

A Handbook *for Social Science* *Field Research*

Essays & Bibliographic Sources
on Research Design and Methods



Edited by

Ellen Perelman ■ Sara R. Curran



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for Social Science

Field Research

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on Research Design and Methods

Edited by

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Maintaining Perspective Is Essential

Bringing It All Back Home: Personal Reflections on Friends, Findings, and Fieldwork

Andrew Schrank

At the heart of the present volume lies an implied paradox: While the authors of the individual chapters have almost invariably described their own methods as “intuitive,” “instinctual,” and “idiosyncratic” (see, e.g., Chapter 6, Chapter 3, and the Part II overview), they would appear to believe that their approaches can be learned—or else they would not have wasted their time and energy contributing their individual stories to the collection. Is the portrait of the intuitive, instinctual field-worker compatible with the need for a guide to the process of fieldwork, and if so, how? I hope to answer the question by arguing that fieldwork is a craft and that, like craftspeople, field researchers would do well to study the efforts of their peers and predecessors *not* because they expect to find ready guidelines and recipes—the knowledge involved in craft production is inherently tacit and

noncodifiable—but to immerse themselves in the cultures and customs of their communities; that is, *to understand a complicated ethos rather than to find a simple formula.*¹

Seen in this light, a book like this one is perhaps different from most “methods” texts, for it is designed to offer an introduction to the craft of fieldwork rather than a guide to the—to my mind nonexistent—science of fieldwork. If it serves to demystify the craft of field-workers, to introduce a culture and community, and to translate and naturalize a language, then it will have achieved its goals.

I learned the craft of fieldwork the hard way—by making mistakes. I entered the field as a member of the first cohort of fellows of the Social Science Research Council’s (SSRC’s) International Predissertation Fellowship Program in 1992. I hoped to study what at the time seemed a fascinating empirical puzzle: the presence of a large community of Haitian migrants in the Dominican Republic, itself the source of an enormous outflow of migrants to the United States. I wanted to know how and why a small, low-income country like the Dominican Republic could simultaneously attract and disgorge migrant laborers. Why would Haitians migrate to a country so poor as to expel tens of thousands of its own citizens every year? And why would Dominicans employ immigrant Haitians rather than their own compatriots? I hoped to answer the question by drawing on theories of race, ethnicity, and labor market segmentation in plantation economies of the developing world.

Little did I know, however, that the question of Haitian immigration had already been addressed—and addressed in a very sophisticated manner—by scholars in the Dominican Republic. On arriving in Santo Domingo in early 1992, I scoured the local bookstores and libraries and found a number of compelling monographs and countless scholarly and popular articles on the subject—many of which I would later discover in my own university library (see, e.g., Báez Evertsz 1986; Moya Pons 1986; Lozano 1992). I simply had not known where or under what author and subject headings to look for them before entering the field, meeting local scholars, and talking to local librarians and booksellers.

Confronted by the potentially anachronistic nature of my research proposal, however, I faced a choice. I could either continue down the path I had set for myself and carry out yet another study of Haitian migrants in the Dominican Republic, or I could look for a new research question and reformulate my plans. Of course, the easy thing would have been to contact my advisers back in Wisconsin and ask their advice. But I was so embarrassed at having written—and, indeed, sold them on—what I now considered to be a “stupid” proposal that I was loathe to acknowledge what was going on. If

this book had been available to me, I would have read Michael Watts's discussion of research proposals (see Chapter 10) and realized that "even the best-laid research plans cannot—and should never—be cast in stone" and that, in any event, I should have been in touch with my advisers all along. They would have welcomed periodic updates, not viewed them as a burden.

But I did not know this. I knew only that I could not in good conscience spend a year of my life and thousands of the SSRC's dollars essentially replicating somebody else's research and publishing it under my name. So, all but debilitated by "ethical angst" (see Chapter 11), I chose Plan B and spent the next few months looking for a new topic.

Fortunately for me, I found one rather quickly. In the early 1990s, the Dominican Republic was undergoing a profound and largely unanticipated socioeconomic transformation. On the one hand, the sugar plantations, which had traditionally constituted the backbone of the Dominican economy—as well as the chief locus of Haitian employment—were in crisis. North American soft drink producers were beginning to use artificial sweeteners. North American sugarcane and sugar beet farmers were beginning to demand protection. And the U.S. government had therefore decided to cut the Dominican sugar quota in half. On the other hand, the country's labor-intensive, export-oriented manufacturing sector was booming. Asian garment manufacturers had reached the limits of their export quotas to the United States. North American apparel firms and retailers were demanding new, more proximate sources of supply. And Washington was therefore encouraging the spread of export processing zones (EPZs) in—and foreign direct investment to—the Caribbean Basin.

Thus, I set out to investigate the social origins and consequences of export diversification in the Dominican Republic—that is, the underpinnings and impact of the country's rapid transformation from an exporter of bulk agricultural commodities in the so-called old international division of labor to an exporter of low-cost, labor intensive manufactured goods in the new international division of labor (see Fröbel, Heinrichs, & Kreye 1980). I hypothesized that the EPZ—a geographically circumscribed "free market island" (Schrank 2001)—would constitute the modern-day institutional equivalent of the plantation, and at first, the parallels seemed uncanny. EPZs not only required ready access to roads, ports, and large volumes of unskilled, easily repressed labor, like the plantations that had come beforehand, but were in many cases built on top of the old plantations themselves—for the Dominican government had deliberately transformed a number of the country's more vulnerable sugar estates into EPZs (see Safa 1999). Nor was I the only observer to note the uncanny parallels. Tom Brass and Henry Bernstein had recently

outlined the similarities in the *Journal of Peasant Studies* (1992), and Michael Lind would eventually argue that “the EPZ is nothing new; it used to be called the plantation” (Lind 1995).

Thus, I returned to the United States and prepared to write a dissertation that would unpack the variegated ways in which the “banana republic,” in Lind’s words, was being replaced by the “sweatshop republic” as “national middle class capitalism” gave way to “global plantation capitalism” (Lind 1995). I took a host of methods classes, not only in sociology, my home discipline, but in statistics, geography, and urban planning as well. I read everything I could find on EPZs and labor-intensive, export-oriented manufacturing more generally. And I returned to the Dominican Republic to pursue exploratory fieldwork in the summer of 1996, armed with my newfound knowledge of theory and methods.

In a sense, my trip was a fruitful one. I carried out open-ended interviews with a number of public- and private-sector officials who had ties to the EPZs. I gained access to a treasure trove of primary documents on U.S. efforts to promote the export manufacturing sector in the 1980s and early 1990s. And I gathered valuable survey data from a number of government agencies. But the evidence contravened my hypothesis in at least three ways. First, while the EPZs were frequently located on or near old sugar plantations, they utilized a distinct labor force. The plantation labor force had been almost entirely male and largely immigrant. The EPZ labor force was disproportionately female and almost entirely native born. Second, while the EPZs were located near the old plantation ports, they almost invariably shipped their materials from the country’s largest containerized port, in Santo Domingo. And finally, and perhaps most important, while a plurality of the country’s EPZs were located in the traditional eastern sugar zone, the most successful EPZs—in terms of attracting and retaining investment, developing local linkages, and “upgrading” their production—were located in the traditional tobacco, coffee, and cocoa territory to the north. Thus, my empirical findings flew in the face of my theoretical expectations. The EPZs were not simply the modern incarnations of the old sugar plantations.

Furthermore, the north’s achievements were particularly vexing, for tobacco, coffee, and cocoa had not been grown on plantations at all but had customarily been grown by commercialized family farmers and exported by large trading houses in the relatively prosperous interior city of Santiago. Why, given its remote location and relatively high factor costs, had Santiago come to dominate the low-cost export-manufacturing sector? On my return to the United States, I tried to ignore the question and to treat Santiago’s success as nothing more than an anomaly—the sort of thing that goes in the error term of a multivariate equation—rather than to “incorporate the

surprise” into a revised version of my hypothesis, as suggested by Michael Piore (Chapter 7).

As time went on and a return to the field—this time to complete my dissertation fieldwork—drew closer, however, my hypothesis grew less and less tenable, and alternatives came to seem more and more attractive. Why? On the one hand, I had by now taken enough mainstream methodology courses to be convinced of the merits of at least a moderate version of “falsificationism,” and if ever a hypothesis had seemed falsified, it was my “sweatshop republic” idea. On the other hand, I had begun to notice a recurrent pattern in the comparative historical literature on secondary cities, that is, that secondary cities are often more dynamic than their primary counterparts (see, e.g., the various references in Hirschman 1968).

Therefore, I began to acknowledge Santiago for what it was: not an exception to be ignored or explained away but the basis for a new theory of export diversification. Perhaps, I reasoned, the residents of Santiago—a geographically and culturally remote secondary city—had little choice but to abandon agriculture for industry. While the public sector offered residents of the capital of Santo Domingo at least a modicum of prosperity and upward mobility, low factor costs guaranteed the flow of foreign investors to the southeastern sugar zone. Santiago had neither public-sector jobs to dole out nor the lure of particularly low-cost labor, however, and as its agrarian economy began to decline in the late 20th century, its relatively cohesive elite was all but forced to pursue export manufacturing with a vengeance. Hence, the success and domestication of the northern EPZs.

I therefore returned to the field once more in early 1998, hoping to explore the variety of agro-industrial transformations under way in the Dominican Republic: the EPZ as a modern-day plantation in the southeastern sugar zone and the EPZ as a potential stepping-stone toward sustainable industrial development in the northern coffee, tobacco, and cocoa zone. So far so good. But on my arrival, I was forced to confront a new—and literal—crisis. The EPZ sector was in turmoil, and firm after firm was closing up shop. The culprit, according to almost every factory owner I interviewed, was the North American Free Trade Agreement (NAFTA), which offered Mexico both tariff and factor cost advantages that the Caribbean Basin could not duplicate. My respondents carried on incessantly about the unfairness of it all, the inevitable disappearance of the Dominican Republic’s new leading sector, and the likely arrival of “una pobreza espantosa” (a terrifying poverty) in short order.

Except for one. One elderly and experienced factory owner, when asked about the crisis, denied its very existence. “Have you looked at the aggregate export data?” he asked. “No,” I replied, “but I’ve seen the bankruptcy

figures.” “Look at the aggregate export data,” he advised. And so I did. And much to my surprise, the Dominican Republic’s textile, clothing, and footwear exports—the chief products of the EPZs—had continued to grow in the wake of NAFTA’s implementation in the mid-1990s. The rate of growth had diminished, of course, but the volume of exports had grown, and by 2000, the number of workers employed in the country’s EPZs had grown by several thousand over the pre-NAFTA level.

What, then, was going on? A shakeout, my source suggested, induced not so much by NAFTA as by broader changes in the industry. I need not rehearse those changes at great length here. I have done so elsewhere, and I feel confident in my—and my source’s—assessment (Schrank 2005). Suffice it to say, however, that I had stumbled onto a much more interesting story than the one I had gone looking for in 1992, 1996, or even 1998. And it gets more interesting as time goes on (see Chapter 2, on case studies).

What does this have to do with the craft of fieldwork? Well, I would like to think that in the course of getting my story straight and learning how to tell it—that is, how not only to understand but to explain the Dominican Republic’s unanticipated and yet undeniable transformation—I also learned how to do fieldwork. But I did not do so as an individual or an isolate. The differences between my first visit to the field and my last included, not only the fact that I had taken courses back home and that I now had experiences from my previous trips to draw on, but also, and not least importantly, that I had had exposure to dozens of other scholars who had themselves undertaken fieldwork. When I first went to the Dominican Republic, I was a year out of college. Not only had I never been to the field, but almost none of my peers had either. I was not even an apprentice yet. I was a novice. By the time I reentered the field to undertake my dissertation research, however, almost all my peers had undertaken fieldwork, and we had spent countless hours discussing our experiences: our successes, our failures, and what we would do differently if we had it to do over again. I was, in other words, part of a community of craftspeople, and our craft was fieldwork.

This book is the product of such a community. It contains many of their stories, and I cannot help but think that if I had had it, or something like it, in 1992, I would have learned faster and perhaps more efficiently what I ultimately learned over a period of six years. I know that at the very least I would have learned what I ultimately learned with less “ethical angst” and fewer sleepless nights. For it most assuredly would have helped me deal with what I thought at the time was a unique situation but now realize is all but universal in our craft: the overwhelming sense of distress one feels when one reaches the field and realizes that one’s project is either undoable, already done, or not worth doing: “I’ve invested all this time and energy, pulled up stakes,

moved to a foreign country where I'm a complete stranger to everybody, and for one reason or another, my time here is going to be worthless."

What is the appropriate reaction to such a quandary? I think the chapters in this volume point to a few important lessons: First, don't panic. When I first went to the field and found that my project was anachronistic, I thought I was the only one ever to encounter the situation. When I encountered similar situations later on, however, I had a number of peers to turn to. One friend, in particular, had studied at Berkeley with Michael Watts. He told me that Watts had warned him that when he reached the field, he would inevitably find that his proposal was either undoable or undoable as planned; that he would need to make the necessary adjustments; and that the ability to do so was the mark of the true scholar—all points which, perhaps ironically, he reiterates in his contribution to this volume (see Chapter 10). Just knowing that there was a tenured professor out there at Berkeley who had not only gone through a similar situation but described it as normative or typical calmed me down, and I now realize—as Watts and a number of other contributors to this volume have underscored—that such unanticipated shocks are part of the normal course of events in our profession. It is how we deal with them that matters.²

And how do we deal with them? A second friend quoted his adviser, an American historian, as saying that a topic was not overstudied until it had generated as many books as Abraham Lincoln's life. The Library of Congress contains several hundred biographies of Lincoln, and I am not sure every problem requires so many. But the basic point remains: We tend to think that if a problem or phenomenon has been studied at all, it is no longer worth studying. Nothing could be further from the truth. Scholarly progress occurs when we study and reread an old problem in order to garner new insights. And the best studies are often the latter ones—precisely because they engage and build on their predecessors.

Nevertheless, sometimes we really do encounter a problem that is not worth the effort. In my case, it was not simply that the issue of Haitian migration had been studied to death or that there was nothing new to say on the matter, although the former was close to the truth and the latter a distinct possibility. But many Dominican scholars had already staked their claims in the area, and out of ethical concerns like those discussed by Sara Curran in Chapter 11, I did not wish to encroach on their territory. I did not abandon the project, however, until I had found a more interesting—to me—area to move into. The lesson here, I believe, is not to abandon ship until you have found a reliable lifeboat.

Where are you likely to find such a lifeboat? In Chapter 10 of this volume, Michael Watts suggests a focus on empirical puzzles. Arthur Stinchcombe makes a similar point in his classic work on *Constructing Social Theories*

(1968). Historically, the great social scientists have been problem solvers, he argues. Marx, Weber, and Durkheim “did not work mainly at what we now call ‘theory.’ Instead, they worked out explanations of the growth of capitalism, or of class conflict, or of primitive religions” (Stinchcombe 1968, p. 3)—and thereafter used their findings to build more general theories. When they are good, according to Stinchcombe, scholars like de Tocqueville, Trotsky, and Smelser are doing the same thing: using history and empirical facts to build theory, not vice versa (Stinchcombe 1978, pp. 2–3).³

And finally, how do we know where the puzzles are—let alone how to solve them? Here, I would second the advice of Harrell, Gottlieb, Giles-Vernick, Piore, and Vitalis in this volume and say, at least in part, “Let your sources tell you.” The sources may be living or dead, found in archives or on street corners, but they almost certainly know as much or more about their surroundings than you do, and by listening to them and trusting them, by treating them as your friends and guides, you will almost certainly make yourself a better field-worker.

That said, however, there is still no substitute for the actual practice of fieldwork. After all, Michael Piore (Chapter 7) and Charles Sabel note that skilled craftspeople require a wide array of expertise and ability; that the relevant knowledge is often tacit rather than codifiable; and that they are therefore unlikely to be adequately prepared by “book learning alone” (Piore & Sabel 1984, pp. 273–274). The same holds true of field-workers. While books like this can go a long way toward demystifying the process and opening the door for the novice, actual mastery demands entry into—and immersion in—a community of experienced scholars. As a number of the preceding chapters have argued, however, one need not wait to reach the field before beginning the initiation process. Most universities have archival resources worth perusing (Chapter 1). Many departments offer courses that foster practical experience in ethnography (Chapter 3). And almost all have statistical software and access to online survey data (Chapter 6). Thus, there is no obvious barrier to initiation. It simply takes the willingness to explore new approaches and to learn by doing. Better to begin sooner rather than later.

Notes

1. I would like to express my sincere gratitude to Michael Piore, who has not only taught me most everything I know about craft production and a good deal of what I know about fieldwork but has also served as a model of an intellectually acute, publicly engaged, and downright generous comparative social scientist—as well as a good friend. See Piore and Sabel (1984, especially pp. 273–275) on the training of craftspeople and craft workers in general.

2. Watts echoes these sentiments yet again in his discussion of the “necessary and inevitable risks and uncertainties of *doing* research” in Chapter 10 of this volume (p. 195).

3. Michael Piore (Chapter 7) describes a microlevel version of the same process: using individuals and their stories to build theories. As C. Wright Mills would have noted, the individuals and their stories scale up to constitute what we call “history.”