In safe, comfortable spaces, such as in dark bars late at night, ethnographers and other qualitative sociologists often confide to each other about the remarkably slow pace of their work. Often talented researchers have been ashamed of being so slow. Yet, it is normal for a project to take years. I remember when I started the study that became Unequal Childhoods, other sociologists would ask me about it when we met at conferences; they expressed interest and enthusiasm about my planned book.¹ As the years ticked by, people looked more uncomfortable when they asked me about my book. (This was also probably because I looked embarrassed that my book was not done.) Then, a few years later, they stopped asking! I would meet people at a conference and they would ask about my teaching, my family, or the conference, but they wouldn’t even mention the book. It was too embarrassing. There was a charming essay in the New York Times in 2004 about authors who take years to write a book. One author said, “Once the book is out you go from ‘What a chump she can’t finish that book’ to ‘Wow, what an incredible journey.’”²

Sometimes I think that sociologists interested in naturalistic studies of daily life have ceded too much ground to quantitative sociologists. This
is not to say that quantitative sociologists aren’t smart, interesting, and thoughtful. But the labor process is different. Although there are scholars who collect survey data, most quantitative scholars do not spend any time collecting data. Instead, the data set is given to them. In addition, the data set is established. There are a finite number of variables. The options are limited. Quantitative research also seems much more predictable than doing qualitative work. The pathways are established. This is not to say that quantitative scholars don’t hit brick walls, have perplexing findings, or have findings that inexplicably vanish with the introduction of a new variable. It takes a long time for a researcher to get to know a complex data set. In addition, there is always the terror in number crunching that you might make a small error that would radically change the results. (It has happened.) But the key point is that the process of doing quantitative research is more predictable. It is also faster.

And qualitative work is chaotic as well as slow. Most of the time that I was working on Unequal Childhoods, I had no idea what I was doing. On paper it looked like I knew what I was doing. After all, I had written a grant proposal to the Spencer Foundation (which had graciously given me, as an advanced assistant professor, a major grant). I had read a fair number of books and articles on the relevant topics. I had completed an ethnographic study as my dissertation, published a heavily revised version of the thesis as a book, and even won an award for the book. Thus, you might think that I should have felt like I knew what I was doing. But I didn’t. Instead, I was stumbling along. Each situation was somewhat unpredictable. For example, each family that we observed had different ways of organizing their life; we needed to adjust to them. In addition, although I hoped to learn interesting things from the observations, I wasn’t exactly sure about what I would learn. Even if you have a lot of years under your belt, the course of the research in each project is far from certain. This unpredictability makes the work exciting or terrifying depending on your perspective. Gaining access and maintaining access are huge hurdles; yet access is being constantly renegotiated. Multiple people need to be told about the study as you go along. And you need to build rapport with different people. As you get deeper into the study, you face new challenges. Sometimes people don’t want you around. For example, one of the fathers in my study for Unequal Childhoods, Mr. Williams, was supremely unimpressed with the project when we visited his family regularly. As he told his wife somewhat angrily at one point, “It is your thing.” Ms. Williams laughed merrily when he made
this proclamation (as if he was being a recalcitrant child). His willingness to participate, despite his original reluctance, seemed to be part of a marital dynamic. Mr. Williams signed all of the consent forms, consented to be interviewed, and sometimes joked and laughed with field-workers, but other times (exhausted from a long day of work) he was grouchy. His grouchiness made me tense. I was afraid that one day he would come home, get mad at his wife, and end the study. (Fortunately, he didn’t.)

And, of course, there is the emotional exhaustion. Quantitative researchers can work in their sweatpants, at home, late at night, while ethnographers have to travel to their sites, wiggle their way into complex social settings, help make others feel comfortable, and try to repair problems that pop up. This uncertainty—this pattern of taking three steps forward and two back—can make the progress seem slow as well as draining.

Some social scientists think that an entire ethnographic study can be done in two to three years: a bit of time to formulate the program, write the forms for permission to study human subjects (and submit them to the institutional review board, or IRB), gain access to the field, and collect data; one year to analyze the data, figure out the argument, and start writing; and one year to finish writing, go through the reviews, revise, and send a book (or, occasionally, a series of articles) to a publisher. Doctoral students want to do the entire process in two years. There have been people who have done that. But these people are rare. Doctoral students are generally faster than faculty because they have more time, sometimes they have more guidance, and they have urgency. I have seen doctoral students do a fine study in less than three years, particularly if during one of those years they don’t have to work to earn money. For many faculty members or researchers who are juggling multiple demands in their lives, it takes several months to a year just to get the project going, and it takes another several months to a year to collect data for a qualitative project; sometimes it takes two or even more. It then takes months (not weeks) to figure out the argument, which is followed by writing the book, which often takes a year or more (if drafted at lightning speed) or two or three (at a more normal pace). The manuscript has to be reviewed, it has to be revised, it has to be reviewed again, and then it has to be finalized. There are many steps in the process. For most of us, it is slow.3

The hard reality is that ethnography is a greedy institution. With interviews, it is common for people to forget about the meeting. It can take many, many efforts to schedule one interview. Participant observation takes even more time. I usually budgeted two to three hours in the field and five
hours for writing fieldnotes. When I paid graduate students to write fieldnotes, I encouraged them to write notes for five to twelve hours for each visit. This time doesn’t count time I spend getting myself ready to go, figuring out what small gift to bring (i.e., a pie or tomatoes from the backyard), and traveling there and back. I also am constantly figuring out how to manage problems that surface in the field. For example, in elementary schools it is always a problem when a kid with whom you are trying to build rapport breaks a school rule by hitting another kid right in front of you. On the one hand, the adults expect you to enforce school rules; educators can withdraw permission for you to be there if they are sufficiently annoyed. On the other hand, you are usually trying to build a relationship with a kid; you don’t want to damage that relationship. It is a balancing act. Usually unexpected dilemmas are routine during a study; each needs careful attention. Thinking about them is tiring.

Furthermore, I have never been one of those people who can get home, fix a cup of coffee, sit down, and start writing fieldnotes. Instead, I wander around the house, take a nap, work in the garden, or stall in other ways. My need to start to write fieldnotes looms large. I am anxious because the notes need to be written up that night and it takes hours to write the notes. Vivid, rich, and detailed fieldnotes are the life blood of an ethnographic study. But it is hard for me to force myself to get going. Finally, I settle down. I started following baseball while I was in the field in part so I would have something comfortable to talk about with my families; I now like to have a baseball game on the television or music playing while I write notes. (Television other than baseball, however, is too distracting for me.) As I begin, I have a habit of writing out (by hand) on a pad of paper the topics that I need to cover of what happened during the visit. Then, as I finish writing my fieldnotes on one topic, I decisively cross the topic off with a large X. It is satisfying. Topic by topic, I march along. In addition to the notes, I also write out the chronology of events, and I highlight the analytic themes raised by the visit. By the time I write an analytic memo (in which I try to link the themes in the notes to the broader general question of the study and the literature on the topic), I am exhausted. Each session of writing fieldnotes is slow. Still, it is deeply satisfying to print out each copy and pile them up, adding new visits to the pile. And, looking back, it seems as if the time in the field didn’t last all that long of a period. The days are long, but the years fly by.

There are also difficult conflicts between work and family in doing ethnography. Your life (for better or worse) does not come to a screeching halt.
just because you have started a new study. But I would be lying if I didn’t say that were times when I would wish that all of my life obligations would suddenly vanish so I could work 24/7 on my study. You have to create a balance. Sometimes it is tricky. I have a very good friend who, when I was in the middle of collecting data, got diagnosed with breast cancer. She had two children under the age of ten; it was a twelve-hour trip to travel to her home on the west coast. She went through chemotherapy and her prognosis was promising, but she was exhausted. Her husband needed to go on a long-scheduled work trip for two weeks. One summer evening I was talking with her by phone. She didn’t sound good. I said, “I could come to visit.” She said something like, “How about tomorrow?” My heart sank. We were in the middle of doing observations with an African American family below the poverty level. It was very hard to get permission to visit a family daily for three weeks, but it was crucial for the study. It was all going well, but it was intense. I was doing fieldwork too. But, abruptly leaving to go to see her just seemed to be the right thing to do. I left a few days later for a weeklong visit. I cooked, watched the kids, and hung out with my friend. While there, I also spent time on the phone each night with the research assistants. A crisis developed in the field. Money was stolen from the field-worker by the aunt of the kid. (The aunt was on dope). I personally would not have mentioned it to the mother, Ms. McAllister, but the field-worker (who as a graduate student was strapped for cash) did tell her. (I repaid the field-worker, of course, when I got home.) Ms. McAllister was humiliated and angry. It was a mess. And I wasn’t there to pop by, bring some beer, and make jokes to help smooth it over. Instead, I coached the field-worker on the phone. It all worked out. But it was challenging. And throughout the project each day seemed long as I juggled not only the ethnographic visits and writing fieldnotes but the other considerable demands of my life. Having a large grant is a blessing, but it doesn’t exactly solve this problem. I spoke on the phone to the field-workers after each visit from ten minutes to an hour. (I did this for many reasons: to learn what was going on, to guide the writing up of their fieldnotes, to help them feel emotionally supported, and to facilitate the data analysis and writing process.) Between writing fieldnotes, emotionally supporting the research assistants, managing the grant, teaching, doing committee work, and managing my personal life (when I was a single woman with elderly parents across the country), I had conflicting priorities. It always seemed like a lot to juggle on a good day, let alone a day when I was sick, my elderly mother wasn’t doing well, or the car broke down.
And then, there is the pressure to publish. At many elite institutions, assistant professors are expected to publish two high-quality (well-placed) articles each year. Quantitative and qualitative scholars are expected, more or less, to have a similar profile. Book chapters do not count. A revised dissertation counts less than a new book (since doctoral students are considered to have had help in the project). Before tenure, people doing qualitative work are told that they need two books or one book, numerous articles, and a second project that is far along. It is crazy. Of course, one book that is high-quality (i.e., wins awards and is well reviewed) can offset a perceived lack in the number of publications. But it is a lot of pressure. The fact that qualitative scholars have spent months and years collecting data (while quantitative researchers were spared that time investment) is not, unfortunately, factored into the equations of evaluations. In some circumstances, having a slow research pace can be a career-ending matter.

There are really no easy answers to this “arms race” of publication, but being strategic can help. Everything that is on the CV when a young scholar accepts a job does not count toward tenure in the same way as new papers count toward tenure that are developed and published after a scholar begins a “tenure clock.” Usually there is not enough time in the six years before tenure to collect a bunch of new data for a second project. Hence, my free advice is that young scholars should collect a lot more data than they need for their dissertation or first project. (Many anthropologists work with one data set for years.) Sometimes researchers have data on a related-but-distinct project that can be developed into a second set of articles or book. Often, young scholars can supplement the existing data set by adding a second site, doing a longitudinal follow-up, focusing on an analytic distinct topic, or doing interviews to flesh out a project. It is good to think about that ahead of time.

And, there is no such thing as a “quick” or “easy” publication. Everything is slow. Partly there are simply too much data. Each interview is around forty pages double-spaced in length. Fieldnotes can run twenty pages double-spaced per visit. There are documents too. Since a project can go in twenty different directions, it is hard to focus. And these computer software programs (e.g., ATLAS.ti) are essentially useless in my opinion when you have done all of the data collection. (They are excellent, however, if you have not done the data collection.) They do organize things, but they are sort of like a fancy file cabinet. They don’t do the thinking for you. Most people I know don’t find them very helpful. I do use them in some projects; other projects I don’t. But the qualitative software packages certainly don’t speed things up.
I find that even if I am diligently writing analytic memos as I am collecting data, there is still another very slow period as I am trying to figure out the argument. Sometimes a trial balloon gets shot down. For *Unequal Childhoods*, the most intensive period of data collection was 1993–1995 when we studied the twelve families; some interviews, including with African American middle-class families weren’t finished until 1997. I remember that around 1997 I thought I had figured out the argument. I was invited to the University of California, San Diego, to give a talk. It was a friendly audience; most people in the room had liked my first book. They liked me. But I was hammered. They told me that I had a “culture of poverty” or “deficit” model where I made working-class kids look deficient compared to middle-class kids. Of course, I felt that it was not what I was saying. But that was what they heard. I was disheartened. I was also busy. During this time I was working at Temple University where the students were variable. Some were as strong as students anywhere in the country. Others, however, had never even heard of a “thesis.” Teaching them how to write an essay was an uphill battle. Many, if not most, sociologists work in more challenging conditions. I was teaching four classes per year; many teach six or eight classes per year. Still, I had my classes, committee work, and other obligations. Finishing the book seemed daunting.

It wasn’t until 1999 when I had a one-semester leave (funded by a presidential grant of the Alfred P. Sloan Foundation) that I began to gain ground. I was a visiting scholar at the Berkeley Center for Working Families (run by Arlie Hochschild and Barrie Thorne). In this vibrant intellectual climate, I began to settle on an argument that was received more positively. I spent a few months writing a paper (eventually published in *American Sociological Review*) that summarized the heart of the argument (Lareau 2002). I remember that in 1999 the group read a piece that eventually became a chapter on the Tallinger family. The responses were positive. I could tell that seminar participants really cared about the members of the Tallinger family. That was the first moment when I began to feel that this really might work.

Still, it is difficult to explain exactly how the core concepts of the book emerged. Mostly I read the fieldnotes over, over, and over again. I thought about the weaknesses in the literature. I thought about how I would answer the question “so what?” in the book. I was very impressed by the work of Melvin Kohn on class differences in child rearing values and the work of Basil Bernstein, Shirley Brice Heath, and Betty Hart and Todd Risley on class and language use. Yet, all of these studies seemed too narrow to me; they didn’t
seem to focus enough on the rhythm of daily life in families. (In other words, I had an emergent critique.) In our visits, the families seemed different to me in their approach to life; I gradually came to call this a different “logic of child rearing.” I wrote a series of papers for conferences; I asked colleagues to read them. Many readers told me I was trying to do too much; they couldn’t follow the argument. I kept “trying to keep my eye on the ball” in terms of the answer to this question, which I often asked myself: what are you trying to explain? Eventually I decided I was looking at variations in parents’ organization of children’s leisure time, parents’ language use, and parents’ intervention in education. (I would sometimes get confused about whether the children or the parents were the focal point. I was studying interaction, but eventually—in cases where there was a decision to be made—I decided to prioritize parents’ actions.) Once I set my priorities in terms of the key research question, then some issues were simply less relevant.

I was in a writing group that was invaluable in giving me feedback. I would write drafts; they would “hold up a mirror” and tell me what they thought I was saying. Their feedback would help me get clearer about what I was trying to say and what I was not trying to say. I would revise to clarify the argument. I would then repeat the process. I do recall that I had trouble finding the right terms for what I eventually called “concerted cultivation” and “the accomplishment of natural growth.” (It was a gardening analogy—I thought about the differences between wild flowers, including flowers that bloom on bridges and sidewalks, and flowers cultivated in greenhouses.) At one point I was using the term “hot house parenting,” but the term seemed pejorative. I wanted terms that were neutral. I wanted to respect both approaches to child rearing. I also thought a lot about disconfirming evidence. Over and over again I would think to myself: “Imagine that you got it wrong. Maybe class doesn’t matter. Maybe it is all about race.” Then I would adopt that conceptual model that I was challenging and reread interviews to see if I could get the “disconfirming evidence conceptual model” to fit the data. I would look for any and all evidence that supported an opposing argument. This exercise had multiple goals. It helped reassure me that these data really did support the claims. It also highlighted moments where there was disconfirming evidence; reporting these pieces of data was crucial. These deviant examples showed more complexity in the data; they helped make the argument more nuanced. In addition, the examples helped the reader trust the argument (since it didn’t make it seem as if I was “cooking” the data). But the manuscript also tried to show that these examples
were unusual. In writing up the results, I had a rule of thumb where I would begin a new section with strong evidence showing three or four examples in support of a claim. I would then present one example of disconfirming evidence; in presenting the disconfirming evidence I would explain to the reader that this piece of evidence, while significant, should not distract the reader from the stronger evidence in support of the claim. I also tried to streamline the manuscript so it did not distract the reader with side issues. It was stressful to “let go” of other, related pieces of data since I was attached to them and since I wanted the story to be as complex as possible. But, as a reader, I strongly prefer to read a book that only has one main argument. I tried to write a book that I would want to read. I also tried hard to remember that an example could not speak for itself. A vivid, detailed, and rich example from fieldnotes was an example of something; it was illustrating an idea. I needed to state the idea before I introduced the example. There were countless quotes that could have been put in the manuscript. Given the effort that had gone into collecting them, it was hard to leave them out. But readers told me that the manuscript was repetitive. The quotes had to go.

I did not have a complete manuscript until 2001. It was reviewed. I revised it. I delivered the manuscript to the press in May 2002. I then had a “shoot-out” with the press over the length. (I had not read my contract.) The book would have been 420 pages. The editors at University of California Press (who were generally wonderful in promoting the book) wanted a book that was less than 350 pages because they felt it would impede the use of the book in the classroom, make the book more expensive, and so forth. In the end, I cut 75 pages (i.e., one chapter and eleven words per page) to bring the first edition of the book down to 340 pages. The final manuscript went to the press in September. I had a copy of the book in my hands in July; the official publication date was September 2003. It was out.

In Unequal Childhoods, I have a long acknowledgment on work-family relations. (The subtext of it is, “I know! I know! I know!” It has been almost ten years since data collection!) I worried incessantly that readers would ask themselves: “WHAT WAS SHE DOING?”) The demands of my family life did impede the progress of my book, but my family members also brought me tremendous gifts. As I note in my acknowledgment to my husband (whom I had met at the end of 1994) and my two children by marriage: “In the clockwork of careers, wherein we are assessed for our productivity, families are not really counted as a legitimate force. But to be speedy is not everything, or even, when all is said and done, much at all. To have the gifts of
companionship, nurturance, and good humor in daily life, however, is quite a lot. I appreciate having [my children, and my husband], in my life (2011, xiv).” I still think that being speedy is not really the point; I think that finishing a high-quality study that you care about is the point. Family obligations often slow a project down, but family support is crucial to retaining one’s sense of humor, recovering from a bad day on the project, and putting the enterprise in perspective. Also, except for a period when young scholars are under a tenure clock, speed rarely matters. When I was in graduate school I worried endlessly about my time to degree, but really it turned out that it didn’t matter. And no one ever asked me, face-to-face, why it took me so long to finish my damn book. (They probably thought about it, however.) Over and over again, I run into people who, blushing, stammering, and looking down at the floor, say, “I am slow.” Even senior scholars, for whom it shouldn’t really matter, look distressed. I feel like saying: “Everyone who is doing a good study is slow. What matters is that there is great study.”

To me, a great ethnographic study really helps us understand a social world. Although reasonable people disagree, I believe that outstanding ethnographic studies usually have two key features. First, excellent ethnographies are saturated with “thick description” where the ethnographers evoke the meaning and experience of research participants. The voices of research participants shine forth. Rather than presenting overly general descriptions of daily life, high-quality research is saturated with detail. To capture this level of detail, researchers usually need to do participant observation for a long time, but others have written fine books based on shorter periods of observation or based only on interviews. So, researchers make an effort to show the limitations of previous research. The unique contribution of the current work is clear. Put differently, there is an effort on the part of the researcher to answer the question, “So what?”

It is hard to write a good book. It requires a kind of “fire in the belly” to keep soldiering on—to keep going back, over and over again, to the pull of the field despite the chorus of people who want you to hurry, finish, and publish. And, it seems to me to be grossly unfair that as you are slogging away writing fieldnotes, none of the people—who surface years later—are around to tell you how much they like your book. Instead, you need to create your own cheering team in the form of a writing group, dissertation group, or cadre of friends to cheer you on and offer constructive, critical advice.

Ethnography can often be lonesome as well as chaotic, slow, and confusing. Usually you are alone in the field. It can be difficult to convey to your
friends and family what it is like to be there in the field site; usually people in the field site have little understanding of your life when you are away from them. But ethnography can also be exciting and exhilarating. And usually there is a happy ending. Although building rapport is a slow process, ethnographers typically are able to build rapport. Although most ethnographers stumble along, interesting books and articles do often emerge from a study. Hence, although ethnographers often run a gauntlet of juggling multiple demands in the field, patching together a work-family balance during data collection and writing, not really knowing where the study is going most of the time, and struggling to finish a book, most do finish. Thus, I think that ethnographers should try to quell the self-criticism that can surface as projects take longer to complete than they had hoped. Ethnographers should not be perfectionists; after all, every study has flaws. But they should stay as long as they think they need to stay in the field in order to understand what they are trying to understand. They should also draw on the pieces in this book to educate others about the pace and nature of the work. Finally, they should try to remember that most scholars complete data collection on only one or two ethnographic projects in their entire careers. Being slow is routine. Good studies can and do emerge from the chaos. And most people don’t carry out many ethnographies in their lives. The days are long, but the years fly by.

Notes

I am grateful to the Russell Sage Foundation as well as the Spencer Foundation for their support of my research. Tim Black, Maia Cucchiara, Kelley Fong, and Judith A. Levine provided helpful comments on an earlier version of this chapter. I also appreciate the suggestions of Rosanna Hertz and Peggy Nelson.

1. The book used ethnographic methods to examine differences in the day-to-day lives of African American and White families. Although the study included separate in-depth interviews with the mothers and fathers of eighty-eight children who were in third or fourth grade, the most unusual part of the book involved intensive case study observations with twelve families. Usually, a research assistant or I visited each family daily for three weeks. The families were paid for their participation. The study also included classroom observations in elementary schools, interviews with educators, and interviews with the twelve children (and their siblings) in the intensive study. Unequal Childhoods describes a cultural logic of child rearing where White and Black middle-class families engage in a pattern of “concerted cultivation,” actively developing children’s talents and skills. By contrast, the pattern for working-class and poor families—
“the accomplishment of natural growth”—sees parents caring for children but presuming they will spontaneously thrive. Since the middle-class strategy is more in sync with the standards of dominant institutions, middle-class children gain advantages even at the expense of the rituals of family life. Follow-up interviews with the young people at around age twenty suggest the continuing influence of social class and the growing power of race, especially for young Black men. An expanded edition of *Unequal Childhoods* was published in 2011 by University of California Press; it includes one hundred new pages describing the lives of the young adults. There is also an extensive methodological reflection on the (often angry) response of the families to their portrayals in the book. For more information, see Lareau 2011.


3. Getting permission from the institutional review board can be slow and daunting. For a discussion of how to manage this process, as well as for other concrete advice on doing ethnographic work, see Annette Lareau, “Doing Ethnographic Research: A Companion Guide” (unpublished book manuscript), Department of Sociology, University of Pennsylvania.

4. For an excellent, recent ethnographic study, see Black 2009. For a study that uses ethnographic methods to study families, see Cooper (forthcoming). For a classic book based only on interviews, see Rubin 1976.

References


