we need to start talking about ourselves at greater length, but we need to experiment with different styles of self-understanding, for these can be keys to expanding our alternatives, both for being present in our works and for depicting experiences of others.

**Beyond "Subjectivity":**

**The Use of the Self in Social Science**

Recently, in both the social sciences and in related humanistic disciplines, there has been a restimulation of interest in the relationship between observer and observed. Our attention is called to the many ways in which our analyses of others result from highly interactive processes in which we are personally involved. We bring biases and more than biases. We bring idiosyncratic patterns of recognition. We are not, in fact, ever capable of achieving the analytic "distance" we have long been schooled to seek. While recognition of the interactional and contextual nature of social research is not new, how we interpret ourselves during this new period of self-examination may, in fact, add something fresh and significant to the development of sophistication in social science.

I present the following account of my own work with the hope of contributing to a general sharing of personal stories about what we, as social scientists, now do. My account is one of backward beginnings, wrong ways of doing things, and problems I would rather not have had. Yet precisely because of these things, I think, the story is worth telling. In the following sections, I tell about some of what went into the writing of _The Mirror Dance: Identity in a Women's Community_. A study of a midwestern lesbian social group I conducted during 1977-78. The book focused on problems of likeness and difference, merger and separation, loss and change, and the struggles of individuals for social belonging and personal growth.

I began my research unwittingly. I spent nearly a year participating in the community as a member without the slightest thought of studying it. The community was, for me, as for others, a home away from home, a private social world, a source of intense personal involvements and supportive social activity—a source of parties, dinners, self-help groups, athletic teams, outings, extended-family type ties, a place for finding not only lovers, but also friends. I had moved to a midwestern town to take a job as a visiting assistant professor and had found the community by accident and through need. My participation surprised me. "I did not become a lesbian," I wrote to myself in notes at the time, "to become one of a community." Yet the community won me over in the end, and three months before I was supposed to leave the job and town, I decided to study the community in which I was living, to ask questions of these bold midwestern women.

**Data and the Problem of Interpretation**

I had, for years, been interested in the subject of privacy, and I felt that this private, almost secret sphere of social activity would be a good place to talk to people about it. I wanted to learn about how individuals dealt with how they were known, or not known, to others. I then began two months of intensive interviewing with seventy-eight women who were either members of the community or associated with it. Someone joked that I had solved "the sampling problem" by interviewing the entire community "and then some," which was, by and large, what I did.

My interviews lasted an hour and a half each, were usually conducted in my own home, and focused on personal histories of self-other relationships in the community. I asked each interviewee four basic questions: (1) How would you define privacy (what images come to mind)? (2) How
would you define the local lesbian community? (3) Within that community, how have you been concerned about boundaries between public and private, self and other (i.e., what has been your personal and social history)? (4) With respect to the outside world, how have you been concerned about protecting the fact of your lesbianism (who knows, who does not, and why)? Approximately 70 percent of the time in each interview session was spent on question 3, which concerned internal community relationships. Members of the community and others I approached showed me unusual cooperation. They typically came for interviews within a week of when I called. During the interviews, they spoke to me with great candor.

When I left the community, I took with me, along with my personal memories and accounts, four hundred pages of single-spaced typed interview notes, which were, I felt, "rich data" for a study I would soon write up. Then the unexpected happened. For a year I could do nothing with my notes. I picked them up; put them down; moved them around; took notes on the notes; copied them so that one set could sit in loose-leaf binders in my university office while the original set lay in binders on my kitchen table at home. (I had moved the notes to the kitchen table after realizing I kept avoiding them at my desk.) All the while, I kept trying to do simple things: to isolate themes; to find something to say that could be supported by my data. I thought of punching computer cards. I finally culled out the seven interviews with lesbian mothers and attempted to write about their experience, thinking that in some magical way the subject of motherhood would save me. It did not. Then I gave up. I closed the notebooks. I decided to write a novel. Occasionally, I thought about how despite the fact that I was now twelve hundred miles and many months away from the community, I still did not have a necessary analytic "distance" from the subjects of my study. However, that thought did not help.

Finally, a full year later, done with two drafts of my novel, and haunted still by those volumes of notes—the undefined "promise" of my data, the sense that I should not let the research go to waste—I decided, "I must write about what I can relate to. I must write a personal account." I began writing about what it had been like for me to live in that lesbian community. I wrote many pages, and then I shelved them. What I wrote was interesting, to me. Beyond recalling my experience, it enabled me to see that what I had thought of as a lack of analytic "distance" might more usefully be viewed as a lack of personal "separation" from my data, from all those "other women's voices" that rose up each time I took up my notes. But the account I had written was not social science in a conventional sense, and I wanted very much to be conventional.

However, writing the account did give me an insight. The most immediate problem, it seemed to me, was not that I did not have distance from my data, but that I did have, and probably always had, far too much dis-

tance. Before dealing with problems of "separation," I had to acknowledge that I was estranged.

"Process of Reengagement." I decided to deal with my data in any "sociologically useful" fashion, I would have to get over my estrangement. I would have to feel that I could "touch" the experience of gathering my data, and in a way that I had not allowed myself before. I would have to begin by expanding my idea of my "data" to include not only my interview notes, but also my entire year of participation in the community. I would have to be willing not only to feel again what the experience of living in the community had been like for me, but also to feel it as fully and deeply as possible and to analyze my feelings. Why did certain things move me? What had unfolded over the year's time? Why had I felt estranged? What did I want? What did I receive? What was I afraid of? How could I bridge the gap between myself and my data?

Because I am not at ease simply "feeling" in an amorphous way, I went about "becoming in touch" with my data very methodically, in a highly disciplined and structured fashion. For the next four to five months, I devoted myself to an exercise which I called a "process of reengagement." The first stage of this exercise was a step-by-step analysis of my experience of involvement in the community, beginning with entry, progressing through entanglements in personal relationships, singling out key events and my emotional responses to them, reviewing the interview period, and ending with my feelings on leaving. The second stage was a step-by-step analysis of my experience in conducting the seventy-eight interviews which were the source of my notes. I later wrote a personal account of this process, called "Separating Out: A Method for Dealing with Qualitative Data," from which the following is excerpted. This excerpt describes the second stage in my reengagement process and shows how an understanding of the self can help resolve the problem of interpreting one's data.

"Separating Out":
A Method for Dealing with Qualitative Data

A Case-Analytic Technique

The strategy in the second part of my reengagement process required that I deal with each of my seventy-eight interview cases. First, I sought to identify and examine my responses to my interviewees as individuals. I reviewed feelings I had with respect to each interviewee, first, in anticipation of our interview session and, second, during the interview itself. Finally, I analyzed the data of my interview notes themselves. The analyses in each phase of this process were done by writing down my thoughts and feelings, taking up a separate sheet of paper for each interviewee at
each step. When I was through, I had one set of notes reflecting my “preinterview self-assessment,” another on my “interview self-assessment,” and a third on responses to the interview notes.

**Step 1: Preinterview Self-Assessment**

During this preinterview self-assessment step, I recalled my acquaintance with each interviewee prior to our interview and reviewed how each interview appointment had been made. I remembered social occasions during which the interviewee and I had met and what the biases in introduction had been if the interviewee was known to me primarily through another person. Most important, I noted my personal expectations with respect to each interviewee immediately preceding the session: what I had anticipated with fear, and what with excitement, and what I felt I had wanted for myself in return. In doing this, I sought to identify those prejudices I brought to each interview. It seemed important to separate my personal disappointments and pleasures from my latter interpretations of my data. The following examples are indicative of the preinterview item self-assessment. They are excerpts from longer passages written about each preinterview experience.

**PREINTERVIEW 32:** B. was one of my neighbors across the street who had been fairly open and friendly with me. I chose her to do one of the first interviews because she had been “public” as a lesbian and I felt she would be knowledgeable about the community and straightforward with me. Yet I was nonetheless concerned that she might not speak personally enough with me.

**PREINTERVIEW 44:** I knew D. mainly through K. and was prejudiced against her; no more accurately, I felt fear regarding her—that she was judgmental and did not like me because of my relationship with K. and its troubles—that she was primarily K.’s friend.

**PREINTERVIEW 67:** I knew of V. that she was a straight woman in one of the core support groups in the community. Was afraid she would be distant and would withhold. Also, K. had told me V. played “poor me,” so I worried I might get impatient with her.

**Step 2: Interview Self-Assessment**

A similar approach informed the next interview self-assessment step. Here, again, I wanted to identify my prejudices and any “hidden” personal agenda I might have had. Yet in this step, even more so than in the preinterview assessment, I was intent on recapturing the force of my emotions at the time, since they seemed to me to surround my waiting notes. For example:

**INTERVIEW 32:** This surprised my expectations, because B. was, it seemed, candid with me and more personal than I'd expected. I didn't feel forced to adopt her views or anything of the sort. I really felt for her as a person at the end of the interview, as I had not before.

**INTERVIEW 44:** Interview was very tense for me. I felt D. being defensive. Felt pressure on her part that I join her—see it all her way. Felt she didn't want to be interviewed, felt I was pushing this upon her. I was angry with her because of all these things. When I really wanted to be friends, to win her, to have her like me. In the end, I felt she ran away, wishing she'd not said what she did, angry with me. I wished to run after her, to make it all right—to confront. This is the interview I felt worst about of all of them. It seemed so much a denial and rejection of me. Though I felt its content—what she said—was rich.

**INTERVIEW 67:** Was partly tense because I suspected my own motives about wanting to get to K. through V.—get inside information that would help me settle my troubled feelings. Felt partly pressed by V. to feel as she did. Also that V. was partly confused, yet that she felt she had a collar on rationality. The conversation was almost technical, in that she kept much emotion out. Did not like this (angry?—a little, but repressed it) in the end.

**INTERVIEW 72:** M. was the only one to really break down and cry at the time of the interview and want to be held. This scared me—because I did not want to get involved, and did not want her to become dependent on me. I tried to “handle” it by not making a big deal of it, by holding her and then letting her up when I felt she'd be okay. She had brought a tape recorder to record the interview for herself (only one who did this). When she started to cry and asked to be held, I pushed the recorder off. My leftover feelings were fear—that she'd call on me for more holding and that I'd say no. I might do it with someone but somehow I feared her, or she was not the one. I also had feelings that I invited this, with everyone. Then when I got it, drew back from it. This left me uneasy. Feeling angry (?), lonely. What if it were me who wanted to cry and be held?

It became increasingly obvious to me, as I recalled and noted my responses in each case, that I had felt much discomfort and that that had caused me trouble. Yet these were exactly the kinds of things I needed to articulate, since they had been crucial in frustrating my dealings with my data. For instance, the more I noted my responses, the more I became aware of how very often I had been afraid, both prior to the interview sessions and during them. What I had previously identified as anger was really fear. This, I think, was because each interview session was an intimacy situation for me and an occasion which I felt required proof of myself. I wanted, during the interview sessions, not only to know each of my interviewees, but also for them to know and care about me. I reacted as if it were a denial of myself when an interviewee did not seem to care:
INTERVIEW 62: A disappointment. Because M. seemed to me a lot at front—how she wanted to appear, a line, not a real person. I didn't feel the intimacy, the honesty that I wanted. Felt she suspected that I found her false (unconvincing) in this way and that she was angry at this and defensive. When she left, I was let down and angry. Felt she had dealt with me formally, almost as a functionary for herself, rather than as a person. Which I wanted.

The interview self-assessment was difficult. I kept wanting to describe the interviewee and how she appeared to me when she arrived and as she was involved in the session. However, this seemed largely ungrounded, unless I also noted my own reasons for the response—the emotional issue, or issues, each session raised for me. I had to discipline myself to note a reaction of my own for every action of each interviewee that I noted. I had to take time to figure out the logic of my own reactions, for what they would tell me about barriers to dealing with my data. I had not expected the interview assessment to become highly self-reflective, since I felt I had already been extremely self-reflective during the earlier stage of reengaging with the entire research experience (step 1 of my exercise). Nonetheless, new things were brought to my attention in recalling my specific feelings in each of the interview situations.

My most important recognition occurred after going through approximately one third of the cases. I began to notice that I could distinguish my responses in terms of whether I had felt pressure to become like a particular interviewee or whether I had felt I could "be myself" during the interview. I then began to look at characteristics of the different interviewees in relation to myself in order to understand why I would feel or not feel pressure. I realized that I would become angry and feel bad in these cases where I felt I had to be like the interviewee. My sense gained from these cases tended to overpower my sense of the actually larger number of cases in which I did not feel this threat. For example:

INTERVIEW 75: Went well. I was impressed with B. as a person—her independence, the carefulness of her thinking, her clarity. I got a good picture of her—because her words were honest?—and so did not feel threatened. Maybe this is mechanism: when the interviewees are confused (due to being defensive or otherwise inauthentic, or confused about themselves), I get threatened and confused about who I am, because the relationship is confused. I don't know what I am relating to; while if the interviewees are more clear about themselves, I can be more clear about myself.

I concluded that I had felt threatened where it seemed to me an interviewee was inauthentic in her presentation of self in ways that set off my own doubts as to who I was. I decided that my feelings concerning this were so strong because of the fact that I shared an intimate identity stake with all the women I interviewed. I looked to them, even in the ostensibly other-oriented interview situation, to help me solve the problem of who I was. Although the interviews were highly controlled and guided by me, my controls did not protect me from threats on a deeper level. The interviews were actually occasions of inner panic, occasions during which I feared that others would not allow me to be myself—to act as the person I truly felt I was. This feeling of threat to my sense of self had not been fully clear to me before I analyzed the individual accounts. But finally it was, and I could see in my responses how much I had wanted personal confirmation and acceptance:

INTERVIEW 54: In her office. I was uncomfortable because of her power things—the phone, showing off stuff she'd written, her sensitivity. I felt she was trying to impress me with herself, that I was mostly a pawn to this, a person to be won over, not an independent person to be related to—one who had sensitivity, specialness, etc. And I wanted this other response from her. Perhaps because she was a peer at the university, and an unattached woman, recently out of a relationship. I think I had hopes we might be friends. With sexual possibilities maybe. But even if not, I wanted to be an equal, a real person to her. I left disappointed.

As I analyzed my responses in this way, I felt that my desires for confirmation, while perhaps extreme, might be more widespread in the community. The lesbian community might be functioning as an "identity community" for its members, one in which the most intimate sense of self was frequently on the line, a community in which the power to threaten by lack of confirmation was as strong as the power to confirm.

Reexamining my interview session responses made me aware of something else that was extremely important: the extent to which, even in those special-purpose sessions, I was engaged with the community and acting according to its rules, just as I had been outside the sessions. The interview situations were, in effect, small dramatic enactments of the social dynamics of the larger community. They were microcosms providing specific examples of expected or acceptable community behaviors. In looking back on my responses, I was shocked, for example, to see how often my reactions to interviewees included an element of sexual expectation. In this way, I was clearly a member of the community:

INTERVIEW 2: B. was younger than I had expected and very beautiful, with long straight dark hair. She reminded me of a woman I had been involved with back in California in the winter. I think I felt I would like her to fall in love with me.

INTERVIEW 51: Knew E. casually, and occasionally, it seemed to me, she would be showing sexual interest in me. Some part of me, I think, wanted that more and also was repulsed and frightened by it.

The sexual expectation dilemma had been spoken of candidly by one of my interviewees:
It's like good old sex being such an important part of people's life. And coming to a place with that expectation. Like I am here because other people in this room are here because they have the same sexual orientation I do. It puts a great pressure on you as to what am I up to and what are these people in this room doing? A lot of heterosexual traps I tried to escape, I found here. Because of all these sexual tensions, nobody gets to really know each other or to feeling comfortable with each other.

In this same vein, this interviewee also articulated a predicament referred to in the accounts of others:

There isn't one woman in the community I haven't considered having a relationship with, just because you're in this community and because of all the pressure to need and want a relationship. Because you're in this community and because you have to relate some intimate details to get along, there is always the question of whether you want to be intimate. In a straight community, you have all these girlfriends who you tell things to. But in this community, you have to worry about whether it means you want to go to bed with them.

It was not easy for me or for my interviewees to acknowledge the pervasive and central sexual tensions of the community, since those were often subtle and personally sensitive. However, by examining my own responses, I was able to arrive at important insights. I concluded that my feelings of sexual expectation had less to do with actual possibilities for sexual relationship than with rules for defining the self in the community. For this was a community in which one's sense of personal identity was closely linked with one's feelings of sexual possibility and in which sexuality often appeared as a route to intimacy, as a means by which an individual might become truly known.

**Step 3: Analyzing the Interview Notes**

Once having completed both preinterview and interview self-assessments, and interpreting as many of my own responses as I could, I turned to the task of dealing with the content of my interview notes. Initially, I wanted to treat the accounts of my interviewees, as much as possible, as separate and different from my own. I wanted to see my interviewees as sharing my processes and reactions perhaps on occasion, but not as a rule. Yet as I began to review my notes, seeking concepts appropriate for categorizing and "making sense," I found that I was drawing on my understanding of myself with far greater facility than on anything else that came to hand. The task then reformulated itself as one in which I would seek to determine if and how my interviewees shared versions of the problems I had identified in myself.

I would look for words used by my interviewees that were reminiscent of my own, processes that were similar to mine, and assumptions about self in relation to others that were similar. Most centrally, as I read, I would imagine each individual as existing in a problem situation concerning differentiation of self in the community. I would view each individual as seeking, time and again, confirmation for who she was, all the while suspecting she might not belong.

Increasingly, as I examined the notes, I found what seemed to be parallels among the feelings expressed by the interviewees. For example, there was a frequent concern with possibilities for rejection by the community, whether rejection was or was not likely to occur:

I have a yearning to be part of the community, but I feel, and I know by the grapevine, that I would be rejected.

There was a sense that the community had rules that excluded important aspects of the self, as these excerpts from different interviewees suggest:

It's hard to capture because all that is implicit—a sense that the community does have these strong rules.

There are some things you couldn't say.

A sense of the community as unreal or uncertain appeared often in the various accounts:

The community, for me, became a monster.

I see several different communities.

Like the first two years I lived here, I was unaware there was one [a community].

I think of them as a real tight closed group, that's closed until they know for sure that you're a lesbian, for one thing. And I don't think that you could just go meet them, go hang out with them. I think you have to join them.

In most of the accounts, there was a desire for the community to provide acceptance and self-confirmation:

The community, to me, is a group of women who I could know that I could lean on for support.

Here is a group of women who can understand me, touch me the way I want to be touched.

When I had finally found these people, I felt I had finally found people who could accept my whole life.
Along with the need for confirmation were feelings of extreme disillusionment and disappointment when the community seemed to have failed a particular woman:

You would think it would be easier to assert your differences in a community of women. But it's not. It is real disillusioning.

I needed reassurance that I was doing all right. I needed some indication that I was appreciated. And they kept spewing forth this ideology of the community, the community, this axil of support when I felt totally abandoned.

During this data analysis step, I used my own insights and developed them further with reference to analyses of the interview notes. This enabled me finally to write a paper about the collective reality of participation in the community. The reality I described in that paper was, of course, only part of the reality felt by community members, that portion I could be in touch with as a result of my experience. But by now, as a result of clarifying my experience, I was no longer as frightened of my involvement as I had been initially. I could use my own recognitions as a guide, a source not only of personal but of sociological insight.

This is not to suggest that my interpretations of the community were merely interpretations of myself “writ large” and imposed on the testimony of others. I also had to follow additional rules that granted to other members of the community feelings and responses that were different from mine. Throughout the process of analyzing my notes, it was important for me to maintain a sense that there was much in each interview account that fell beyond my own limited experience. My task was to try to uncover what I could with the tool of myself and my personal recognition. I sought not simply to impose or to apply my newly developed recognitions, but to expand those recognitions by constantly challenging my existing understandings: challenging my perceptions of others with what I now felt I knew about myself and, at the same time, confronting my self-understanding with what my interviewees seemed to be telling me that was different.

I think that often in social research, this is what we really do. We see others as we know ourselves. If the understanding of self is limited and unyielding to change, the understanding of the other is as well. If the understanding of the self is harsh, uncaring and not generous to all the possibilities for being a person, the understanding of the other will show this. The great danger of doing injustice to the reality of the “other” does not come about through use of the self, but through lack of use of a full enough sense of self, which, concomitantly, produces a stifled, artificial, limited, and unreal knowledge of others.

Conclusion

The preceding account describes an exercise that helped me to reengage with my data at the same time as I was “separating out” a sense of myself. The exercise proved immediately useful in generating insights. However, my problem of estrangement was not so easily solved. In 1980 when I returned to California, I again had trouble dealing with my data. I wanted to work with it and, simultaneously, to leave it—to begin new research. At that point, it helped for me to think back on the exercise I had engaged in during the previous year. That exercise had given me some confidence and an initial understanding of the nature of my problem. I knew I would have to “assert myself,” even if my assertion felt uncomfortable, and even if I would continually feel I was illegitimately imposing myself on my data.

Two and a quarter years had passed since the original research for my study was completed. I finally began writing The Mirror Dance. I wrote it quickly, relying upon what now seemed deeply imbedded intuitions. The book was published in 1983. Responses from both reviewers and readers suggested that its portrayal was valid, to a surprising and somewhat uncomfortable degree. Yet I knew that what I said in The Mirror Dance was dependent on a very personal and idiosyncratic process of data gathering and analysis. Because that process was so personal and because it worked essentially “backwards”—to understand my community, first I had to understand myself—I have presented a partial description here of an analytic exercise that helped me greatly.

The exercise I engaged in was, for me, a way out of a problem. It was a source of insight both about others and about myself. It gave me some confidence when I needed it; it gave me a feeling that “I had a right” to say something that was mine. I had studied a community that I felt I was part of and, at the same time, that I felt estranged from. I was, at one point, overwhelmed by the voices of all the women in that community. They were all telling me what to do, and they were each telling me something different. It took a long time—longer than I had expected—to find, in myself, a voice by which I could speak back to them.

I found that voice and, as The Mirror Dance attests, I hid it. The Mirror Dance is written in an unusual ethnographic style, in which the voices of the women of a community interweave with and comment upon one another, analyzing their collective situation. The subjective “I” of the author is hidden in the book, never mentioned, emerged finally back in with the community from which it emerged. It is precisely for that reason that the preceding account seems important to me, for it speaks to the origins of the book’s inner voice. More crucially, it speaks of a personal process. In social science, I think, we must acknowledge the personal far more than
we do. We need to find new ways to explore it. We need to link our statements about those we study with statements about ourselves, for in reality neither stands alone.

Notes

This article originally appeared in *Qualitative Sociology* 8:4 (1985): 309–324; it is used here by permission. For their help in preparing the original article, I thank Estelle Freedman, Marytheima Brainard, Nancy Chodorow, Meredith Gould, and Ann Swidler.

1. This restimulation of interest has been sparked most dramatically by the development of feminist scholarship across fields. This new scholarship has led to a reexamination not only of the difference that gender makes in determining what we see, and how we see it, but of other perceptual nets as well. In the recent literature, of great interest are Evelyn Fox Keller’s writings on gender and science: *Feminism and Science*, Signs 7:3 (1982): 589–602; *Feminism as an Analytic Tool for the Study of Science*, *Academe* 69:5 (1983): 15–21; *A Feeling for the Organism: The Life and Work of Barbara McClintock* (San Francisco: W. H. Freeman, 1983); and *Reflections on Gender and Science* (New Haven: Yale University Press, 1986). Keller deals with notions of objectivity, subject-object splits, and gender in the work of scientists. See also Carol Gilligan, *In a Different Voice: Psychological Theory and Women’s Development* (Cambridge: Harvard University Press, 1982), concerning women’s distinctive developmental experiences and how these can lead to highly contextual ways of seeing; and Nancy Chodorow, *The Reproduction of Mothering: Psychoanalysis and the Sociology of Gender* (Berkeley: University of California Press, 1978), which provides a basic psychoanalytic statement concerning women’s self-other relationships. Each of these works, to a significant degree, draws on theories of object relations, a field in which an important recent contribution is Margaret S. Mahler, Fred Pine, and Anni Bergman, *The Psychopathological Birth of the Human Infant: Symbolism and Individualization* (New York: Basic Books, 1975).

Carrying out a research project is a journey, an intellectual journey. As with all journeys, it has unpredictable moments. Part of the way through the trip, participants often become discouraged. The moment of arrival—a finished paper, the end of the semester, graduation—seems far away. Some individuals may doubt the wisdom of having chosen the pathway at all. Although these "bumpy moments" are attached to almost all serious endeavors, research using ethnographic methods has more predictable moments of uncertainty, serendipity, and unavoidable error than do other research methods. These mistaken moments can be particularly painful to novice researchers. Newly minted researchers can ignore the special contributions they bring to a project as humans, including their enthusiasm, excitement, energy, and drive. They can, as Annette Lareau shows in her appendix, focus on the inevitable slips and problematic moments of the process.

Lareau, a sociologist, wrote the following piece as an appendix to her book *Home Advantage: Social Class and Parental Intervention in Elementary Education* (1989). Lareau’s essay lays bare some of the common trials and tribulations faced by a graduate student engaging in her or her first field study. When she considered publishing the piece, she worried that her fieldwork mistakes were so serious that revealing them could have a negative impact on her scholarly reputation and, possibly, her future career. In the end, her frustration with the lack of frank appraisals in the field won out. She was also swayed by assurances from colleagues that all researchers face such dilemmas. In 1991, her book won the Willard Waller Award for Distinguished Scholarship by the Sociology of Education Section of the American Sociological Association.

Drawn from the research for her doctoral dissertation, Lareau’s study examines social-class differences in parental involvement in schools. In the book she argues that educators and sociologists have failed to understand that social class provides parents with resources to comply with the request of teachers for assistance. Whereas teachers understand the tendency of working-class parents not to comply with their requests as a result of parents’ (lack of) concern for children’s educational success, Lareau suggests that social class leads parents to mean different things when they assert they want to be helpful. For working-class parents there is a separation between home and school; these parents turn over responsi-
bility for schooling to “educated people.” They depend on the school to educate their children. They worry, at times correctly, that they are unable to help with school. Middle-class parents, by contrast, generally see an interconnectedness between home and school. Such parents view themselves as capable of helping with schoolwork as well as capable of critically assessing the professional competence of teachers.

Using case studies, Lareau shows positive consequences for some children from (middle-class parents) involvement as well as negative consequences when working-class parents adopt a separation between home and schooling. Using the work of Pierre Bourdieu and the concept of cultural capital, she argues that parents’ strategies for family-school relationships draw on the resources of middle-class positions to boost their children’s school experience.

Lareau’s current project has involved a dramatic change in qualitative methods, with a heavy focus on participant-observation rather than interviewing and a much larger sample size diversified by class, race, and gender. She believes that there has been an ongoing “conversation” in herself, and her second project compensates, avoids, or overcompensates for weaknesses in the first project. Although new problems have developed, she still reports taking pleasure from removing or being able to avoid repeating some of the “thorns” that plagued her in her first major work.

Common Problems in Field Work:
A Personal Essay

ANNETTE LAREAU

In his appendix to Street Corner Society, William Foote Whyte describes why twelve years after his book was originally published he decided to write a detailed portrait of how he did his famous study. He reports that he was teaching a methods course and had trouble finding ‘realistic descriptions’ of the field work process:

It seemed as if the academic world had imposed a conspiracy of silence regarding the personal experiences of field workers. In most cases, the authors who had given any attention to their research methods had provided fragmentary information or had written what appeared to be a statement of the methods the field worker would have used if he had known what he was going to come out with when he entered the field. It was impossible to find realistic accounts that revealed the errors and confusions and the personal involvement that a field worker must experience (Whyte 1981, p. 359).

Three decades later the problem remains: realistic descriptions of how research data are collected are unusual. Most studies by sociologists who use qualitative methods devote a short section to the research methodology: they describe the number of respondents, the selection of the sample, and general procedures for data collection. But, as Whyte complained, these studies—some of which are exemplary works—rarely portray the process by which the research was actually done, nor do they give insight into the traps, delays, and frustrations which inevitably accompany field work (but see Walford 1987).

This lack of realistic portrayals is a problem, for they are not simply to assuage readers’ desires for more personal information about the author, or to get—for those of us with more malicious inclinations—‘the dirt’ on a project. Rather, they give qualitative researchers a formal avenue for reporting how they proceeded with data collection and analysis. Without these details, it is hard to tell when researchers did an exemplary job in the data collection and analysis and when they did a ‘quick and dirty’ job. It is agreed, of course, that one should establish a rapport with one’s respondents, be sensitive to the field setting, take comprehensive field notes, analyze your data carefully, and write it up in a lively and accurate fashion. What that actually means, and what researchers actually do, is often anybody’s guess. Most studies do not reveal their inner-workings, and good writing can cover up awkwardly collected and poorly documented field work.

In his appendix Whyte chose to do his ‘bit to fill this gap’ (1981, p. 359), and in this appendix I have decided to do my bit as well. One of the biggest problems is that this entails writing up my mistakes as well as my successes. In most lines of work, including teaching, almost everyone is forced to admit to having made mistakes from time to time. But admitting mistakes in field work seems more difficult. Partly, this is because we often have an overly romantic notion of field work, which emphasizes the glory of ‘going native’ and glosses over the difficulties and problems of the endeavor. The implicit message is that mistakes are rare. Partly, this
reluctance is an artifact of a scholarly tradition in which a public discussion of "inner-workings" is considered unseemly and unnecessary. Finally, admitting to mistakes in field work raises questions about the quality of the body of the research and the conclusions drawn from it. Given these considerations, it is hardly surprising that so little has been written about actual experiences in the field. Likewise, it is clear that all of us who are engaged in qualitative research could greatly benefit from a more frank sharing of our experiences.

My project has strengths as well as weaknesses. There are parts of the data set in which I am fully confident and parts which I think are considerably weaker. This assessment is implied in the way in which the work is written up, but in my view it is worth making this more explicit. So, in a fit of immodesty as well as honesty, I provide my own assessment of the strengths of the project, and identify my successes as well as failings as a researcher.

This appendix consists of two parts. In Part I, I review the background for the study, access and entrance to Colton and Prescott, my role in the classroom, the selection of families, the interviews, and my assessment of the major mistakes I made in the research. I also briefly summarize the logistics of data analysis. In Part II, I turn to the development of the conceptual model and my struggle to formulate the research question. It is my hope that readers will find this 'expose' of a research project useful, not only for gaining insight into this particular study but for detailed examples of how to cope with common problems in field research. As I bumped about in the field not knowing what I was doing, I often felt—incorrectly, as it turned out—that I was making a terrible mess of things, that my project was doomed, and that I should give up the entire enterprise immediately. This pessimism came from my persistent feeling that, despite my having had a research question when I started, I didn't truly know what I was doing there. In part, my gloom signaled the continuing struggle to clarify the intellectual goals of the project.

As I have discovered, using qualitative methods means learning to live with uncertainty, ambiguity, and confusion, sometimes for weeks at a time. It also means carving a path by making many decisions, with only the vaguest guidelines and no one to give you gold stars and good grades along the way. It has its rewards. Yet, there were times in the field that I would have killed for an inviolable rule to follow—an SPSSX command to punch into the computer and let the results spill out. I found it exhausting, as well as exhilarating, to be constantly trying to figure out what to do next. It is unlikely that qualitative work will ever have specific research rules to punch into a computer, but it can—and in my opinion should—offer novice researchers more concrete guidance on matters of data collection, data analysis, and the writing up of qualitative work. This appendix is one, small contribution toward that process.

---

**Part I: The Method of Home Advantage**

**Personal Background**

I grew up in a white, upper-middle-class family; my father and mother worked as school teachers. When I was in college, I spent three months in a small, predominantly black community in rural California, working in the schools as a teacher's aide and helping children with their homework in the evenings in their homes. After I graduated from college I thought about becoming a school teacher, and had there been jobs available I might have done so.

Instead, I got a job interviewing prisoners in City Prison for the San Francisco Pre-Trial Release Program. The program was called the OR Project because it released defendants without bail on their own recognizance (OR). Every day at 6:30 a.m. or at 5:00 p.m. I went inside City Prison. There, with one or two other co-workers, I made a record of who had been arrested, called them out to be interviewed, and spoke with them in the waiting room, through double-paned plastic windows and over telephones. Typically, I interviewed three to eight persons per day in the prison itself, then, in the office, I usually interviewed (by telephone) another ten or fifteen persons throughout the day. Each case needed three references—people who knew the defendant well and could verify the information collected, particularly the defendant's address, contact with relatives, and employment history. Over the course of two years, I did a lot of interviews.

The conditions for interviewing in this job were not exactly ideal. The telephones in City Prison did not work well; one or two were regularly out of order, and the ones that did work sometimes had static, so conversations were often conducted in a shout. Another OR worker was often sitting right next to me (about one foot away) also shouting interview questions. For each interview I would talk over the telephone (through the window) to a defendant, using my right hand to plug my ear so that I could hear her/his response. Once I heard the answer, I would balance the telephone on my left shoulder, use my left hand to secure the paper, and write down the answer. Throughout the interview other defendants stared down at the scene, and bruisers, lawyers, families, and the guard with the door keys were all within ear shot. The defendants were often in a crisis: many were dazed, angry, and adjusting to City Prison.

When I finished that job, I thought (modesty aside) that I had become an outstanding interviewer. I knew, particularly in telephone interviews with the families of respondents, that I often could get people to cooperate when other interviewers failed. I also knew that my interviews were very detailed, accurate, and, despite my truly terrible handwriting (a tremendous liability in field work), were considered to be among the best.
in the office. From that job I developed a love of interviewing as well as a firm desire to avoid ever being arrested and put in prison.

After I quit this job, I entered graduate school at the University of California, Berkeley, where I also worked intermittently as an interviewer. The twin experiences of working for two years as a full-time interviewer and working part-time on several research projects meant that I approached the field work for this study with uneven skills. In retrospect, I believe that this background had an important influence on the quality of the data I collected. I discovered I was more comfortable as an interviewer than as a participant-observer. While the months in the classrooms provided crucial information for this study, my field notes, for a complicated set of reasons which I explain below, were not as comprehensive, focused, or useful as they should have been. The interviews were much better. I felt I had a good rapport with the mothers and fathers I interviewed and I have confidence in the validity of the results.

The fact that the interviews were tape-recorded was also a major advantage. As my research lurched from studying everything in front of me in the field setting to a specific topic, my interests in a particular interview also shifted. Had I taken notes instead of tape-recording, I am certain that the comprehensiveness of my interview notes would have varied according to which question in the field setting seized my interest at that moment. Although tape recorders do introduce an effect, particularly during the initial stages of an interview, I would not plan a new research project without them. In my opinion, they provide a form of insurance on the accuracy and comprehensiveness of data collected in the face of shifting intellectual concerns.

The Beginning of the Project

The research proposal, in its original formulation, was to study social class differences in family life and the influence of these family patterns on the process of schooling and on educational performance. I had grand plans. I was going to link class differences in family-school relationships to achievement patterns. I had hoped to study three rather than two schools; interview six families in each school; and I wanted to supplement the qualitative study with a quantitative analysis of a national data set of family-school relations. Almost immediately reality began to set in. Although I still think it would have been a good idea to have had a third school that was heterogeneous in students’ social class, I also still think it would have been too much work. Without any real idea of where to begin, even comparing two schools seemed like two schools too many.

I did have a rough idea of what types of schools I wanted. I decided to study two specific social classes—white working-class and upper-middle-class parents. In this regard, as I note in the text, I followed in the footsteps of others in defining social class, notably Rubin (1976) and Kohn and Schooler (1983). I also wanted schools with a large number of white children to prevent the confounding influence of race. I ultimately sought two homogeneous schools, with a concentration of children in each of the two social classes. Since most schools in the greater Bay Area are, in fact, segregated by social class, and to a lesser extent by race, this initial focus provided hundreds of schools as possible sites. I was timid about approaching schools. I worried about why a school would ever admit me. At times simply getting in seemed insurmountable, a problem discussed extensively in the literature. In the end I used a different strategy for each school.

Access and Entrance at Colton

About two years before I began, I visited Colton school (and four other schools in the district) and interviewed the principal and vice-principal as a graduate research assistant on another project. The principal investigator of that project had asked for schools with a range of students by social class and Colton was the low socio-economic school. It was considered to be one of the best run schools in the district and I liked the principal and the vice-principal. In addition the school had a large number of white working-class students, a relatively unusual pattern. After a lot of stalling, I wrote the principal a letter (which unfortunately I have lost) asking for permission to visit one first grade classroom to learn about family-school relationships. I then called him and set up a time to talk about the project with him and the vice-principal.

To my astonishment, both of these administrators were very positive. We met for about fifteen minutes in the teachers’ room (they had my letter in front of them) and most of the discussion centered on choosing a teacher. They had five teachers to choose from; I left the choice to them. They recommended one of their best first grade teachers, Mrs Thompson, and I accepted their choice. I knew it would be difficult to get one of their worst teachers and there were not, at least in my mind, any compelling analytic reasons for asking for one. In fact, I preferred to have two good teachers in schools with good leadership. If I did, indeed, find class differences in family-school relationships, I didn’t want those findings confounded by questions about the quality of the teachers or administrators.) After our brief chat, the principal and the vice-principal said they would talk to the teacher for me and suggested that I return the following week. I left the school completely elated. I felt as if I might, after all, get this project off the ground.

The next week I returned, fifteen minutes late (I forgot my map and got lost), and the vice-principal took me to the classroom, where class was in
progress. After the children went out to lunch, Mrs Thompson joined me at the table in the back of the classroom where I had been sitting. My notes from this encounter are sketchy at best:

I summarize [the] project as an effort to learn about non-school factors (influence on achievement). She says what do you want to do next; I say just observe, and then select five children and start to interview the parents. In the meantime, though, just observe and if I can help out in any way in the classroom then I am happy to do so. I also say that I realize that it is a busy time of year (tell her my mother was a teacher for 18 years) and that if I become a burden she should feel free to tell me. We then talk about when I will come next; she doesn’t know exactly when class starts (she says, ‘I just listen to the bells’) and so checks chart on the wall . . . We determine I will come Monday at 9:04.

From our first encounter on, Mrs Thompson was extremely nice, very friendly, and always tried to be helpful. Although I would like to think it is something I did to put her at ease, I think that basically she is a very nice person who goes through life being considerate and helpful.

In what became a play within a play, Mrs Thompson and other Colton staff were very helpful with the project and, without consulting parents, provided extraordinary materials. The teachers and staff simply gave me the test scores for all of the children in the class without any concern about consent forms or parents’ permission. The principal, in considering the project, did not express any concern about the burden on parents and never suggested that I clear the project with the district office. And I never asked him if this was necessary. This was a mistake because for the rest of the project I was unnecessarily worried about what would happen if the district research officer found out about it. I also needed some district statistics and finally had to call the office and ask for them, without mentioning why I needed them. In addition, the principalship changed between the first and the second year of the study; both the principal and vice-principal left. I wrote a letter to the new principal and he agreed to cooperate and to be interviewed, but he might not have. This would have been extremely costly since I was almost one-half of the way through the study. As a result, I may have in getting the highest official’s formal approval for a project early on. I think it is very wise to contact respondents through informal channels but, once having secured access, it is important to gain official approval as well. This is usually not very hard to do (after you are already in the door).

Sometimes I puzzled about why Colton teachers and administrators cooperated so easily. The principal and vice-principal were interested in the research question; all of them thought family involvement in education was important but, although it was never articulated, they mainly seemed to think that being studied was part of their job. They had other researchers before me and expected others after me so they did not seem to treat it as a ‘big deal’ and were unruffled, helpful, and a bit blase about the entire matter.

Access and Entrance at Prescott

At Prescott it was another story. There I was not given any real difficulty but the goals of the project were closely scrutinized. The district and school administrators expressed concern about the perspective of parents and the burden on parents, but the fact that both a district official and the principal were also graduate students appeared to be helpful in ultimately securing permission. Whereas I was never even asked about consent forms at Colton, the principal at Prescott asked that I get a separate slip from the six parents giving her their permission to release test scores to me. She felt that the human subjects permission form, although important, was not specific enough to cover the release of those materials. Knowing I had consent forms for only six families, the principal would never have released the test scores for the rest of the class to me.

Part of this greater formality and rigidity at Prescott may have been related to my point of contact with the school. At Prescott I went through the district office which increased the emphasis on the procedures for approval of research projects (such as consent forms). I ended up at Prescott, rather than another school, through informal networks or the ‘strength of weak ties’ (Granovetter 1973). When I was looking for an upper-middle-class community Charles Benson, a very helpful member of my dissertation committee, suggested I think of Prescott. One of his graduate students (whom I knew slightly) worked as a district official, and at his suggestion I called her and then wrote her a letter.

That letter is reprinted in an end note. It has many problems and it is much too long. Access letters should state the problem very briefly and then summarize accurately what the officials are being asked to do. In my letter the most important part (what I was asking them to do) is buried. The content of the letter I wrote to the district official was different than what I planned to tell the teacher and principal. Given that the district official was another graduate student I felt that I somehow owed her a longer, more academic explanation, but I had planned to adopt a much more vague approach with the teacher and parents. This strategy backfired because the district official forwarded the letter I wrote to her to the principal, who in turn gave it to the classroom teacher. I was quite upset at myself for this at the time as I should have known that might be a routine procedure. The lesson from this for me is that it is foolish to think, even if you are fellow graduate students, that one person should get one version
of your project (when you are requesting access) and another person should get another. It is better to draft one version suitable for everyone.

Moreover in this age of bureaucracy, unless you are lucky, you will have to write a letter formally requesting access to a site. And the note presents an introductory letter which, given what I know today, I wish I had written. It is much shorter, more direct, and it focuses primarily on what I need from the site. Respondents do not need to be told, nor are they generally interested in, the details of the intellectual goals of the project (but see Walford 1987a). They seem mainly interested in knowing how much work you are asking them to do.

I now think that before I go into the field, I need a very short and very simple explanation for what I am doing there. When I began I had a one sentence description I was comfortable with: 'I want to learn more about how families help children in school.' If the listener wanted more information, however, I floundered. My answer, inevitably long and rambling, made both the person who asked the question and me squirm. Since that time I have been bored and perplexed when a simple question to a graduate student ('What are you studying?') produced a long, ambiguous, and defensive treatise.

As a matter of politeness, many people ask researchers what they are planning to do while in the field. It is essential to have a fleshed-out response prepared well in advance. In fact, in my busy moments, I think that no researcher should begin a field study without memorizing a jargon-free summary of her/his intentions. This will save many awkward moments, increase rapport with people in the field, and help prevent the problem of respondents feeling particularly 'on stage' when they begin to engage in the activities in which they know that you are interested. A brief, accurate, and general statement will not of itself produce good rapport, but it is a better beginning point than a long and confused one.

After receiving a copy of my letter (which was a mini-paper) the principal called me. She explained that the school was concerned about overcrowding parents but that she was a graduate student as well and was sensitive to problems of research. Her biggest concern seemed to be the choice of the teacher. I wanted a self-contained first grade classroom; that year Prescott had only one first grade and one split classroom. There was only one choice and that was Mrs. Walters.

I don't know what the principal, Mrs Harpst, told Mrs Walters. I do know that Mrs Walters was originally reluctant to have me in her classroom. As she told me later, she was afraid I would be a 'critical presence.' She agreed to participate, however, and the principal, in another telephone exchange, told me what day to begin the field work. Consequently I entered the school without ever meeting the principal face-to-face, and although I was at school regularly I did not meet her for several weeks.

Common Problems in Field Work

The first day I appeared at school, Mrs Walters' welcome was cool. She showed me where to hang up my coat and put my purse but said very little in answer to my questions. Her aide, Mrs O'Donnell, was much warmer and bubbly. Mrs Walters told the children who I was while they were waiting in line outside the classroom. Her comments were:

Today we have a visitor named Miss Lareau. She hasn't been in a classroom for a long time and so she wanted to visit our class and see how you work and talk and play.  

Mrs Walters' classroom was much smaller than Mrs Thompson's, and there was no free table at the back of the room. I felt painfully and obviously out of place that first day, as I listened to Mrs Walters talking to the children outside the classroom and watched Mrs O'Donnell work in the corner on some papers. They had not suggested where to sit or stand and I felt continually in the way. Finally, I found a chair in an empty space at the back of the classroom. When the children walked in they all stared at me intently and then walked to their seats. Still staring. I was uncomfortable, Mrs Walters seemed uncomfortable, and the children seemed uncomfortable as well, although, as I explain below, they quickly adjusted to my presence.

My entrance to Prescott therefore was less smooth than at Colton for many reasons, including Mrs Walters' general discomfort at having me in the classroom, her lack of control over being selected as a research subject and, on top of that, her having been shown my overly complicated letter. I was worried that the focus on social class described in the letter might have had an important influence on Mrs Walters' behavior. She never seemed to remember what I was studying; she consistently treated me as if I were an educator studying the curriculum (which I was not) rather than someone interested in family involvement in schooling. As noted in my field notes:

Mrs Walters seems very interested in explaining the logic of learning activities to me. She carefully explains the bucket program and the 'hands on training' they are receiving. . . . This pattern, of Mrs Walters repeatedly telling me about the curriculum, makes me think that she sees me as an educator with the tools to evaluate a good or bad learning program. And/or, [it makes me think] she is worried about being evaluated.

Over time my relationship with Mrs Walters gradually warmed up. I considered the day she told me that she originally hadn't wanted me in the classroom to be a watershed. I felt that I had reached some level of acceptance but it took more time and more work than my relationship with Mrs Thompson. As I complained in my field notes, 'I often feel at a loss for words [with her].' Being somewhat shy myself, I felt ill at ease with her.
when we were alone together and I often seemed to stumble in my efforts to chat with her. But the aide was so friendly and got along so well with both Mrs. Walters and me, that when she was there, the social interaction was quite comfortable and pleasant. During recess the three of us would go get a cup of coffee and visit together. When the aide was not there (in the afternoon or when she was sick) relations between Mrs. Walters and me were much more formal. Like some older married couples, Mrs. Walters and I both seemed to be more comfortable in the classroom with the children between us than trying to negotiate socializing together in a quiet classroom. At first I almost dreaded recess and lunch time with Mrs. Walters, and I felt that I truly did not know what to do with myself. If Mrs. Walters was doing an errand my choices were to sit in the classroom (which I felt self-conscious about since I never saw any aides or teachers do this), sit in the teachers' room (where I didn't know any of the teachers and conversation seemed to grind to a halt with my presence), or go to the bathroom and then return to the classroom looking busy (I did that a lot).

From this I learned that I had difficulty 'hanging out' and that I was happier in more structured situations, such as when class was in session or when I was interviewing someone. I also concluded that life would have been easier if, during the very first days in the field, I had come to school more frequently than twice a week for a few hours. If I had stayed all day and come three or four days in a row during the first week I would have been introduced to all of the staff and become more integrated. As it was I was introduced to a few staff members, but after that I saw a lot of familiar faces but was never introduced to them. Today I am much better at being able to say, 'I don't think we have met, although I have seen you around. My name is . . .' But at that time I felt tongue-tied and often moved in and out of the teachers' lounges in both schools without talking or getting to know the other teachers.

Although I came to feel accepted in the classroom in both Prescott and Colton I never felt very comfortable outside of the classroom. This meant that my study was essentially restricted to single classrooms, and I lost the possibility of learning about the organizational dynamics at each school. Even today I feel that if I had been a more skilled field worker, had become more comfortable on the site, had been better at easing my way into informal settings and simply 'hanging out', that I would have learned more than I did. In particular I might have learned more about routine conflicts between parents and teachers in other classrooms, disagreements among teachers about how to manage parents, and principal-teacher relationships. I also might have gotten onto a more human footing with Mrs. Walters and, for example, learned more about sensitive issues, including how she felt about parents breathing down her neck and who really was. As it stands the manuscript treats these issues only superficially.
any work at all. The children seemed to operate under an implicit classroom rule that if an adult was watching you then you behaved and worked. I was ambivalent about what my role was to be: I didn’t want to be a teacher or a disciplinarian. Like a favorite aunt or family friend, I was hoping to avoid discipline issues altogether. I wrote about this ambivalence in my notes:

I am unsure as to what my role should be when children are not working productively or are ‘acting out’ with squabbles and minor fights. It is noteworthy that most of the children ignore me and continue their disputes in my presence (while with Mrs Walters and usually with Mrs O'Donnell the dispute is changed or is dropped).

The children quickly realized that I would not scold them and force them to work; as a result they would continue to misbehave in front of me. This made me uncomfortable. On the one hand I didn’t want to be scolding children; on the other hand I didn’t want Mrs Walters to feel that I was not helping out and doing what adults normally did in the classroom. Consequently I sometimes looked foolish and ineffective in the classroom, as this example makes clear:

[Today] two boys were pushing each other in their chairs while they were supposed to be playing the numbers game. I came up behind them and said something weak/mild such as, ‘Are you boys playing the numbers game?’ They obviously were not as they continued to shove and push each other. Mrs Harris then saw them and came over and said harshly, ‘Jonathan, Roger. Stop that instant! Now sit up and sit in your chairs and behave!’ (She physically pushed them apart and pushed their chairs closer to their desks).

Clearly, Mrs Harris was not ambivalent about controlling children. As my notes reflect, I began to think that I might have to get off the fence and take a more assertive role:

When I started volunteering I wanted to disrupt the classroom activities as little as possible and so I made a concerted effort to stay away from the teacher/disciplinarian role. I am discovering, however, that in the world of children the adult/child split means I am often forced into the teacher/disciplinarian role. Otherwise I am seen as powerless, not threatening, and the object of a great deal of acting out behavior when the children are not under the teacher’s rule.

I didn’t write down the actual date that I finally decided to abandon my passive role, but by about one third of the way through the field work I was controlling the children more and following the roles of the parents and teachers. This seemed to help; I felt more comfortable in the classroom and the children, Mrs Walters, and Mrs O'Donnell began to treat me like another parent or teacher’s aide. I helped children with their stories, their art work, and various projects. I gave tests. I dictated problems which they wrote on the board, and supervised children, enforcing classroom rules when we went to the auditorium for a special event. When Mrs Walters left school to have an operation six weeks before the end of the semester, I continued to visit the classroom. By then I was integrated into the classroom, and Miss Chaplan, Mrs Walters’ replacement, seemed to accept my presence. I helped organize the report cards and the games on the last day of school.

As I discuss in the text, my relationship to parents mirrored the pattern of family-school relationships in the two schools: I had much more contact with parents at Prescott than at Colton and the parents at Prescott scrutinized my activities much more closely than they did at Colton. There were advantages and disadvantages to this. The advantage of my more active role in the classroom at Prescott was that I worked with some of the parents and was, in many ways, a valuable assistant to Mrs Walters, which she appreciated. The drawback was that I couldn’t take notes in the classroom. I only tried that once in Mrs Walters’ class. The room was too small to accommodate a desk for me so I had to write on my lap; and I was only two or three feet away from the children’s desks so my note-taking distracted them. Also, I was often there for independent time and Mrs Walters needed adults to walk around and help children as they all worked independently. As a result I had to try to retype notes after I left the site. This increased the amount of time that field work demanded and produced notes with fewer quotes than at Colton.

Access and Entrance to the Families

When I entered the field I had planned to select the parents of children at the end of the school year, after I had observed in the classroom. Seeking a balance by gender and achievement levels, I decided to select a boy and a girl from the high, medium, and low reading groups for interviews. In each school, I wanted five children from intact families (although their parents could be remarried) and one child from a single-parent family. At Colton, since almost one half of the class was non-white, and around a quarter were from single-parent homes, only about one third of the children were potential candidates. One day after school, Mrs Thompson and I sat down with the reading groups. We chose a boy and a girl from each reading group. Whenever possible Mrs Thompson recommended children whose parents she knew from having interacted with them at school. As a result, the Colton families I interviewed were somewhat more active in their children’s schooling than the average parent. After we had made the choices, she gave me a booklet with the names, ad-
dresses, and telephone numbers of the families and I copied them out. She also gave me test scores for the entire class.

Mrs. Walters was gone from Prescott by the end of the year. One afternoon after school, as we were cleaning up the classroom, the teacher’s aide, the replacement teacher (Miss Chaplan), and I talked about whom to select for the study. The decisions were as follows, I selected Donald since he was clearly the highest achiever in the class and I had met his parents at Open House. Mrs. O’Donnell also told me that Donald’s parents were enthusiastic about the study and were hoping to be selected. Such flattery is hard to resist. I selected Carol and Emily because I had met their mothers and observed them in the classroom. I selected Jonathan, although I had not met his mother, because he was the lowest achieving boy. I added Allen in part because both Jonathan and Donald were well behaved and I wanted someone who was more of a troublemaker. Allen fitted that bill. The children represented almost one quarter of the class but, since five of the six mothers volunteered in the classroom, a slightly higher percentage of mothers active in school. After we selected the children I copied down the names and addresses of the families.

With these two sessions the sample was set. At Colton, however, two of the families moved during the summer after first grade before I had interviewed them. In the second year of the study I needed to add two more families, a boy and a girl, one of whom was from a single-parent family. Because I had not anticipated this, I had met the other names and addresses from which to choose. During the next year I visited Colton occasionally, and I discovered that Mrs. Sampson’s second grade class had a white girl, Suzy, who was a high achiever and whose parents visited the school frequently. Her father was a sheriff and her mother was a student. The teacher gave me their telephone number and I contacted them. Because of scheduling difficulties I had only one interview with them, but it was a long one and both the mother and father participated in it (with their eight-month-old girl sitting on my lap for much of the time).

The other child I added to the study, Ann-Marie, was in Mrs. Sampson’s class, and Mrs. Sampson frequently mentioned her in informal conversations. I checked my field notes and I had a lot of notes about her from first grade. I decided to add her in the second year because I had been following her; she was from a single-parent family, and she seemed to exemplify important tensions that can occur between parents and schools. This child was costly, however, as it upset the gender balance and left me with four girls and two boys in the Colton sample. Ann-Marie’s mother did not have a telephone but Mrs. Sampson told me when her parent–teacher conference would be held. So, with a show of confidence I didn’t actually feel, I simply went to the conference and spoke to Ann-Marie’s mother there. She agreed, with no resistance, to be in the study. This scrambling around to add respondents to the study could have been avoided if I had started the year by following a pool of ten or fifteen families, expecting that some would have moved (or dropped out for other reasons) by the second year.

In reflecting on the choice of families, I continue to feel that the children at both Colton and Prescott schools were a reasonably good sample of the classroom. There were no glaring omissions in terms of discipline problems, achievement levels, temperament, popularity, and parent involvement in schooling. At both schools I had a range of parents, from the most heavily involved to the least involved in school site activities and, according to teachers, in educational activities at home. Still, the sample was small and non-random so I cannot confirm this impression.

In addition to the twelve families in my sample I interviewed both principals, the first and second grade teachers at both schools, and the special education teacher at Colton. I interviewed the first grade teachers in the summer after first grade; the interviews with the second grade teachers and the principals were about a year later. The interviews ranged over a number of issues, including teachers’ ideas of the proper role of parents in schooling, and their assessment of the level of educational support which the families were providing for their children. These discussions of the individual children were very helpful; they provided a useful contrast to parents’ assessment of their behavior. At times teachers provided me with information which I would have liked to have asked parents about, as when Mrs. Thompson told me the parents of Suzy, because of body odor. Unfortunately the demands of confidentiality precluded me from probing these issues as much as I would have liked. I did ask parents general questions; if they did not discuss the issue I was looking for, then I simply dropped it. To have done otherwise would have violated the teachers’ confidentiality.

Requesting Interviews

In requesting interviews with parents I followed a different strategy for each school. In a qualitative methods class I took, Lillian Rubin cautioned against writing letters to working-class families asking them to participate in a study. She said that it was usually better to telephone, since working-class families did not read as much nor did they routinely receive letters on university stationery. This advice made sense to me and I followed it. I telephoned the Colton mothers, verbally explained the study and asked permission to visit them in their homes. At Prescott, I sent parents a letter describing the study and requesting their participation. I then telephoned a few days later and set up a time for the interview. These written requests for participation did not go out at the same
time. They were sent out about a week before I was able to schedule the interview. At both schools the requests and the interviews were staggered over a period of several months.

All of the mothers at Colton and Prescott agreed to participate with little hesitation. The fact that I had been in Mrs Walters' and Mrs Thompson's classes seemed to help in gaining access to the children's homes. After I interviewed the mothers at the end of first grade, I told them I would like to return a year later. All of the mothers were amenable to this. At the end of the second interview with each mother, I asked if I could interview the father as well. I interviewed all five Prescott fathers (Gail lived in a single-parent family). At Colton, I succeeded in interviewing only three of the five fathers. Mrs Morris and Mrs Brown were doubtful and reluctant to arrange for me to interview their husbands, and I did not press my request. I regret that now—I think with a bit of pressure I could have interviewed Mr Morris, since I met him at school once and at home once. I never even saw Mr Brown. He never went to school and his wife said that he was very shy. I doubt that, even if I had pursued it, I would have gained his cooperation. In addition, because of scheduling difficulties, I only interviewed Jonathan's mother (Prescott) once rather than twice.

In my telephone conversations and my letters to mothers asking for permission to interview them, I said the interviews would last about an hour and fifteen minutes ('depending on how much you have to tell me'). It turned out that the interviews took much longer; they always took at least ninety minutes and in most cases two hours. I discovered this very quickly and should have changed what I told parents but, again, fearing rejection, I didn't. Now I would. It is a risk but, if it were happening to me, I would be irritated if I had set aside an hour and the interview took two. Furthermore, for reasons I don't completely understand, when I was in their offices or homes respondents rarely told me that it was time to go. Instead, adopting etiquette norms regarding guests, they seemed to want me to wait. It was easy to delude myself and think that the respondents were enjoying the conversation so much that they didn't mind it going overtime, and in some cases that was true. But it was rude of me knowingly to conceal the true length of the interview (even by fifteen minutes to a half-hour) when I made my initial requests. It violated both the spirit and the letter of the notion that a researcher must respect her/his subjects.

**My Perceived Role with Parents**

The Prescott parents did not have any trouble figuring out who I was and what I was doing. They knew what graduate school was, they knew what a dissertation was, and they understood the concept of someone doing research on education without being an educator. Many had friends and relatives in doctoral programs. My general introduction was followed by questions from Prescott parents about my specific academic and career goals (e.g., 'Is this for your dissertation?')

The Colton parents did have difficulty figuring out who I was and what I was doing there. All of the mothers asked me if I was planning to become a teacher. When I said no, that I was working on a research project for the university, I generally drew nods accompanied by looks of confusion. In the beginning I often said I was a 'graduate student'. I dropped that description after a mother asked me if that meant I was going to graduate soon. From then on, I said that the university did a lot of studies and I was working on a research project to find out how families helped children in school. If mothers continued to ask questions about my plans I often took them through a brief explanation of the higher education system: 'After graduating from high school some people go to college. After four years of college people graduate and get a Bachelor's degree. After that, some people go on to more school, do research and get another degree. That is what I am trying to do now.' Overall, I would say that the Colton parents seemed to think that I was friendly, but that I was from a foreign land 'over there', a world they had little contact with and did not understand. Even without that understanding, however, they were willing to participate in the project.

One consequence of this confusion was that Colton mothers mistakenly thought I worked at the school. My efforts to establish myself as being independent of the school took on new vigor after my first visit to Jill's home, which was early in the interviews. I had finished the interview, had packed everything up, and was standing in the kitchen, chatting. Suddenly I saw that on the wall of the kitchen was a calendar, and on that day's date was written 'visit from school'; with the time of our interview. I considered that to be very bad news; it could, and probably did, shape what the mother was willing to tell me. But the interview was over; it was too late to do anything more.

Thereafter, with parents, especially Colton parents, I stepped up my efforts to convince them that I was not from the school by stressing at the beginning of the interview, and repeating it in different ways at different times during the interview, that I did not work there ('Now, I am not from the school and there is something I don't understand very well . . .'). Although I can't be certain, I think these strategies worked; with some probing, all of the Colton parents did express criticisms of the school, although, as I show in the text, they were of a different character than at Prescott.

**The Interviews**

The interviews took place in the homes, in the living room or dining room. The interviews with mothers who worked in the home were often in the middle of the day, the ones with the fathers and the mothers who
worked outside the home took place in the evening or at the weekend. In some cases the houses were quiet; in others children, dogs, house-cleaners, and the telephone frequently intervened. The interviews were open-ended and were set up to be more like a conversation than an interview. I had an interview guide but I sometimes varied the order of the questions, depending on how the interview was evolving. I had a tape recorder and I did not take notes during the interview. Instead, I tried to maintain eye contact, nod frequently, and make people feel comfortable. In the course of these and other interviews, I have discovered that each interview guide has its own rhythm. I have found that there is a particular time (often one eighth of the way through the interview) when the respondent should be 'with you.' If the respondent is not 'with you', it usually means that the interview is in trouble.12

In my interviews in people's homes, I found that within fifteen minutes of my arrival we should be set up and ready to begin the interview. Fifteen minutes into the interview things should be more relaxed; the respondent should look less tense and be sitting more comfortably in the chair; the original tension in the room and interpersonal awkwardness should be easing up; and there should be a sense of movement and revelation. Usually that happened, occasionally it did not. Some respondents (like some students in an examination) never seemed to settle into the rhythm. The situation remained awkward all the way through. In those instances I often discontinued the interview and started another. A few respondents may have felt uncomfortable. I was trying to make the interview seem more like a 'regular person', one that they could talk to easily. In addition, I was interested in hearing them talk about something they cared about and could discuss with ease. That was helpful, for it gave me a sense of the tone and demeanor which I was striving for when I went back to the interview questions.13

Sometimes these conversational diversions, while hardly subtle, did seem to help. Respondents seemed to relax and began to forget the tape recorder. (Noticing that I didn't turn off the tape recorder or apparently mind wasting tape on a discussion of the family dog seemed to help some respondents to relax.) A few interviews—my first interview with Laura's mother at Colton and my interviews with Gail's mother at Prescott—never seemed to 'click' fully. There were good moments followed by awkward ones. For example, when I arrived at Laura's house the television was on and the mother didn't turn it off; in fact, she continued to stare at it from time to time, and comment about it during the interview. It was one of my first interviews in a home and I didn't have the nerve to ask her to turn it off. Now I always make sure that the television is off or, if others are watching it, I move the interview to another room. One of the first things I say after I get set up with the tape recorder is, 'Do you mind if we turn off the television for a while? I'm afraid this tape recorder is quirky and the television really causes problems. It shouldn't be too long'. I also thank them when they do turn off the television and again, when I am leaving, apologize if they missed any of their favorite programs because of the interview.

Considering the number of interviews, having two or three awkward ones was not very many, but I found such occasions to be extremely depressing. I tried to take comfort in Lilian Rubin's comment that it 'happens to everyone'. She confided that, after trying everything she could think of to enliven a failing interview with no success, she would simply finish the interview as quickly as possible and 'get out of there'. Still consider that to be good advice.

Data Analysis

I did two data analyses on this project. The first was half-hearted; the second time I was more systematic as I followed many of the ideas in Matthew Miles and A. Michael Huberman's (1984) very good book on data analysis, *Qualitative Data Analysis: ASourcebook of New Methods*. Fortunately, the results did not change when I analyzed the data more carefully, although the second attempt did highlight themes I had not seen before. Readers interested in data analysis generally are referred to Miles and Huberman. In this section, I simply summarize the steps I took in the two analyses.

In my first effort, I finished collecting the data and then, based on what I had learned, I wrote it up. I felt that I had to portray the data accurately and I carefully reviewed my interviews and field notes. I also drew heavily on the notes I wrote after each interview: a short statement (usually three pages, single-spaced) which summarized the key issues in the interview.14 During this period I transcribed sections of tapes where I felt there were important quotes, making carbon copies of these transcriptions. One copy of these quotes was put into a file, with the folders organized by child; the carbon copy was cut up and glued onto index cards. I also made numerous charts, sketching out the responses of parents to different issues, a precursor to 'data displays'. But the entire process was informal.

The second time I analyzed the data, the analysis was much more comprehensive and systematic. First, I spent hours listening to tapes. I purchased a portable tape recorder (a 'Walkman') and listened to tapes in the house, as I rode my bike, made dinner, and went about my life. In addi-
tion, all of the interviews were transcribed verbatim. It took an average of ten to fifteen hours for me or the secretaries in my department to transcribe a two-hour interview, depending on sound quality. The shortest interview was ten pages, single-spaced; the longest was twenty-five pages, single-spaced. For a few interviews, only critical sections (anywhere from seven to fifteen pages of single-spaced quotes per interview) were transcribed. In all, I had thirty-seven interviews with typed quotes, each interview quite lengthy.

I cut these single-spaced transcriptions up into individual quotes (with a code name on each quote) and glued them on five by eight inch index cards. Colton was yellow, Prescott was white, and the teachers in both schools were blue. I ended up with over one thousand index cards. At first the cards were simply in groups by school and by child. Then they were sorted by basic categories: parents' view of their proper role, their educational activities in the home, and their complaints about Mrs. Walters. I also had categories for family life, including children's lessons outside of school and the social networks among parents in the community. Teachers' cards were grouped according to what educational activities they sought from parents.15

As the analysis continued, I tried to clarify my research question in the light of the literature. In particular I tried to see how my data could modify, challenge, or elaborate known findings. The cards continued to be in piles by major analytic categories (all over the living room floor), but the composition of these groups shifted as I reviewed the quotes, thought about the research question, looked for negative examples, and tried to clarify the differences within the schools as well as between them. For example, during the first analysis I focused on parents' educational activities at home and their attendance at school events. Gradually I realized that Colton and Prescott parents' actions went beyond helping at home. Parents in the two schools differed in how much they criticized the school and supplemented the school program. I also found omissions in the literature on this issue. This shifted the focus from looking at social class differences in parents' support (i.e., how much parents complied with teachers' requests) to the more inclusive notion of linkages.

As I pursued this idea the analytic categories became more numerous: teachers' wishes for parent involvement; parents' beliefs regarding their proper role, information about schooling, scrutiny of teachers, interventions in school site events, criticisms of teachers, educational aspirations for their children; and possible explanations of why parents were—or were not—involved in schooling. Differences between mothers and fathers and the disadvantages of parents' involvement were two other categories.

During this time I maintained index cards about each child in the study. These quickly became inefficient and cumbersome because the case stud-

ies of children were incomplete and I was 'borrowing' cards from the analytic piles to supplement information on each child. Finally I developed a dual system. For each child I had a collection of transcribed interviews on paper for the mother (both interviews) and the father. I also had the interview notes that the teacher had made about the child. These typed interviews were all paper-clipped together and put in three piles (Colton, Prescott, and educators). In addition, copies of all of the interviews were cut and pasted onto hundreds of index cards which were kept in analytic categories within open cardboard boxes (with rubber bands grouping cards in subcategories), and rearranged slightly as the analysis developed. Ultimately the chapters of this book mirrored the boxes of cards.

Following Miles and Huberman, I also made numerous 'data displays'. For example, I created matrices with the children listed in rows and various types of parent involvement in columns (i.e., reviewing papers after school, reading, attending Open House, attending conferences). I also produced matrices on select issues: in one chart I compared the criticisms Colton and Prescott parents had of school, in another I displayed what parents said was their proper role in schooling. The information on the cards duplicated the data displays (on large pieces of poster board) which provided a quick, visual overview of the evidence. Put differently, the cards showed me what I had, as the groups of cards provided stacks of evidence in support of ideas; the data displays showed me what I didn't have—as the cells revealed missing cases or showed exceptions to the pattern. Producing these matrices was time-consuming, but they were very helpful in displaying the strengths and weaknesses of the argument. Together the coding categories, sorting system, and dual system of case studies and analytic categories gave me a chance to look for other patterns, and increased my confidence in the accuracy of my interpretation of the data.

Mistakes: Lessons from the Field

I made one very serious mistake in the field: I fell behind in writing up my field notes. Writing up field notes immediately is one of the sacred obligations of field work. Yet workers I have known well all confessed that they fell behind in their field notes at one time or another. Researchers are human—we get sick; we have an extra glass of wine; we get into fights with our spouses; we have papers to grade, due the next day; or we simply don't feel like writing up field notes immediately after an interview or a participant-observation session. On top of that, at least for me, writing field notes is both boring and painful: boring, because it repeats a lot of what you just did and it takes a long time to write a detailed description of a fifteen-minute encounter/observation; painful, because it
forces you to confront unpleasant things, including lack of acceptance, foolish mistakes in the field, ambiguity about the intellectual question, missed opportunities in the field, and gaping holes in the data. To be sure, there is a tremendous sense of satisfaction in having placed on paper the experiences of the day and then adding these to the top of a neat and growing pile. But the time! Initially, one hour in the field would take me three hours to write up. Missing sessions of writing field notes can, like skipping piano practice, get quickly out of hand ... exponentially, in fact.

If I wrote up my interviews two or three days later, I put 'retrospective notes' (or retro for short) at the top of the first page. In many cases I believe that I could have recreated, even several weeks later, a good account of what happened in the classroom, but I imposed on myself a certain 'code of honour'. If I missed my deadline and didn't write the event up within a few days of its occurrence, I wouldn't allow myself to write it up a week or two later and use my recollections as field notes. I was sure that the information would be distorted. So there were notes that I never wrote up despite my best intentions. My delinquencies multiplied because I didn't stop going into the field; gaining acceptance in the field is dependant on being there and being part of things. The more I went the more interesting things I saw, and the more people told me about upcoming events that they encouraged me to attend (i.e., the Easter Hat parade, a play coming to school). Like a greedy child on Christmas Day who keeps opening package after package without stopping to play with them and then asks for more, I kept going to the field, didn't write it up, but went back to the field anyway for fear of missing something really important. I usually went to the field three times per week (alternating schools), or about a dozen times per month. I don't know exactly how many transgressions I committed. My best estimate is that I completed about 100 hours of observation, with more hours at Prescott than at Colton, and I failed to write notes on about one eighth of my field work. Today I faithfully record in my calendar when I go into the field, where I go, and how long I stay. In my current and future work I want to be able to state, as Lubeck (1984; 1985) did, how many hours of field work the study is based on. This record of visits to the field also helps me keep track of sets of field notes and interviews.

In spite of these omissions I had, of course, quite a large amount of data. I was in the classrooms for several months and had stacks of carefully written notes of routine activities. Many studies (Lightfoot 1983) have been based on far less, but it was a serious breach of field methods and, although I cannot prove it, one that I am convinced is more common than is noted in the literature. In hindsight, the writing up of field notes was linked to the renowned problem of 'going native'. I liked being in the classrooms; I liked the teachers, the children, and the activities—making pictures of clovers for St Patrick's Day, eggs for Easter, and flower baskets for May. I liked being there the most when I felt accepted by the teachers and children. Thinking about taking notes reminded me that I was a stranger, forced me to observe the situation as an outsider, and prevented me from feeling accepted and integrated into the classroom. Writing up my field notes was a constant reminder of my outsider status. It was also a reminder of the ambiguous status of my intellectual goals; I knew only vaguely where I was going with the project. I also worried I might be making the wrong decisions, such as when I began to take a more active role in the class at Prescott or spent most of the time at Prescott during independent time (when Mrs Walters needed help) rather than visiting the classroom regularly at other points in the day. There was a lurking anxiety about the field work: Was it going right? What was I doing? How did people feel about me? Was I stepping on people's toes? What should I do next? —and this anxiety was tiring.

The few times when I forgot about note-taking and observing and just enjoyed being there, I felt a tremendous sense of relief. I liked the feeling of giving up being a researcher and simply being a teacher's aide. The seduction of participation sometimes overshadowed the goal of participation; and the cost was a lack of carefully collected information. If I could do it over, I would arrange things so that I had a different set of choices. I would change my schedule and slow down the project. Although it was advantageous to be in both schools at once, in the interest of completeness I would now probably do one school at a time. I was also in a hurry to get through graduate school, a goal that now seems short-sighted. As a result I have developed what I call the Lareau Iron Law of Scheduling:

Never (and I mean never) go into the field unless you have time that night, or in the next twenty-four hours, to write up the notes.

Such rigidity may seem hard to enforce because presence in the field is critical to sustaining access and rapport. There is also the 'somewhere else' problem (Walford 1987) that something critically important will take place and you will miss it. But whatever happens will often happen again, particularly if it is part of the routine social interaction that qualitative workers are usually trying to study.

This iron law of scheduling can be carried out, it just takes self-restraint. And it is crucial: field work without notes is useless and destructive. It is useless because without documentation the observations cannot and should not be incorporated into the study; it is destructive because worrying about missing notes takes away valuable time and energy from the project, creates new problems, undermines competence, and turns a potentially rewarding process into a burdensome one. In my experience at least, it is not worth it.
A Hybrid Pattern

In most of the classic studies, the researchers were sustained by grants and field work was all they did. Today such full-time devotion to field work is uncommon because difficulty securing full-time funding means that researchers are balancing other economic commitments while in the field. For graduate students, making ends meet often means working as a research assistant on someone else’s project. For faculty, it means continuing to meet teaching obligations while doing field work. Although researchers would love to face only a computer when they leave the field, many in fact must go to committee meetings, write lectures, go to work, pick up children, fix dinner, etc. For many researchers, a hybrid pattern of commitments has replaced the single commitment model of field work that characterized the community studies of the past.

This new hybrid pattern affects the character of field work in many ways. In my case other obligations severely curtailed the amount of time I could spend in the field. I was working twenty hours per week. I had many school obligations, I had to run my own household, and I was living in an area with family and friends in the immediate vicinity. It was often hard to find six to ten hours a week to go to the schools. In addition I felt the strain of straddling two different worlds, I would leave Prescott school and, with my head swimming with thoughts about how I should have handled Allen poking Jonathan, drive to the university, try to find a parking place in the middle of the day, and go to work as a sociology teaching assistant. It was disorienting. Because being in the field requires more formal attire than was the norm among students at the university, I found myself constantly explaining to people I met in the hallway why I was so dressed up. I felt on stage and out of place when I was visiting the classroom, but I also felt myself a misfit at the university. I had trouble getting used to this; it seemed as if I could never establish a routine.

I think that researchers need to take seriously this hybrid pattern of research and analyze the differences it makes in access, entrance, rapport, data collection, and data analysis. It seems to me, for example, that access must be negotiated over longer periods of time, and more often, when the worker is moving in and out of the field than when she/he is living there (see Bosk 1979). Data collection is slower when the researcher is in the field less often, and moving in and out of the field is a strain, though possibly less of a strain than living in an unfamiliar environment for months at a time (Powdermaker 1966). Although data collection takes longer, data analysis and the clarification of the research question may move along more quickly under this hybrid pattern. Being in a university environment as well as in a field setting provides more people with whom to discuss the research question. This ready availability of sounding boards may help the researcher move ahead more rapidly with the data analysis.

Common Problems in Field Work

Whether a commitment pattern is hybrid or single, all qualitative researchers inevitably experience errors and confusion in their research. In the course of defining the problem, negotiating access, beginning observations, and conducting interviews, many decisions must be made, some of which—in retrospect—are regrettable. This is true in all research, but in qualitative methods the mistakes are usually carried out and observed by the researcher first hand (rather than being committed by others and reported—or not reported—to the principal investigator by subordinates). Qualitative researchers also work in naturalistic settings and they lack opportunities to ‘rerun’ the data. Moreover, overburdened by the immediacy of the field setting, the sheer amount of data collected, and the many possibilities which the project offers, some researchers—temporarily or permanently—lose sight of their intellectual question(s). I turn now to a discussion of this problem.

Part II: Problems with the Research Question

Blinded by Data

Two months into my field work, a graduate seminar on participant-observation was offered by Michael Burawoy. Thinking that it might be useful to have others to talk to about the project I enrolled in the course. As I soon discovered, Burawoy (1979) viewed qualitative data as data that tried to help answer a question. He allowed that the mode of inquiry might be very different than the mode of presentation in the final report, but he was interested in having us—all of us—answer sociological questions. ‘So what?’ was the question of the quarter.

As Burawoy soon discovered, I resisted this approach. More precisely, I was ambivalent and confused about how to write up the data I was collecting, which grew, literally, by the hour. Data collection is an absorbing process and it pleased me to add more and more field notes to the pile and make arrangements to complete interviews. Still, the sheer amount of data sometimes seemed overwhelming and I did not feel prepared to analyze it. I had unconsciously accepted the methodology of survey research which consists of four steps: a) formulate a problem; b) collect data; c) analyze it; and d) write it up. I was overextended simply trying to get to both schools, take notes, write up the notes, work as a teaching assistant, and keep up with Burawoy’s class. As far as I was concerned, the analysis could wait.

My ambivalence, however, centered less on the problem of not having time to do it and more on the proper strategy for analyzing and writing up qualitative research—a problem which ultimately haunts almost all qualitative researchers. I wanted to describe social reality, to supply the
details and the vivid descriptions that would draw my readers in and carry them along; I hoped to produce the holistic and seamless feeling of many of the ethnographies that I had read. Some of the ethnographic works in this genre are analytical. Tally's Corner (Liebow 1968), Worlds of Pain (Rubin 1976), and Everything in Its Path (Erikson 1976) all have arguments—but the analysis seems subordinate to the data. They certainly aren't written in the 'now-I-am-going-to-discuss-three-ideas' style which characterized everything I had written during graduate school. Captivated by some of the ethnomethodology and anthropology I had read, I was eager to abandon explicit intellectual questions and 'simply' describe social reality. More to the point, I believed that was what good ethnographers did. Describing reality provided intrinsically interesting information. The intellectual ideas, tucked away in a concluding chapter or footnote, did not spoil or constrain the novelistic portrayal of reality. I had hoped to use my own data to draw compelling pictures which would not—to use a favorite expression of mine at the time—violate the complexity of social reality. But I was also interested in ideas. I had waded through Bourdieu and found his approach useful. I was genuinely interested in the way in which social stratification was reproduced, and in the contribution made to children's life chances, by the interactions between parents and teachers.

As my field work progressed I struggled to determine the 'proper' relationship between theory and qualitative data. I had framed a question before I began my field work, but once I got caught up in the drama of actually being in the field my original question became hazy. I had trouble linking the data back to the original question or modifying the original question. Instead, I was preoccupied by the characters—Mrs. Walters, Mrs. Thompson, the children, and even my own role in the research process. This intellectual confusion is reflected clearly in my field notes. My notes—and I know that I am not alone in this—had some sensitive concepts (Glaser and Strauss 1967) but then were all over the map. They were a hodgepodge of observations made on the basis of shifting priorities. One day I recorded the curriculum and how children interacted with the materials, their skills and how they displayed them. Another day I looked at how the teacher controlled the classroom and her methods of authority. Another day I looked at my role in interacting with the children, how I responded when children started breaking classroom rules in front of me, and my relationships to the teacher and the aide. Observations on the relationships between the aide and the children, the aide and the teacher, the parents and the children, all flow indiscriminately through my field notes. I wrote detailed descriptions of special events (e.g., a school play, a description of an Easter egg dyeing project). I also watched for and noted hallmarks of social class: labels on clothing, vacation plans, parents' appearances in the classroom, and different relationships between parents and teachers. Anything and everything that went on in the classroom I tried to record. In my efforts to capture social reality as comprehensively as possible, I forgot about the need for a focus.

Burawoy had no such memory lapse. He read a sample of my field notes and promptly advised me to narrow my interests. He also asked me (as well as the other members of the class) to spend a paragraph or two at the end of each set of field notes analyzing what was going on in the notes. After each session of observation, we were to write out our notes and then evaluate them in light of our question. We were expected to assess what we had learned, what new questions had been raised by our observations, and how we planned to proceed. Burawoy's advice was excellent. Today, I make my graduate students do the same thing, but, as with much, if not most, good advice (i.e., to lose weight or stop smoking) it was easier to give than to follow. I found the required analyses extremely difficult to do. I hated them. Worse yet, I did them only when I had to—the ten times I was required to give them to Burawoy.

Part of the reason that I avoided these analyses was that they highlighted the murkiness of my intellectual purpose. Methodologically I was cleaner: I wanted to provide a rich description of social reality. The problem was that my experience with ethnomethodology didn't help me frame my research question in a way that would allow an answer that made a theoretical contribution. I was asking, 'How does social class influence children's schooling?' The answer was supposed to be a description of social reality. What I lacked was another, more conceptual, question: 'Do these data support one interpretation and suggest that another interpretation is not as useful?' or 'Can we understand parents' involvement in schooling as being linked to their values? Does cultural capital provide a better explanation for why parents are involved in school?' These questions have 'yes' or 'no' answers which can be defended using data from the study. By framing a 'how' question I could not provide a similarly defensible answer. I could not show that one explanation was superior; I could not demonstrate that these data helped to address an important issue. In short, I could not answer the 'So what?' question.

At the time I did not really understand the implications of posing the 'wrong' question. I analyzed my notes as rarely as possible and I didn't really notice that my goals changed hourly. I was more focused on building rapport with the teachers, taking comprehensive notes, trying to get the notes typed up, and getting permission to interview parents and teachers so I could complete the next stage of the project.

Burawoy, however, was concerned about the way I framed my study. He expressed this in all of our meetings. From our first discussion (ol-
lowing his review of my field notes), he repeatedly cautioned me to think the study through in greater analytical detail'. This advice sailed right by me or more accurately I ignored it. In the sixth week of the quarter I wrote a paper on what I had learned from my observations. It was long and my first effort to assess what I had learned from almost five months of research. It was all description: how teachers at Prescott and Colton looked, how they interacted with the children, how much math the children knew, where the children took their vacations, and a little about children's feelings about their academic ranking in the class. I discussed parents, noting that Colton parents were rarely there, seemed more deferential, and didn't seem to know as much as Prescott parents. The paper was vivid in parts and dull in others but it didn't define a question. It was an unfocused description of classroom life in two schools.

Burawoy's reaction to the paper, strongly worded and highly critical, proved to be the turning point in the conceptual development of the project. His comments made it clear that I could not continue to conduct a study that posed no problem and articulated no argument. He noted:

One's reaction to what you have written has to be, so what? What is so surprising? At no point do you attempt to present plausible alternatives to your findings. I would like to see you produce a theoretical beginning to this paper. I want you to use the literature to highlight the significance of the data you have collected. I really think you have to develop an argument, particularly as I presume this will be part of your thesis.

The chair of my dissertation, Troy Duster, gave me the same feedback although in a different way. Slowly I began to realize that quotes and field notes (which I found fascinating of course) would have to be applied to an intellectual problem. An unfocused thick description wouldn't do.

Using my original formulation of the problem and my conversations with others in my department, I began to try to link up the data with the intellectual problem. I wrote another, much shorter, paper noting the significant correlation between social class and educational achievement and arguing that this correlation was linked to parent involvement in school. This attempt was, as Burawoy commented, 'a major advance' over my earlier paper, but I still had a long way to go.

In retrospect, part of my problem was that the question I was framing was too heavily embedded in quantitative models. I was trying to unravel the way in which class difference in family life influenced schooling and shaped achievement. I seriously thought I could provide some kind of causal model using qualitative data. Today, that goal strikes me as outlandish. The strength of qualitative data is that it can illuminate the meaning of events. It cannot demonstrate that parent behavior 'a' has a stronger effect on achievement than parent behavior 'b' in a sample of two classrooms.

This preoccupation with achievement as a dependent variable and steady immersion in the quantitative literature made me overlook qualitative sociological studies that could provide a suitable framework for my project. I had not read many of the socio-linguistic studies that had been done in the United States, nor was I familiar with the work of cultural anthropologists. I unwittingly ignored the work of potential role models—people who had used similar methods successfully and whose studies could provide valuable examples.

I also failed to realize that just as an individual develops a personal identity most researchers develop an intellectual identity, one that often includes a theoretical as well as a methodological orientation. This identity does not usually change significantly over a single research project, although it might be modified in some ways. I began my project admiring radically different types of qualitative research: my own intellectual identity was in flux. I failed to realize that my multiple admissions were prompting me to strive for mutually incompatible goals. This was not, I have come to realize, an idiosyncratic pattern for I have observed many novice researchers do the same.

For example I admired many ethnological studies in which the flesh and blood of real life is portrayed in vivid detail. Yet most of these studies emphasize that it is critical that the researcher's description remains true to the actor's subjective experience. I do not embrace this view. I believe my respondents should be able to agree that I have portrayed their lives accurately, but I do not want to restrict myself to 'folk explanations'. It does not trouble me if my interpretation of the factors influencing their behavior is different from their interpretation of their lives. Parents at Prescott and Colton schools cannot be expected to be aware of the class structure of which they are a part, nor of the influence of class on behavior. I want to be able to make my own assessment, based on the evidence I have gathered and my understanding of social structural factors. It is difficult, if not impossible, to provide a detailed, comprehensive portrayal of social reality (particularly using the actor's subjective experience) which also selects out elements of that experience to build a focused, coherent argument. A comprehensive portrait and a focused argument are different goals. As with many things in life, you cannot do everything. You have to choose.

This is why it is very helpful for a researcher to know her/his intellectual identity at the beginning of a research project. If you know what you believe in, what type of work you are trying to do, what you would consider acceptable and what you would consider unacceptable, you have a framework and general parameters for your research. You are also better prepared to make compromises: what kinds of weaknesses in your research are you willing to live with and what are completely unacceptable? Being clear about matters such as these can improve both the qual-
ity and the quantity of data collection. Well-defined, mutually compatible goals make it easier to focus in the field and also contribute to better organized data.

The Lone Ranger Problem

Even with a clear intellectual identity and a general theoretical question, almost all research questions undergo modification in the light of the data. A favorite description for this in qualitative methods courses is that the research ‘evolves’. Many researchers adopt the myth of individualism here. The lone researcher collects the data and, aided by her/his powers of sensitive observation and skill in writing up field notes, the researcher’s initial question ‘evolves’ and becomes more focused. After having collected the data, the researcher retreats into her/his study to write up and then emerges with a coherent work.

This is a mistaken view of the research process. Research, like everything else, is social. Ironically, this is more obvious in the physical sciences, where researchers must share expensive laboratory equipment, than it is in the social sciences. In the physical sciences, faculty, post-doctoral fellows, graduate students, technicians, and (occasionally) undergraduate assistants all share the same work space—and equipment. Lab interactions and lab politics are a routine part of the work process. Social scientists, even those collaborating on large research projects, rarely work together in such a way. Usually the research team meets periodically for a couple of hours and co-workers may share a computer or an office, but they spend much more of their work time alone than do their colleagues in the physical sciences. Still, the research process in sociology is social. Researchers do not get ideas from vacuums; they arise from interactions and discussions in the lab and the hallways.

In my own case, my argument (and the relationship between the conceptual model and the data) went through four or five stages, becoming narrower and less sweeping at each point. As the question became clearer, my data collection became more focused as well. I began to collect information about parents and looked closely at the differences between the two schools. I ultimately dropped my effort to explain achievement and developed an interest in the debates on cultural capital and, to a lesser extent, parent involvement in schooling. To say that my research question ‘evolved’ is true, but this is far too passive a description. Just as reproduction of the social structure does not happen automatically, so the narrowing and refining of a research project is not an automatic process. Qualitative researchers take steps to produce a more focused research question. Participant-observation, writing up field notes, and reflecting on field notes are the steps which are normally emphasized in the literature but are not always followed by some kind of effort to push the question forward. This can be a one hour conversation with a colleague (by telephone if necessary), a comparison with other studies, or a long memo which is then reviewed and criticized by others. Such efforts must include reflections on the overall goals of the project, the theoretical question, the data, and the remaining gaps. The analysis at the end of field notes and this ‘state of the question push’ are similar but not identical. The former is focused on a particular event or dynamic in the field setting; the latter is broader, more reflective, and—most importantly—more social. It is an effort to reach out and place the study in a social context, to get others’ feedback, to evaluate the study in terms of its contribution to the field. It is not usually very difficult to arrange this social interaction, but it must be solicited by the researcher; it will not happen automatically.

Thus, all of the conceptual advances in this project were linked to the production and criticism of written work. Writing was helpful because it required that I organize, systematize, and condense volumes of information. It helped me struggle to build the argument and it allowed me to assess the evidence in a new way. The criticism of others, particularly the comments of colleagues around the country, challenged me to rethink some of my ideas. Although I had many enjoyable conversations about the project and bouncing ideas around, I learned less from talking and listening than I did from writing. One consequence of this is that every few months or so (depending on the pace of data collection) I write a paper about my current project. (A deadline, such as giving a talk about the research, is helpful here.) These working papers are not polished and in most cases are not publishable.

Overall it was the social interaction (especially the criticism from others) that helped advance my work. While the lone scholar image has its appeal, it does not accurately portray the actual process in qualitative—or quantitative—research.

Writing It Up

After I signed a book contract and was committed to finishing this project, I began to ask colleagues who did qualitative research what books they
considered exemplary models of writing up a qualitative project. I was
shocked at how much trouble people had thinking of exemplary books.
Moreover, when they did recommend books they were not usually within
the field of the sociology of education. Several people recommended
Tally's Corner (Liebow 1967). One colleague recommended Charles Bosk's
book Forgive and Remember (Bosk 1979), a study of the socialization of med-
ical residents into surgery. It is a compelling book and, I believe, a useful
model. Another suggested Everything in Its Path (Erikson 1976), an award
winning book which portrays the destruction of a community by the fail-
ure of traditional support systems following a dam burst.

When I began to reflect on the books that didn't make the list (only 99
percent of the available literature), it became clear that there were many
ways that qualitative researchers could end up producing mediocre
books—even those beginning with interesting ideas and good evidence.
Many studies represent good solid work but they have a plodding tone
and analysis; they lack lively writing. Others seem as though the au-
thor(s) had not accurately represented the community under investiga-
tion and/or had missed important things in field research. Some books
had good ideas and an interesting argument but seemed to be unsyste-
matized in the analysis and portrayal of evidence. Others were long on ideas
and short on data, while some lacked an argument altogether.

The downfall of many of these books lies in their failure to integrate
theory and data. In my own case, as I began to try to write up the results
of this study, I would careen rather abruptly from discussions of theory
and the research problem to presentation of the data. I also presented
very few quotes. Detailed—and negative—comments from reviewers
helped me see the error of my ways. Mary Metz, a guest editor for Sociol-
ygy of Education, summarized the complaints of reviewers, complaints
that I have echoed in my own reviews of other manuscripts using qualita-
tive methods:

You need to work with your data and decide what can be learned from it and
then present your theory tersely as it will help us understand those findings
and put them in context.

The reviewers also complained that I made sweeping generalizations
without enough evidence to back them up, another common problem in
manuscripts based on qualitative research.

I used the reviewers' and the guest editor's criticisms to improve my
dissertation. I cited and used more qualitative research and I worked to
change the focus from a heavily theoretical piece to a more empirically
grounded one, but problems remained. I over-shortened the literature re-
view and the quoted material was not integrated with the text. Following
Aaron Cicourel's advice, I labored to integrate the data with the analysis,
supply more data and be more 'aggressive' in showing 'what is missing
empirically and conceptually' from other studies.

Cicourel's advice was useful again as I prepared to write this book; it
reminded me to use the data to build an argument. Nevertheless, while I
knew that adding more data would strengthen my argument, I wasn't
clear how much additional data to include. I had an urge to add almost
everything. Finally, in a move of some desperation, I turned to books and
articles that I admired and counted the number of quotes per chapter or
page; most averaged one quote per printed page. The quotes were not
evenly spread throughout the chapters; there would be pages without
any quotes and then three or four quotes per page. Most of these studies
also provided examples in the text. Of course the right number of quotes
depends on many factors, but the count gave me a ball park figure for my
own writing which I have found useful. The problem of linking theory
and data is an ongoing struggle. I made a rule that every chapter had to
have an argument. I also remembered, although I did not always follow,
the advice that someone passed on to me that every paragraph should be
linked to the argument. I tried to show that my interpretation was a more
compelling way of looking at the data than other interpretations. In other
words I tried to answer the question, 'So what?'

It will be for others to judge how well I have done in connecting the
theoretical argument and the research data. I know that I have done a bet-
ter job of integration with this book than I did with the written work that
preceded it, notably drafts of papers and the dissertation. I used almost
none of my dissertation in preparing this book. Instead I began again,
adding probably three to four times as many quotes and streamlining and
increasing the aggressiveness of the thesis. This pattern of modest im-
provement in linking theory and data gives me hope: maybe experience
will help. In fact a comparison of first and second books does suggest that
some people get much better at this as they go along; others however do
not, and a few seem to get worse.

Reflections on the Making of Home Advantage

This project had its share of mistakes but it also had its successes. The de-
sign, which included interviews with both parents and teachers, is un-
usual as most studies do one or the other. This yielded insights that
would not have been possible if I had studied families or schools. It was
also helpful to follow children over time and clarify that parents adopted
similar modes of interaction regardless of the teacher. It was very impor-
tant to supplement the interviews with classroom observation which im-
proved the interviews and enabled me 'to ‘triangulate' in a way that
would have been impossible with interviews alone.
In the end I did have a good rapport with the staff, particularly the classroom teachers I worked with most closely. On her last day of school Mrs Walters gave me a hug goodbye; Mrs Thompson thanked me warmly for being in her classroom. In both schools children ran up and gave me a goodbye hug on the last day of school. By the end of the interviews I felt I had genuinely come to know and enjoy many of the mothers and fathers, and I was also certain that in several cases the feelings were mutual. This was a reward. There are plenty of awkward moments in field work, even among the best researchers, but there are also rewards and signs, little and big, of acceptance. These are important to notice and remember. This is harder to do than one would think. Moments of foolishness and the damage they have wrought are easy to worry about. I spent a lot of time fretting about the mistakes I made in this study. They scared me so I wanted to try to hide them; I worried about each and every one of them, and they overshadowed my assessment of the project. This kind of self-criticism, in which the impact of each criticism is five times that of each compliment, is not productive.

It was productive, however, to spend time thinking about the strengths and weaknesses of the study and the confidence which I have in the results. As this appendix and the format of the book make clear, I have confidence in the validity of the interviews. I feel that I was helped by my previous experience as an interviewer. Although it is difficult to prove, I am confident in the quality of the data—that I did not lead, badger, or trap respondents in interviews, that I listened to them carefully and was able to get them to talk in an honest and revealing way. The field notes were also carefully recorded. When I went into the field I thought I would find evidence of institutional discrimination. I thought, as Bowles and Gintis, Cicourel and Kitsuse, and others had suggested, that the teachers were going to differ significantly in their interactions with parents of different social class. I did not find evidence to support this position. When I did not find it I looked for other explanations rather than trying to force the evidence into that intellectual frame. The project did not have as many field notes focused directly on the intellectual problem as I would have liked, but the ones that were there were carefully done.

Can we learn anything from a study of two first grade classes, twelve families, four teachers, and two principals? Yes, I think we can use a small, non-random sample to improve conceptual models. This study shows that a very high proportion of parents would agree that they want to be 'supportive' of their children's schooling but that they would mean very different things by this. It suggests that family-school models are inadequate. Researchers do not spend enough time addressing the differences in objective skills which social class gives to parents. Independent of parents' desires for their children, class gives parents an edge in help-

ing their children in schooling. My confidence in the validity of the findings is bolstered by the fact that they elaborate a pattern that has been noted by many researchers, although often only in passing. They also mesh with the conclusions of other recent works (Baker and Stevenson 1986; Stevenson and Baker 1987; Epstein 1987).

Although not a form of systematic evidence, I must add, that just as after you learn a new word you see it everywhere, after I finished this study I began to notice that social class differences in family-school relationships are as evident in the Midwestern city where I now live and work as it was in the West Coast communities I studied. I see working-class neighbors and friends take a 'hands-off' attitude toward their children's schooling; emphasizing their own inadequacies and turning over responsibility to the school. I see upper-middle class families, particularly academic couples, trying to monitor and control their children's schooling. I think that while there may be aspects of the argument that need modification, the overall pattern, that class gives people resources which help them comply with the demands of institutions, is really there. Other research, using multiple methodologies, is necessary to establish that and to illuminate the interactive effects of class and parent involvement; for example, working-class parents are much less likely to make requests of the school staff, and when they do make such requests are more likely to have them honored than upper-middle-class parents.

What this study cannot do is provide an assessment of how important individuals' competencies are relative to other factors influencing parent involvement (i.e. values, teachers' roles), nor can it evaluate how common parents' actions are; including parents' supervising teachers and compensating for weaknesses in the classroom. A small sample imposes restrictions that cannot be surmounted with felicitous phrases such as 'one half of the sample believed...'. Large-scale, representative studies are much better for describing the proportion of people who share certain beliefs, and internal variations, while addressed here, can be better elaborated with a larger group. What qualitative methods can do is illuminate the meanings people attach to their words and actions in ways not possible with other methodologies. Although I admire the many quantitative studies, they are in some ways 'unnaturally' straightforward. Data analysis and computer analysis have a much smaller range of options and there is less of a domino effect than occurs in qualitative work. Quantitative research does not have the ambiguity and uncertainty of field work.

In my view qualitative work is more cumbersome and more difficult than survey research at almost every stage: formulation of the problem, access, data collection, data analysis, and writing up the results. It is more time consuming; it is harder to spin off several publications; and, to add insult to injury, it is considered lower status by many members of the pro-
fession. But it adds to our knowledge in a critical and important way. It is that pay-off that draws me back, despite all I have learned about the enormous commitment of time and energy that qualitative research demands. If it were not one of the only ways of gaining insights into the routine events of daily life and the meaning that makes social reality, qualitative methods would not have a lot going for it. It is too much work. But it is one of the only ways, and possibly the only way, to achieve such insights. The usefulness of these insights rests, however, on the character of our research. Exchanging notes on our disappointments and successes in field research is an important step in increasing the quality of our work.

Notes

1. I am indebted to William F. Whyte's work not only for the idea of writing an appendix but also for providing a model of how to write one. I have shamelessly adopted elements of his organizational structure, including this one, in my appendix. Readers will note, however, a difference in the content and goals of the two appendices. Whyte's appendix elaborates issues of access, entry, and the formulation of the intellectual problem. He also provides a very good discussion of ethics and holding the line between researcher and native. My goals are somewhat different. Although I briefly review the issues of access and entry, my focus is on the practical considerations of data collection, data analysis, and the writing up of the results. I do, however, also discuss the task of formulating an intellectual problem in qualitative research.

2. My job was to help determine if recently arrested defendants were qualified to be released on their own recognizance. To help indigent defendants save bail money, the Own Recognition Project (OR Project) would prepare cases by providing a summary of the social ties a defendant had to the area, including her/his correct address, contact with relatives, and employment history. Unlike bail, which was simply a matter of producing the money and the collateral, OR cases required judges' signatures. Primarily because of negative publicity, many judges were very reluctant to exercise the OR option. Although the San Francisco City Prison was not as bad as some prisons, most people found prison so uncomfortable that they wanted to get out as soon as possible. For them OR was too slow and too chaotic so they bailed out instead.

3. Unfortunately for those of us not trained in shorthand, it is not possible to write down every single word and idea in an interview, particularly if you are trying to maintain eye contact and build a rapport with the subject. Without a tape recorder researchers must do some editing while taking notes. For most of us this means that some particularly interesting passages are written in more detail than others. Yet what is considered interesting changes as the project and the research question develop, thus note-taking is inevitably altered by these intellectual questions.

4. Whyte (1981) has a good discussion of the problems of access, but almost all books on qualitative research methods discuss the problems. The writing on qualitative methods has increased radically in recent years and there are many good pieces around. Bogdan and Biklen (1982), while directed at research in education, is a useful overview. Other works include Silverman (1985), Agar (1986), and from a somewhat different perspective Glaser and Strauss (1967). Although older, Schatzman and Strauss (1973) provide a succinct discussion of key issues. In more specialized discussions, Gorden (1987) focuses on interviewing, Kirk and Miller (1986) the problems of reliability and validity in qualitative work, Maciorie (1985) the task of writing up one's results, and Punch (1986) on the politics of field work. Erickson (1986) also has a useful overview of the steps in a qualitative research project using studies of teaching as an example. Finally, for reflections on the research process, see Robinow (1977), George and Jones (1980), van Mannen (1988), Simon and Dippo (1986), and Schon (1987).

5. As part of the human subjects approval process at the university, I wrote consent forms for all of the parents, children, teachers, and others I interviewed. [Since I was not disrupting the classroom activities, I was not required to gain consent forms from all of the children in the classes.] These forms briefly described the goals of the project and the methodology, including that parents and teachers would be interviewed. Before I gave parents and teachers the forms I stressed that these forms were routine and added that they were developed after serious abuses by researchers, such as prisoners being given drugs without being told. Although I agonized over the content of the form almost no one read it. Only two parents—a lawyer and his wife—read the form carefully before signing; the remaining parents and educators signed it with only a glance.

6. My letter to Prescott was as follows:

Dear Mrs. Finnegan:

This letter is in regard to our recent telephone conversation regarding my request to conduct a small research project in your district. As I mentioned, I am a graduate student in a doctoral program at University of California Berkeley, in the sociology department. As part of my dissertation research, I am conducting a study on social class variations in the family-school relationships for young children. As you probably know, the social standing of a child's family is a key predictor of educational outcome. The purpose of the research is to examine the process through which social position affects the educational process. In particular, the research will focus on the impact which the social position of professional-middle-class and working-class families has on day to day experience of school life.

I would like to conduct a very small pilot study on these issues in Prescott School District. The research would involve one first grade classroom in your district. The study would include interviews with the teacher, principal, school secretary, and five families of the children in the classroom. In addition, I would like to observe the children in the classroom for a short time, perhaps amounting to six or eight visits. All of the interviews would be 'semi-structured' interviews with open-ended questions. The interviews would last a little more than one hour and would be tape recorded. All of the persons in the study would be assured of confidentiality.

The interviews will cover a number of issues in family life and school life. The study will ask both parents and the child questions about the family's
approach towards schooling. The parents' view of schooling, the way in which the parents convey this view to the child and the behavior of the parents will be explored. In addition, the conflicts between parents regarding education and the proper type of educational experience will be studied. The purpose of this study is to compare differences between working-class and professional-middle-class families in their view of the ideal family-school relationship. The interviews in your district will provide a basic description of the family-school relationship for a small number of families of relatively high socio-economic status.

A slightly different set of issues will be taken up with the teacher, principal, and school secretary (the secretary is included as the front office often is the first point of contact between families and schools). First, it is important to note that I would like to request that the school send a letter to the families indicating that the researcher has the permission of the district to conduct the interviews. I would be happy to contribute in any way possible to the writing and mailing of such a letter.

Secondly, the interviews with the teacher, principal, and school secretary will focus on the amount of information which school personnel have about family life. Questions will focus on the types of information which school staff learn about families, and the informal ways in which this information is gathered. In addition, the researcher will solicit the perceptions of school staff regarding the way in which family life shapes the day to day educational experience for young children. It is important to emphasize that the purpose of the study is not to evaluate teachers, schools, or parents. Indeed, the specific teaching style of a teacher is really of very limited interest as the study seeks to understand social class patterns of family-school interaction.

These brief comments are intended to provide you and your colleagues with better insight into the concerns of the research project. If you or anyone else in the district has further questions, I would be happy to provide additional information. I appreciate your consideration of this request and look forward to hearing from you in the future.

Sincerely,

Annette Lareau

7. With hindsight, this is the letter I would write today:

Dear Mrs Finnegar:

Thank you for taking the time to speak with me the other day. As we agreed, I am writing to request permission to conduct a study in your school district.

In this project, I am interested in learning more about how families help children in school. I would like to visit one first grade classroom in the school district on a regular basis this school year (e.g., two times a week). My visits would be scheduled to be at a convenient time. Having worked in classrooms, I know how important it is to take an unobtrusive role in the classroom. I would be happy to work as a classroom volunteer if the teacher would like.

In addition, at a convenient time, I hope to interview the parents of five children in this classroom, as well as the teacher, principal, and school secretary. The interviews will last an hour or so. All information collected would be kept confidential; neither the identity of Prescott school district, nor that of any parents or teachers, would ever be revealed.

I am requesting permission to observe in the classroom and for you, or the school staff, to supply names and addresses of parents, with the understanding that parents may refuse to cooperate in the study. For your information, I have attached a sample copy of the letter which I would mail to parents.

I know that you, and the teachers, lead busy lives. Teachers have reported that the experience of working on this research project was interesting and pleasant. If it would be helpful, I would be happy to make a brief presentation about the project to school staff. If you would like any other information, please feel free to contact me at (615) 453-2494.

Again, I appreciate your consideration of my request. I look forward to hearing from you in the future.

Sincerely,

Annette Lareau

8. I always told people that there was another school involved and that the school was of a different level of affluence. In the beginning I used the term ‘socio-economic status’; that really raised eyebrows. I now realize that it is much too long a term and much too academic to be useful.

9. Having come from Berkeley I found this ‘Miss Lareau’ title to be astounding in the 1980s, but it happened in all of my interactions in the school. No one called me Miss Lareau, and many people asked me: ‘Is it Miss or Mrs?’ Unmarried teachers, including Miss Chaplin, used the term Miss in all of their interactions. It didn’t really bother me; however, and I never asked to be called Ms. I didn’t really care what they called me. I was just glad to be in a school doing field work.

10. I met several mothers, including Allen’s and Emily’s, during these periods. As children’s work got underway the mothers would often chat with me and ask me questions about my study. They also observed me in the classroom and my interactions with the children. Mrs. Waiters often complained about mothers visiting during volunteering saying, ‘You get more work out of one parent than two’. In my own case it meant that mothers were watching me just as they watched Mrs. Waiters. There were also indications that mothers discussed me and my study in their conversations with one another. Thus my role with parents paralleled that of the teachers; Prescott mothers knew more about me, scrutinized, and questioned me much more closely than Colton parents.

11. If there was a statement which I thought was important I would repeat it to myself over and over again while in the classroom and write it down immediately after I left—usually in my car before I drove away. Most of the field notes from Prescott do not have direct quotes; if there are quotes, however, I am quite confident of their accuracy.

12. While interviewing defendants in City Prison for the OR Project I found that by two or three minutes into the interview I needed to have the defendant calmed down, no longer trying to tell me the story of his or her arrest, and concentrating on the names of three persons (with telephone numbers) who could act as references, otherwise I felt the interview was in trouble. This ‘transition point’, therefore, varies from study to study, depending on both the length and the substance of the interviews.
13. Although I believe I was almost always genuine in my admiration for aspects of the respondents’ lives, the content of my compliments and ‘fishing expeditions’ varied according to social class. In Colton I found myself discussing television programs, admiring respondents’ house plants and, to a lesser extent, their clothes. In Prescott I talked about classical music preferences, houses, and house decorations.

14. In these summaries I wrote a description of the respondents, the house, and key parts of the interview. I also listed critical quotes and their location on the tape (i.e. ‘good quote about criticisms of school, end of side one’).

15. These categories had been the analytic structure of my dissertation which had seven chapters: 1) a literature review and statement of the problem, 2) a description of the research methods, 3) a description of the two schools and the amount of parent involvement in each school, 4) parents’ attitudes towards their role in schooling and the degree to which they complied with teachers’ requests, 5) family life (i.e., lessons, gender roles, kinship ties) and the influence on family-school relationships, 6) teachers’ wishes for parent involvement, and 7) the importance of cultural capital in shaping family-school relationships.

16. The class had a distinct (and very effective) structure. We were divided into groups of four, in roughly similar intellectual areas. We were to meet twice a week outside class to compare and discuss each other’s field notes and problems in the field. Twice during the quarter we made presentations in class and shared our field notes with the entire class. Burawoy also read our field notes and commented on them. Course requirements included a critical literature review to help formulate a problem, a paper based on the field work, and ten sets of field notes.

---

Epilogue: A Selective Guide to the Literature

Research using ethnographic techniques has become fashionable. In the past twenty years there has been a dramatic increase in the number of published works in the field. For persons who want to get familiar with major issues in the field but have limited amounts of time for training, the literature can be overwhelming and frustrating. In this short, closing piece, we offer a friendly guide to this literature where we briefly discuss works we have found helpful or have known to be helpful to others. We stress the selectivity and, in some respects, arbitrary nature of the guide. We are certain that there are excellent works omitted; pieces we have found helpful may lack usefulness for others.

Textbooks


In this thin textbook department, there is the widely used (and recently revised) piece by John Lofland and Lyn Lofland, Analyzing Social Settings (1984), which provides a nice summary of the key issues in the field. An older but still handy small book is Field Research (1973) by Leonard