PROLOGUE

Bringing Theory to the Field

This book arose from the badgering of Loïc Wacquant, who insisted that it was time to collect these essays, new and old, and throw down the gauntlet to the Chicago School. While I'm grateful for all his encouragement, forcing me to rethink once again what I have been doing for forty years, I could not follow his proposal to inaugurate a Berkeley school of ethnography. I doubt there could ever be a such a school, since Berkeley's distinction lies in the diversity of its approaches to everything, and to ethnography in particular. Our ethnographies run the gamut from Marxism to feminism and postcolonialism, from positivism to reflexive sociology, from symbolic interaction to comparative history. As ethnographers all we have in common is a commitment to studying others in their space and time. From the beginning the ethos of Berkeley sociology has always been antischool.

To deny the existence of a Berkeley school is not to say that my vision of ethnography appeared as an immaculate conception or was cultivated in heroic ethnographic isolation. To the contrary, the essays that follow have been forged in Berkeley since the mid-1980s: in debates with my colleagues, in courses on participant observation and methodology, in dissertation seminars that have generated a stream of ethnographically based books. Before Berkeley I learned my trade from Jaap van Velsen in the Zambian tributary of the Manchester School of social anthropology, and beyond Berkeley I absorbed much from my collaborations with János Lukács in Hungary, and Pavel Krotov and Tatyana Lytkina in Russia. Inevitably, my most supportive critic has been Erik Wright, an outsider to the cult of ethnography, always quick to point out nonsense in my writing—although sometimes nonsense has virtues that he won't acknowledge.

There is a second reason why my writings cannot be tied to a Berkeley school. Not only is there a rich diversity of traditions within Berkeley but the approach adopted here—the extended case method—is found in other departments around the world, and in other disciplines, most notably anthropology and geography. Within sociology, insisting on an ethnography that forges micro-macro connections through the reconstruction of social theory is not as heretical as it once was. Yet it does continue to face resistance from a naive empiricism that regards ethnography as special because it gets at the world as it "really is," that assumes social theory grows tabula rasa out of that reality, and therefore only by ridding ourselves of biases and prejudices can we coax the field into disclosing its truth. This naive empiricism is often combined with an equally naive positivism: to grasp reality we can and must stand outside the world we study. This presumes a social world divided into two spheres: one sphere occupied by the producers of objective knowledge, separated from a second sphere inhabited by the subjects of knowledge.

In this view ethnographers must not disturb the worlds they study, but instead they must aspire to be the proverbial fly on the wall.

The approach of this book is very different. It is based on the following six postulates.

- We cannot see social reality without theory, just as we cannot see the physical world without our eyes. Everyone carries and uses social theory, cognitive maps of the world we inhabit, although not everyone is a social theorist, that is, someone who specializes in the production of such maps. Thus social theory ranges all the way from practical to tacit knowledge (knowledge we take for granted in conducting our lives) to abstract formalisms that look more like mathematical theorems than maps of the world.
- No impenetrable wall separates the worlds we study from our laboratories of science. To the contrary, we are inherently part of the world we study. What differentiates social scientists from the people they study is the theory they carry that allows them to see the world differently and, I would say, more deeply. I call the theory that we self-consciously develop analytical theory or social science, whereas the people we study possess an unreflective, usually tacit, theory that I call folk theory or common sense. Social scientists are not suspended in an ether of analytical theory; they too have their own folk theory. When it comes to their own lives, even their lives as sociologists, they all too easily suspend the insights they apply to others. Sad to say, we can be as unreflective and myopic about our everyday worlds as anyone else.
- Analytical theory or science reveals the broader context of our actions, but it also shows how the context creates the illusion

of its own absence, of an everyday world that is autonomous and self-contained. We may blame ourselves for unemployment, whereas its sources are markets and governments—external forces that not only produce unemployment but also mystify that production. In revealing the connections between micro and macro we are developing what C. Wright Mills called the sociological imagination. That is our vocation.

- The university is not a neutral terrain but a field of competing theoretical perspectives and methodological approaches, research programs, if you will, that offer different insights into the way micro and macro are connected. These divergent approaches form nodal points in a hierarchical field of power, refracting the impact of forces beyond its boundaries.
- Analytical theory enables us to see and thus comprehend the world, but that does not imply automatic confirmation. To the contrary, the world has an obduracy of its own, continually challenging the causal claims and predictions we make as social scientists on the basis of our theories. That is how we develop science, not by being right but by being wrong and obsessing about it.
- Analytical theory is not necessarily incomprehensible to lay people. Social science and common sense are not insulated and incommensurable. In other words, it is possible, but not always easy, to forge a passage from common sense to social science, and it is possible that one can elaborate a good sense within the common sense. Indeed, that is the task of the public ethnographer.

These postulates have their roots in four decades of participant observation, in the factories and mines of four countries

(Zambia, the United States, Hungary, and Russia), resulting in studies of the microprocesses of four great transformations (decolonization, the transition to organized capitalism, the Soviet transition to socialism, and the transition from socialism to capitalism). You may well ask how a single ethnographer, working in a single factory, can illuminate a great transformation. Although definitive of the sociological imagination, the task may seem absurd to many a conventional ethnographer.

The answer lies with the extended case method, defined by its four extensions: the extension of observer into the lives of participants under study, the extension of observations over time and space; the extension from microprocesses to macroforces; and, finally and most important, the extension of theory. Each extension involves a dialogue: between participant and observer, between successive events in the field, between micro and macro, and between successive reconstructions of theory. These dialogues orbit around each other, each in the gravitational field of the others. To make sense of these dialogues the different studies described in this book make different simplifying assumptions.

In the first chapter I describe the genesis of the extended case method. In effect I apply the extended case method to my own participation in the academy and in the field—participations that are in dialogue with each other. In the second chapter I develop a more formal framework for the extended case method by reference to my study of race and class in postcolonial Zambia (1968–72). I end by developing two models of science: positive and reflexive, each autonomous but necessary for the other. The third chapter develops the idea of a reflexive science through the idea of the revisit. If my Zambia study was based on an extension back into history, an archeological dig, here I dwell on revisits to earlier ethnographies of the same place. The chapter sets out from a comparison of my own ethnography of a Chicago factory with one of the same factory conducted thirty years earlier. From there I examine other types of focused revisits but end by elevating the "revisit" as a trope for all ethnography.

The fourth chapter extends the ethnographic approach to comparative history, and it also extends the number of cases from two to three. It contrasts the analyses of revolutions found in the writings of Trotsky and Skocpol. It underlines the difference between the reflexive science of a participant observer and the positivist science of the comparative sociologist. In participating in the revolution that he studies, and in reconstructing the Marxist theory of socialist transition, Trotsky offers one prototype of the extended case method. The fifth chapter extends beyond three cases. It turns to the transition "back" from socialism to capitalism. It analyzes a series of successive factory, and then community, ethnographies that I carried out in Hungary and Russia between 1982 and 2002. It shows how each study built on preceding ones, wrestling first with a comparison of Hungary's state socialism and the organized capitalism of the United States and then with the Soviet transition from state socialism to market capitalism.

If the opening chapter is a self-analysis of my own trajectory, the concluding chapter focuses on my intellectual engagement with the four great transformations of the twentieth century. Here I try to assess the strengths and weaknesses of the extended case method, steering a course between romanticization of my subjects and reification of the external world. I ask what light my ethnographies have shed on these great transformations,

what the latter have in common, how they are connected to each other, and what the implications are for the twenty-first century.

I have been accused of creating disasters wherever I go. After I left Zambia, the price of copper plummeted and Zambian society with it. After I left Allied Corporation, it went bankrupt along with the rest of south Chicago's industry. The area became an industrial wasteland. After I left Hungary, the Lenin Steel Works, and Hungarian industry more generally, disintegrated in the face of market forces, quickly catching up with south Chicago. I was in Russia for only seven months before the edifice of the Soviet Union crashed down on the heads of its workers. I plead innocent. I was not to blame. Correlation is not causality. All these sites became victim of what I call third-wave marketization, which began in the middle 1970s, a tsunami that continues to devastate our planet. Ethnography offers an especially potent insight into the catastrophic collapse of so many communities, while extending the extended case method to global ethnography helps us discern common patterns around the world and the forces that create them.

I may not have been the cause of disaster capitalism, but that is not to say my ethnography was not without its effects. Indeed, one might think that ethnography's direct engagement with participants lends itself to public engagement. But this is far from necessarily being the case. While Trotsky's analysis definitely fits the category of public ethnography and so did my study of Zambianization, this was not true of the Chicago factory study or the studies in Hungary and Russia, which were more clearly aimed at an academic audience. Even these intently professional studies, however, by linking microprocesses to macroforces, provide the foundation for a public sociology that turns private problems into public issues. Ethnography may not necessarily be public sociology, but by engaging with suffering and domination, hierarchy and inequality, ethnography calls attention to our accountability to a world beyond and thereby inevitably raises the specter of public sociology. This is the topic of my epilogue.

Inevitably, the ethnographer's debts are enormous since our work is inherently collaborative. To recognize the anonymous actors of our field in a ritualistic sentence or two is an inadequate acknowledgment of our responsibilities to publics, both the ones upon whom we depend in the process of research and the ones to which we are more distantly connected. As for my academic colleagues, I have acknowledged their contributions to the individual essays at the end of each chapter. I am grateful to Harvey Molotch, Mitch Duneier, and Diane Wolf for their support for this project as a whole and to Art Stinchcombe and Diane Vaughan for their comments on the chapters that are new to this book. Most important, Naomi Schneider has been a font of support for ethnography, mine and others, since she arrived at the University of California Press twenty-five years ago. She has been a potent force behind the continuing ascendancy of the extended case method.

Introduction

From Manchester to Berkeley by Way of Chicago

On a hot and muggy September day in 1972, I was dragging my suitcases across the South Side of the Windy City in search of the University of Chicago. I'd just finished my master's in social anthropology at the University of Zambia and decided to take my chances in the United States. I had somehow sneaked in under the Chicago admissions wire, ready to pour my life savings into the first year of graduate school. Chicago had offered me no fellowship, no job. In fact, the sociology department clearly didn't want me. I was seeking out the Committee for the Comparative Study of New Nations, which had pioneered the much-calumniated development theory circulating in Africa, ideas associated with such figures at Clifford Geertz, Aristide Zolberg, Edward Shils, Lloyd Fallers, Lloyd Rudolph, and Susanne Rudolph. The Committee on New Nations had disbanded before I arrived.

After Zambia, Chicago sociology looked decidedly provincial. I had arrived in the Zambian capital, Lusaka, in 1968, four years into independence. At that time Zambia had all the vitality and optimism of a new nation. By 1970, when I enrolled for a master's degree, the University of Zambia was already populated with its first cohorts of undergraduates, an incipient elite from different backgrounds, instinctively oppositional and idealistic. They would annually take to the streets in protest against various governments, including their own, for betraying social justice, especially in dealings with apartheid South Africa. Among the faculty many in the social sciences were old hands from Africa and other developing countries, deeply engaged with the challenges facing Zambia, often working together in stimulating interdisciplinary seminars. Indeed, Africa as a whole was awash with exciting debates about socialism and transformation. These were inspiring times for social science.

TORMENTED IN ZAMBIA, REBELLING IN CHICAGO

In Zambia I had three extraordinary teachers who introduced me to the world of sociology. The first, with whom I developed the closest and longest relationship, was Jaap van Velsen—a vigorous and domineering Dutch anthropologist nurtured in the Manchester School under Max Gluckman. Jaap was a lawyer by training before he became a no-nonsense materialist social anthropologist. His *Politics of Kinship* (1964) was a study of the manipulation of kinship norms among the Lakeside Tonga of Malawi. Anticipating Pierre Bourdieu's now-celebrated theory of practice, Jaap would apply his "poststructuralism" to any institution, from the family to the law court to the United Nations (see Van Velsen 1960, 1964, 1967). He was especially interested in

systems of labor migration in southern Africa. His methods and ideas, often delivered in passionate and booming off-the-cuff lectures, are deeply etched in my sociological habitus.

My second teacher was Jack Simons, an activist-intellectual within the South African Communist Party. He had been expelled from South Africa but was still very engaged with the African National Congress in exile. He would later, already in his sixties, leave for the military camps to teach Marxism to freedom fighters. With his wife, Ray Simons, the legendary South African union leader, he had just completed the now-classic history of South Africa, *Class and Colour in South Africa* (1969). Revered by the students he left behind in Cape Town, he was a fearsome presence in any context. Finally, there was Raja Jayaraman, just arrived from India, having recently completed his dissertation on caste and class on Sri Lankan tea plantations, a dissertation completed under M.N. Srinivas, the guiding father of Indian social anthropology. Raja was also of Marxist inspiration. He was definitely the gentlest of the three, but he too could develop a combative streak in the presence of his senior colleagues. They were an intimidating troika. Each week they struck gloom and terror into my soul as they openly competed to shred my essays to pieces. After this battering I was ready for any punitive pedagogy Chicago would hand out.

If Chicago faculty also prided themselves on bullying students, they could not match the intellectual virtues of my Zambian teachers. I was not prepared for the boring conventionality of Chicago sociology and the quiescent conservatism of its politics, with such notable exceptions as Richard Taub. To be sure, Chicago had had its excitement, its student revolt centered in sociology. But this had been snuffed out by 1972 when I arrived, leaving the sociology department a bastion of professionalism. With interest in other countries in remission, I turned my attention from the sociology of development to the much-vaunted Chicago School of urban ethnography. But here too I was disarmed by insularity. Its practitioners were still treating their field sites as Malinowski had treated the Trobriand Islanders, cut off from the world and from history. It seemed as if the very point of ethnography was an obsessive presentism, an abstraction from history, a repression of the past.

It was a confinement in time but also in space. I was dismayed to discover how ethnographers imprisoned neighborhoods in their physical environment—tracks, building, schools, parks, and so on. How different, indeed, from their own founders, from, for example, Thomas and Znaniecki, whose *The Polish Peasant in Europe and America* (1918–20) was an early classic of the Chicago School that traversed continents and centuries in its interpretation of letters exchanged between communities in Chicago and Poland. Even Louis Wirth's *The Ghetto* (1928) had taken history seriously. What had become of that original global and historical imagination? Indeed, what had become of ethnography, reduced to a minor moment in Chicago sociology, now inundated with network analysis and rational choice theory?

So I became a missionary for the "extended case method"—the Manchester School of ethnography, which was developed in the towns and villages of central and southern Africa and situated field sites in the wider society and its history. Social anthropologists trained in Manchester were dispatched to the colonies to do their fieldwork. I was taking the method in the other direction, from Africa to Chicago.[1] My friends laughed at me

when I passionately explained how, in his original essay, Max Gluckman had sketched the social structure of South Africa by describing the opening of a bridge in Zululand (Gluckman [1940 and 1942] 1958). Equal skepticism greeted my own "extended case study"—a three-and-a-half-year study (1968–72) of the processes of racial succession in the Zambian copper industry in which I traced those processes back into colonial history and out into the postcolonial class structure (see chapter 1). I was beyond comprehension and certainly beyond the pale. There was, however, one exception. Bill Wilson had just joined the faculty and generously devoted time to this wayward, iconoclastic student. Indeed, he became quite interested in my argument about the class basis of racial orders, an argument I was then applying to South Africa.

The Marxism that had become second nature to me in Zambia was refined by the brilliant teaching of Adam Przeworski, also just arrived in Chicago but in the Political Science Department. For my dissertation I settled on the question of work organization and class consciousness, deciding to explore this through participant observation in a local factory—a Marxist resurrection of the old Chicago School studies of industrial work, long since forgotten by sociology. Little did I anticipate that this would be more than a resurrection but a serendipitous revisit to the same plant that the great Chicago ethnographer Donald Roy had studied thirty years earlier (1952a, 1952b, 1953, 1954). What was originally intended to be a devastating critique of plant sociology—bounded by the factory walls and confined to the present—turned into a historical analysis that used Roy's study as a baseline. My historical analysis sought to reconstruct Marxism by showing how the factory too was a site of politics where consent to capitalism was organized. The comparison with Roy's study allowed me to argue that this "hegemonic regime" of production politics was a feature of advanced capitalism, very different from the more despotic production politics of early competitive capitalism (see chapter 2).

PARIAH IN BERKELEY, ESCAPE TO MADISON

I survived Chicago under the protective umbrellas held out by Bill Wilson and Adam Przeworski and the comradeship of other graduate students. Through a series of rather fortuitous events and unintended consequences I landed the dream job at Berkeley (Burawoy 2005). I arrived there in 1976, fresh out of graduate school. As far as Berkeley's graduate students—many of whom had actively promoted my candidacy and were largely responsible for my getting the job—were concerned, my appeal lay with my Marxist credentials. Among the major sociology departments of the time, Berkeley's had been known for its radicalism, yet none of the faculty was teaching the newfangled Marxism. Indeed, when I arrived, students were organizing their own courses and running seminars on such topics as Marxism, feminism, and the political economy of South Africa. To arrive in this fissiparous department and to face lofty student expectations proved rather daunting.

Among other things, students could not comprehend my obsession with ethnography. Surely, they remonstrated, a Marxist cannot also be an ethnographer? Marxism deals with large-scale historical transformation, while ethnography confines itself to microprocesses, and never the two shall meet. Of course, that was a statement about Berkeley ethnography at the

time, itself deeply influenced by the Chicago tradition imported in the 1950s with Herbert Blumer, Tamotsu Shibutani, and Erving Goffman. While Dorothy Smith and Arlene Kaplan Daniels had subsequently given it a feminist twist, and my new colleagues David Matza, Troy Duster, and Arlie Hochschild undoubtedly gave it a critical edge, its lineage was unmistakably Chicagoan.

From some quarters it was skepticism, but from other quarters it was outright hostility that greeted me. When it came to tenure colleagues appointed to evaluate my fitness to join their inner circle had problems that ran the gamut from bad teaching and ideological bias to weak scholarship. It appeared to me and, fortunately, many others, to be a poorly formulated and thinly veiled attack on Marxism, which had proved too popular with graduate students. Sure enough, the substantive focus of their critique lay with the flaws in my methodology. The claims I made in *Manufacturing Consent* (1979) about the nature of advanced capitalism, they averred, were speculative and unscientific, driven by a theoretical tradition that belonged to the previous century.

When it was all too clear that I would never survive at Berkeley, I gratefully accepted a position at Madison, Wisconsin, where faculty, especially the demographers, were far more open to novel ways of studying the empirical world. They didn't care about my Marxism so long as I was empirical, and that I surely was. At that time, with the exception of a language analyst, I was the only ethnographer in the department.

But here's the twist. If Berkeley graduate students thought that a Marxist ethnographer was an (oxy)moron, at Madison they took the opposite view. Students had never seen an ethnographer before; they knew me only as a Marxist. Since I did something called ethnography, that must be *the* Marxist method. My arrival was greeted with relief, especially by those "class analysis" students who were resistant to Erik Wright's analytical and quantitative approaches. For them the joining of Marxism and ethnography appeared to be a perfect and seamless marriage. This volume aims to demonstrate that Marxism and ethnography can indeed be partners, but they are by no means necessarily or unproblematically so. Too often Marxism is trapped in the clouds, just as ethnography can be glued to the ground.

That graduate students at these two departments had such opposing views of ethnography thirty years ago only underlines how participant observation had become ghettoized within the discipline. It had not always been that way. The separation can be traced to the postwar battle for the soul of sociology: Harvard's grandiose structural functionalism challenged the supremacy of the Chicago School, which reacted with antitheoretical microempiricism, brilliantly mislabeled as grounded theory. Yes, one might say that theory had been grounded, in the sense of stalled, stranded, cramped, and limited, having ditched the major theoretical traditions of our discipline. Today, we may say, with the exception of a few diminishing holdouts, ethnography has been reintegrated into diverse bodies of social theory to the benefit of both ethnography and theory.

Reflexive ethnography merely cements and spells out this assimilation by transcending conventional oppositions: participant and observer, micro and macro, history and sociology, theoretical tradition and empirical research. We transcend these oppositions not by dissolving their difference but by bringing them into dialogue. First, we do not strive to separate observer from participant, subject from object, but recognize their

antagonistic coexistence. No matter how we approach our research, we are always simultaneously participant and observer, because inescapably we live in the world we study. The technique of participant observation simply makes us acutely aware of this existential and ethical conundrum. But without theory to ground us we would lose our way.

Second, there can be no microprocesses without macroforces, nor macroforces without microprocesses. The question is how we deal with their relationship. It requires that we recognize how theoretically embedded we are when we enter the field. Rather than seek to repress this as bias, we turn it into a resource for constructing the linkage of micro and macro. Third, history and sociology do not occupy watertight compartments; we are living history as we do research. Conceived of as a succession of revisits, participant observation is itself inherently historical—how we see ourselves today is inherently shaped by how we were yesterday. Once again theory helps us tie together past and present. Finally, theory lies like a stagnant pool if it is divorced from its lifeblood, empirical research, which, paradoxically, also threatens its very existence. The vitality of a theoretical tradition depends upon continually being put to the test and then meeting it with ingenious strategies of survival.

Where positivist science denies and represses these antinomies, reflexive social science centers them, making them the object of reflection, not in the abstract but by situating them in the context of their production. We are a participant and observer in the way we study others but also in the way we understand our own practice as social scientists. This is not a hindrance but an indispensable support for social research. The extended case method tries to follow these principles of reflexive science.

IN THE FIELD WITH THE EXTENDED CASE METHOD

I was correct: my prospects for staying in Berkeley were poor. The department was locked in an internecine struggle over my tenure, but beyond the department, away from its microworld, Berkeley faculty were more open to the way I did research. Indeed, the further from the department, the more positive the evaluation, and as my case climbed through the university hierarchy, so the reception became warmer, until, in a final grand reversal, the all-powerful budget committee granted me tenure. Surely this was a case of macro damning the micro, although the outcome was never predetermined, as it was the product of academic warfare.

I returned to Berkeley from Madison in 1983 to take up unfinished tasks, to resume my defense of the extended case method, in effect connecting two opposed traditions within the department—the detailed analysis of microprocesses and the sweeping accounts of macrostructures. Analyses of local production of science, delinquency and deviance, emotional labor, and schooling stood opposed to studies of legal systems, the organization of communism, the history of managerial ideology, social revolution, industrial revolution, the social bases of liberal democracy, the changing character of the welfare state, and so forth. Although I didn't see myself as a bridge—indeed, I was irredeemably identified with one faction of the department—at first subconsciously and then ever more consciously I took it upon myself to sew together these two visions of sociology: on the one hand by elaborating a method that would move from heaven to earth through studying the microfoundations of macroprocesses and, on the other, by elaborating a method that would

move from earth to heaven through studying the macrofoundations of microprocesses.

I had to excavate and bring to the surface the tacit skills I had learned in Zambia under the guidance of my teacher Jaap Van Velsen. What was it that I did when I practiced the so-called extended case method? I needed to understand its theory of practice, its methodological assumptions, and even its philosophical foundations. I became more reflective in the way I conducted research. *Manufacturing Consent* made claims about the way industrial work was organized in capitalism and the class consciousness of its workers. To make the argument more convincing it was incumbent on me to show how things were different in noncapitalist societies. But what noncapitalist societies could I study?

On August 14, 1980, the Polish working class erupted in a way no working class had ever done before. Moreover, it was collective action organized against state socialism and perhaps, so I thought, on behalf of a democratic socialism. My attention was riveted by what came to be known as Solidarity's self-limiting revolution, and I resolved to make my way into the Polish proletariat. As so often happens with academics, my bags were not even packed when events passed me by. On December 13, 1981, sixteen months after it had begun, the movement was suppressed by a military coup. The gates to Poland slammed shut before this ethnographer could reach them.

I did the next best thing. by accepting the invitation of Iván Szelényi to visit Hungary. Why had Hungary escaped such a working-class revolt? After all, in 1956 it was the Hungarian and not the Polish working class that staged the most dramatic confrontation and self-organization against the party-state. One might have expected the Solidarity movement to have taken shape in Hungary, not Poland. So from 1983 to 1989 I migrated from factory to factory in search of an answer, trying to understand the specifically socialist character of Hungarian work organization, work regulation, and working-class consciousness. My sorties into the hidden abode of socialist production pursued a two-layered comparison: why Poland and not Hungary had been the scene of working-class mobilization in 1980 and why a working-class revolt had occurred in state socialism rather than advanced capitalism (here the comparison was between Hungary and the United States). How could the divergent class experiences in the United States and in Hungary be attributed to the very different political economies in which workers were embedded (see chapter 4)?

In this case my comparative studies allowed me to explore the macrofoundations of microprocesses. That is, I started with social processes on the shop floor and extended out to the macroforces shaping them. If south Chicago and now Hungary offered me the opportunity to study the macrofoundations of a microsociology, what about microfoundations of a macrosociology? Here I became a participant observer within the field of sociology, within the production of knowledge. Curious as to how the dedicated scholarship of Theda Skocpol had produced such a wooden theory of revolution, while Leon Trotsky's deep involvement in the Russian Revolution generated such a compelling account, I compared their methodologies. I contrasted Skocpol's clearly enunciated science at a distance with Trotsky's science of engagement (see chapter 3). In this way I tried to understand how Trotsky became such an astute critic of the Russian Revolution, how his reflective participant observation

had enabled him to grasp the social processes that underlay its inevitability as well as its dénouement. Compared with Skocpol's positivist science, which presupposed that all revolutions happen (and turn out) in the same way, Trotsky's reflexive science differentiated the French, German, and Russian revolutions. Each had its distinctive dynamics and outcomes. But Trotsky's theory did not spring tabula rasa from the data but from wrestling with and refashioning Marxist theory. The pessimism of Skocpol and the optimism of Trotsky derived not so much from the obduracy or malleability of the world as from the way each engaged and interpreted it.

Here lies the secret of the extended case method—theory is not discovered but revised, not induced but improved, not deconstructed but reconstructed. The aim of theory is not to be boringly right but brilliantly wrong. In short, theory exists to be extended in the face of external anomalies and internal contradictions. We don't start with data, we start with theory. Without theory we are blind—we cannot see the world. Theory is the necessary lens that we bring to our relationship to the world and thereby to make sense of its infinite manifold. Everyone necessarily possesses theory—understanding how the world works, linking cause and effect—but some specialize in its production. The practice of social science is becoming aware that theory is its precondition.

Those who would have us strip ourselves of theory before we enter the field are deceiving themselves. In their supposed purity they become the unconscious victims of the bias they seek to avoid. Far better to become conscious of our theoretical baggage, turning it to our advantage rather than letting it drag us down into the marshlands of empiricism. And, of course, last but not least, theory makes it possible for us to extend from the micro to the macro, to identify the forces at work in confining and reproducing micro social processes.

BACK IN BERKELEY WITH THE EXTENDED CASE METHOD

While I became more conscious of the methodological principles guiding my research even as I did the research, I discovered those principles not from gazing at my navel—although there was quite a bit of that—but through interacting with others, particularly in teaching. Teaching is research through other means. Teaching is not about filling empty vessels with useful knowledge; it is a dialogue of self-realization, both teacher and taught. Teaching is a form of participant observation—a process of learning what it means to be a sociologist.

On returning to Berkeley I began teaching the required introductory methodology course for first-year graduate students but only after I had taken a reading course under the supervision of the then–graduate student Tom Long. Under his guidance I steeped myself in philosophy of science and then set about organizing the methodology course around a single question: Is sociology a science? In the first half of the semester we interrogated the different meanings of science, ranging from the crudest inductivism to Feyerabend's anarchism, culminating with the methodology of scientific research programs, and in the second half we examined hermeneutic alternatives to and critiques of sociology as science, many of which harbored misguided criticisms of science. We ended up with Habermas's ecumenical account of knowledge and human interests, combining positivist,

interpretive, and critical approaches. In teaching this course I convinced myself of the centrality of theory to all social research.

The inspiration behind and motivation for these explorations in the philosophy of science and antiscience lay with the participant observation seminars I had continued to run, seminars that generated many profound and unanswered questions about our quest for knowledge. The seminar works as follows: Students arrive with their half-baked projects, and I tell them that in three days they have to give me a short proposal that describes why they want to study the particular site they have chosen, how they are going to study it, and, most important, what they expect to find. Their expectations are bound to be wrong, I tell them, and so immediately this sets up a puzzle—why did they think X and yet find Y, with what theory are they working that is so clearly wrong? I tell them that their proposal is the first draft of the final paper, and the semester will entail revising it many times to accommodate at least some of the surprises (anomalies) that the field will throw up. They begin with a theory—even if they are not aware of it—and they will never leave theory. Theory guides their research from day to day, suggesting hypotheses to be investigated and anomalies to be tackled.

In this version of ethnography we don't deliver our minds from preconceptions but clarify and problematize them; we don't accumulate data day after day only finally to code it and thereby infer theory at the end, as though no one else had thought of these matters before, but we continually engage theory with data, and theory with other theories. Theory is the condensation of accumulated knowledge that joins sociologists to one another; it is what makes us a community of scientists. We are theory bound.

A course like this runs itself. I may dominate the conversation for the first week or two, but then I'm slowly marginalized as students quickly learn how to engage each other's work. My ignorance of their sites becomes a pedagogical virtue. Students, hitherto shy and retiring, flower as they develop the confidence that comes with a monopoly of knowledge about their sites. Students may be responsible for their own project, but they participate in everyone else's project. I work with students one on one in my office and by e-mail, going through their notes and their analysis. Tell me, I ask them, why should I care about your site, your findings, how does this add to some body of sociological knowledge, some sociological theory of how the world works? What theory that you consider important is challenged by your observations, and how then can you improve it?

I taught this course for fifteen years—sometimes it degenerated, disintegrated, but other times it fused into a collective spirit that transcended its participants. On one such occasion I suggested we continue meeting for a second semester, and so we did. It was a particularly convivial group. They liked cooking (and I liked eating), so as we consumed sumptuous meals we planned the rewriting of the papers they had produced the previous semester. In some instances this involved further research. Slowly but surely, often painfully, the papers took shape until we had the manuscript that became *Ethnography Unbound* (Burawoy, Burton, et al. 1991). For me this was the place and time to formulate the principles of the extended case method that we had followed, to write out the knowledge accumulated in teaching and doing participant observation. As a book it did unexpectedly well in disseminating an alternative approach to participant observation.

The principles are quite simple. The first principle is the extension of the observer into the community being studied. The observer joins the participants in the rhythm of their life, in their space and their time. The observer may remain an observer (nonparticipant observation) or be an active member (participant observation). The observer may declare her intentions—overt participant observation—or remain incognito—covert participant observation. The second principle is the extension of observations over time and space. There is no way to predetermine how long the observer is in the field, but it has to be long enough to discern the social processes that give integrity to the site. Here we look for signifying events and dramas, rituals of reproduction as well struggles and contradictions. The third principle is the extension from the microprocesses to macroforces, looking at the way the latter shape and indeed are shaped by the former. We have to be careful not to reify those forces that are themselves the product of social processes—even if those social processes are invisible to the participant observer. The fourth principle is the extension of theory that is the ultimate goal and foundation of the extended case method. We start with theory that guides our interaction with others and permits us to identify relevant forces beyond our site. In the process its inadequacies become apparent in the anomalies and contradictions we seek to rectify. Whether theory is lay or academic, it turns the site into a case that gives meaning to the site beyond its own particularity.

A decade after I began *Ethnography Unbound,* I found myself grounded as department chair and supervising a bunch of brilliant but obstreperous students conducting ethnographies in different parts of the world. I invited them to use their dissertation research to write a book called *Global Ethnography* (Burawoy et al. 2000). Could the extended case method be extended beyond the locality, the region, and even the nation to the globe? They couldn't resist the challenge. During the first semester we read some of the great theories of globalization—the most exciting seminar I've been part of at Berkeley. But we concluded that none of the theories was adequate to the task of global ethnography. Most were floating in the sky, unable to grasp the diversity of studies we embraced: welfare workers in Hungary, shipyard workers in San Francisco, homeless recyclers in San Francisco, women's movements in northern Brazil, nurses from Kerala, software engineers in Ireland, breast cancer movements in the San Francisco Bay Area, union organizing in Pittsburgh, village wastelands in Hungary. While each study could be read on its own, and while we mapped out three approaches to globalization—supranational forces, transnational connections, and postnational consciousness—there really was no worthwhile global theory to reconstruct. So we were left, like the feminists before us, to build up something de novo from the ground.

While *Ethnography Unbound* and *Global Ethnography* were openly collective projects both in process and in product, they are but the tip of the collaborative iceberg that shaped my reflexive ethnography. For nearly thirty years I have held a dissertation seminar that met weekly or biweekly. It has been the forge for many dissertations and books. In these seminars, in the dark chamber of my abode, we learned together, sometimes quite tortuously, what we were up to. The essays that follow were first presented in these seminars, and therefore it is to their participants that I dedicate this book.

ONE

The Extended Case Method

Race and Class in Postcolonial Africa

> Methodology can only bring us reflective understanding of the means which have *demonstrated* their value in practice by raising them to the level of explicit consciousness; it is no more the precondition of fruitful intellectual work than the knowledge of anatomy is the precondition of "correct" walking.
>
> Max Weber, *The Methodology of the Social Sciences*

True, anatomical knowledge is not usually a precondition for "correct" walking. But when the ground beneath our feet is always shaking, we need a crutch. As social scientists we are thrown off balance by our presence in the world we study, by absorption in the society we observe, by dwelling alongside those we make "other." Beyond our individual involvement is the broader ethnographic predicament—producing theories, concepts, and facts that destabilize the world we seek to comprehend. So we desperately need methodology to keep us erect, while we navigate a terrain that moves and shifts even as we attempt to traverse it.