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.

Like other handicaps, the ethnographic condition can be dealt with in one of two ways: containing it or turning it to our advantage. In the first strategy we minimize our predicament by limiting our involvement in the world we study, insulating ourselves from our subjects, observing them from the outside, interrogating them through intermediaries. We keep our feet on the ground by adhering to a set of data-collecting procedures that assure our distance. This is the positive approach. It is best exemplified by survey research in which every effort is made to suspend our participation in the world we study. We try to avoid affecting the situation we study and attempt to standardize the collection of data, bracket external conditions, and make sure our sample is representative.

In the alternative strategy we thematize our participation in the world we study. We keep ourselves steady by rooting ourselves in theory, which guides our dialogue with participants. Michael Polanyi (1958) elaborates this idea in detail, rejecting a positivist objectivity based on "sense data" in favor of a commitment to the "rationality" of theory—cognitive maps through which we apprehend the world. This "dwelling in" theory is at the basis of what I call the reflexive model of science—a model of science that embraces not detachment but engagement as the road to knowledge. Premised upon our own participation in the world we study, reflexive science deploys multiple dialogues to reach explanations of empirical phenomena. Reflexive science starts out from dialogue, virtual or real, between observer and participants, then embeds such dialogue within a second dialogue between local processes and extralocal forces that in turn can be comprehended only through a third, expanding dialogue of theory with itself. Objectivity is not

measured by procedures that assure an accurate mapping of the world but by the growth of knowledge, that is, the imaginative and parsimonious reconstruction of theory to accommodate anomalies (see Kuhn 1962; Popper 1963; and Lakatos 1978).

The extended case method applies reflexive science to ethnography in order to extract the general from the unique, to move from the "micro" to the "macro," to connect the present to the past in anticipation of the future, all by building on preexisting theory. In my own use of the extended case method I have drawn on my experiences as a research officer in the Zambian copper industry to elaborate Fanon's theory of postcolonialism. I tried to expose the roots of consent to American capitalism by applying Gramsci's theory of hegemony to my experiences as a machine operator in a South Chicago factory. I have explored the nature of work organization and class formation under socialism by combining Szelényi's theory of class structure and Kornai's theory of the shortage economy. This was based on laboring in Hungarian factories—champagne, auto manufacturing, and steel. Subsequently, I worked my way outward from a small furniture factory in northern Russia in order to develop theories of the transition from socialism to capitalism. Here I drew on Marxist notions of merchant and finance capital. How can I justify these extravagant leaps across space and time, from the singular to the general, from the mundane to the grand historical themes of the late twentieth century? That is the question that motivates this chapter.

Although it is more usual for ethnographic studies to confine themselves to claims within the dimensions of the everyday worlds they examine, I am not alone in "extending out" from the field. Indeed, this was one of the hallmarks of the Manchester School of social anthropology, which first coined the phrase "extended case method."[1] Instead of collecting data from informants about what "natives ought to do," Manchester anthropologists began to fill their diaries with accounts of what "natives" actually were doing, with accounts of real events, struggles, and dramas that took place over space and time. They brought out discrepancies between normative prescriptions and everyday practices—discrepancies they traced to internal contradictions but also to the intrusion of colonialism. Manchester anthropology began to restore African communities to their broader, world historical context.

Not just in Africa but in the United States too there is a rich but inchoate tradition of scholarship in the implicit style of the extended case method. Community ethnographies have not always stopped at the tracks but incorporated the wider contexts of racism and labor markets as well as urban political regimes.[2] Workplace ethnographies, traditionally confined to "plant sociology," have also taken into account such external factors as race and ethnicity, citizenship, markets, and local politics (see Lamphere et al. 1993; Thomas 1985; Smith 1990; and Blum 1991). Participant observation studies of social movements locate them in their political and economic context (see Fantasia 1988; Johnston 1994; and Ray 1998). Ethnographies of the school have always sought to explain how education is shaped by and at the same influences wider patterns of social inequality (see Willis 1977 and MacLeod 1987). Family ethnographies have found it impossible to ignore influences beyond the household, upholding Dorothy Smith's feminist injunction to locate lived experience within its extralocal determinations.[3]

The rudiments of the extended case method abound in these examples and elsewhere. What I propose, therefore, is to bring

"reflective understanding" to the extended case method by raising it to the "level of explicit consciousness." But, contra Weber, this is not simply a clarificatory exercise. It has real repercussions for the way we conduct social science. Indeed, it leads to an alternative model of social science and thus to alternative explanatory and interpretive practices—something social scientists are reluctant to countenance. We prefer to debate appropriate techniques or even tolerate the rejection of science altogether rather than face the possibility of two coexisting models of science, which would wreak havoc with our methodological prescriptions. Still, I hope to demonstrate that reflexive science has its payoff, enabling the exploration of broad historical patterns and macrostructures without relinquishing either ethnography or science.

By ethnography I mean writing about the world from the standpoint of participant observation; by science I mean falsifiable and generalizable explanations of empirical phenomena. In developing my argument it will be necessary to distinguish (a) research method (here survey research and the extended case method), which is the deployment of (b) techniques of empirical investigation (here interviewing and participant observation) to best approximate (c) a scientific model (positive or reflexive) that lays out the presuppositions and principles for producing science. In elaborating the different dimensions of the extended case method, I seek to present it as a science, albeit a reflexive science, to improve its execution by recognizing its limitations and to draw out broader implications for the way we study the world.

In order to illustrate and explicate the extended case method, I return to a study conducted between 1968 and 1972 in the then-newly independent African country of Zambia. Of all my studies I have chosen this one because it most effectively illustrates both the virtues and the limits of the extended case method. First, the virtues: The extended case method is able to dig beneath the political binaries of colonizer and colonized, white and black, metropolis and periphery, capital and labor to discover multiple processes, interests, and identities. At the same time the postcolonial context provides fertile ground for recondensing these proliferating differences around local, national, and global links. Second, the limits: The extended case method comes up against the very forces it displays. As the renascent field of "colonial" studies makes clear, the colonies were not simply the site of exotica but of experiments in new tactics of power, subsequently reimported to the metropolis (see, for example, Stoler 1995 and Mitchell 1988). Domination took on especially raw and exaggerated forms, transparently implicating sociologists, and especially anthropologists, coloring their vision in unexplicated ways (Clifford and Marcus 1986; Asad 1973). Colonial and postcolonial regimes of power throw into relief limits inherent to the extended case method.

Accordingly, this chapter is constructed as follows: I begin with a narrative of my study of the Zambian copper industry, highlighting the social embeddedness of reflexive research (Burawoy 1972a, 1972b, 1974). I then show how my study violated each of the four principles of positive science. If there were only this model of science, I would have to abandon either the extended case method or science. However, the extended case method is not alone in violating positive principles. I show that survey research, the quintessentially positive method, transgresses its own principles because of inescapable context effects stemming from the indissoluble connection between interviewer and respondent, and from the embeddedness of the

interview in a wider field of social relations. We can either live with the gap between positive principles and practice, all the while trying to close it, or formulate an alternative model of science that takes context as its point of departure, that thematizes our presence in the world we study. That alternative is the "reflexive" model of science that, when applied to the technique of participant observation, gives rise to the extended case method.

In saving both science and the extended case method, however, I do not eliminate the gap between them. Making context and dialogue the basis of an alternative science unavoidably brings into prominence power effects that divide the extended case method from the principles of reflexive science. Postmodernism has done much to highlight these power effects but, rather than make do with an inadequate science, it rejects science altogether. I find myself working on the borders of postmodernism, without ever overstepping the boundaries. If we choose to remain on the side of science, we have to live with its self-determined limitations, whether they be the context effects of positive science or the power effects of reflexive science. Given that the world is neither without context nor without power, both sciences are flawed. But we do have a choice. So I finally ask when, where, and why we deploy each of the two models of science and their corresponding methods.

THE ETHNOGRAPHIC CONDITION EMBRACED

Reflexive science sets out from a dialogue between us and them, between social scientists and the people we study. It does not spring from an Archimedean point outside space and time; it does not create knowledge or theory tabula rasa. It starts out from a stock of academic theory on the one side and existent folk theory or indigenous narratives on the other. Both sides begin their interaction from real locations.

My own study of the Zambian copper mines began from publicly debated dilemmas of the legacies of colonialism. I traveled to the Copperbelt in 1968 in search of the policies and strategies of transnational corporations toward the postcolonial regime. The two mining companies, Anglo American Corporation and Roan Selection Trust, had their roots in the colonial order of Northern Rhodesia, a British protectorate until 1964. How were these companies responding to Zambian independence, whose stated objective was to reappropriate control of the nation's economy? This was not a trivial question since the copper industry employed about fifty thousand people, 90 percent African and 10 percent expatriate. At the time of independence the mines provided 90 percent of foreign exchange and 50 to 70 percent of government revenue. As far as Whitehall (and later the Federation of Rhodesia and Nyasaland) had been concerned, Northern Rhodesia's reason for existence was its copper. Road and rail transportation, land and agriculture, taxation and trade, labor and education, nationality and race were all designed to maximize the export of copper. Zambia was the archetypal enclave economy, with copper mining its organizing principle.

It was easier to study how mineworkers were faring than to disclose the mysterious corporate practices of Anglo American Corporation and Roan Selection Trust. Mine was not a study that could be accomplished by combing through documents because, as I was to learn, they revealed so little. Interviews, conducted from the outside, were no more useful since managers were protected by layers of public relations officials. Instead I

took advantage of my recently won mathematics degree and my contact at Anglo's headquarters to land myself a job in the Personnel Research Unit of the Copper Industry Service Bureau. Located in Kitwe, at the heart of the mining region, this unit was the center of industrial relations, both policy and practice.

Once there my attention turned to the more specific question of labor force localization, or what was called African advancement but since independence had come to be called Zambianization. Colonial rule left Zambia's four million people with barely one hundred university graduates and just more than twelve hundred Africans with secondary school certificates. So the country remained heavily dependent on white managers and experts. Historically, the mining industry had been organized according to the color bar principle, that is, no black person should exercise authority over any white person. A major aim of the anticolonial movement was to eradicate all such traces of white supremacy. How had things changed in the postcolonial period? I began with the figures put out by the new government's Zambianization Committee, which painted a rosy picture of achievement. Four years after independence fewer expatriates and more Zambians held "expatriate" (white) positions. What lay behind this portrait of deracialization?

If comprehension of managerial strategies was largely barred to outsiders, any serious study of Zambianization was totally off limits. Racial succession in what had been an apartheid order was simply too explosive a question to openly investigate. Yet it hung like a heavy cloud over all aspects of industrial relations. I could not have been better placed to observe the different forces at work. Not only was I sitting in the mining industry's data-gathering center but I became an active contributor to the industry's new job-evaluation scheme, which aimed to integrate black and white pay scales. As part of my job I learned the stakes in negotiations among management, unions, and government.

So much for the perspective from the top. How did Zambianization look from inside and from the bottom? Here I had to be more surreptitious. I organized a survey of the working and living conditions of African miners, unrelated to Zambianization. But as interviewers I chose young Zambian personnel officers who, I had reason to believe, were at the storm's eye of Zambianization. We would meet every week in the nominally desegregated Rokana Club to discuss the progress of the survey but also Zambianization. Still, this was not enough. I worked in the Personnel Research Unit for one and a half years and continued the research for another two years while I was a master's student at the University of Zambia. There I recruited undergraduates to join me in studying postcolonial work organization, underground and on surface. At least officially, that was our goal; we were also exploring Zambianization from below, from the standpoint of the vast majority of unskilled and semiskilled workers. How did they feel about the Zambianization of supervisors and lower-level managers?

Our extended observations showed that white management used two types of organizational maneuvers to meet government Zambianization targets as well as management's own interest in maintaining the color bar. The first strategy was blanket Zambianization. In the days of colonialism the personnel manager was king, reigning over his African supplicants and to a lesser extent over whites, too. The personnel department was lord of the company town, of life in the mine and the "compound."[4] An obvious target, the department was entirely and

rapidly Zambianized but at the same time shunted aside and stripped of its powers, especially over expatriate employees. They were placed under the guardianship of the newly created "staff development adviser"—one of the white former personnel managers.

The second strategy was shadow Zambianization. During the three and a half years of our research the position of mine captain, that is, the highest level of underground supervision, was Zambianized. A number of old white captains were promoted into the newly created post of assistant underground manager and took with them many of their old powers and responsibilities. Any Zambian successor had to operate in the shadow of his predecessor. He became a buffer between his subordinates and the "real" mine captain, now removed to a comfortable office on the surface.

These maneuvers to maintain the color bar had several organizationally dysfunctional consequences. First, the organization became increasingly top heavy as the layers of management thickened. Second, conflict increased between workers and their new Zambian supervisors, who were less effective, even if less abusive, than their predecessors. Maintaining the color bar through Zambianization was a recipe for organizational rupture, conflict, and inefficiency.

If blanket and shadow Zambianization undermined the organization, why did it continue? What were the forces behind the retention of the color bar? How could a nationalist black government ignore the continuity of the racial order, as it effectively did in its Zambianization report? I sought the answer in the broader constellation of interests. First, while the government embraced the rhetoric of Zambianization, African trade unions, representing the unskilled and semiskilled miners, were more interested in higher wages and better working conditions than in the upward mobility of supervisors. Second, Zambian successors, caught between black subordinates and white bosses, were a lightning rod for racial and class tensions. They were organizationally weaker than white management, which retained a virtual monopoly of knowledge and experience.

Third, corporate executives in the industry had long fought to raise the color bar and replace white with black since this reduced labor costs. If the executives faced organized resistance from white staff before, now they were threatened with exodus. Fourth, the Zambian government regarded the mining industry as a sacred cow, the source of revenue for its nation-building projects. It did not dare jeopardize profits from copper. Moreover, it was content to let expatriates run the industry because, although they had economic power, they did not pose a political threat. They were on limited three-year contracts that could be terminated at will. Zambian managers, as a powerful faction of the dominant class, could pose many more problems for a Zambian government. This balance of forces meant that, despite independence, the overall class and racial patterns in the mines did not change substantially.

From the microworlds of Zambianization I "extended out" to the class forces maintaining not only the old racial order but underdevelopment in general. That is to say, obstacles to development arose not only from dependence on copper in a world economy controlled by advanced capitalist nations but also from the reproduction of the class relations inherited from colonialism. An emergent African "national bourgeoisie" had class interests in a racial order that inhibited economic transformation. Thus my study had reconstructed and reconfigured indigenous

narratives into a class analysis of postcolonialism, which, as I will show, fed back into society in unanticipated ways. First, I want to translate this research into the language and terms of the extended case method and the science it represents.

POSITIVE SCIENCE REVISITED

What is positive science? For August Comte sociology was to replace metaphysics and uncover empirical laws of society. It was the last discipline to enter the kingdom of science, but once admitted it would rule over the unruly, producing order and progress out of chaos. Thus positivism is at once science and ideology. Today sociology has, for the most part, dropped its pretensions to a ruling ideology, and we can call this stripped-down version of positivism simply positive science. The premise that distinguishes positive from reflexive science is that there is an "external" world that can be construed as separate from and incommensurable with those who study it. Alvin Gouldner (1970) once called this premise methodological dualism—social scientists are exempt from the theories they develop about others. Positive science calls for the distancing of observer from the object of study, a disposition of detachment. The purpose of positive science is to produce the most accurate mapping of the workings of this external world, to mirror the world (Rorty 1979).

Constituting the observer as outsider requires an effort of estrangement, facilitated by procedural objectivity. In his exemplary discussion of "analytic fieldwork," Jack Katz (1983) lays out the "4R's," what I refer to as the four prescriptive tenets of positive science. First, sociologists must avoid affecting and thus distorting the worlds they study. This is the injunction against reactivity.

Second, the external world is an infinite manifold, so we need criteria for selecting data. This is the principle of reliability. Third, the code of selection should be formulated unambiguously so that any other social scientist studying the same phenomena could produce the same results. This is the principle of replicability. Fourth, we must guarantee that the slice of the world we examine is typical of the whole. This is the principle of representativeness.

Katz accepts these principles as definitive of social science. He tries to show how participant observation can live up to the positivist ambition, namely, the 4R's, if it follows "analytic induction," or what he prefers to call analytic research. However, in the process he radically destabilizes his methodological principles, embracing rather than proscribing reactivity, dissolving the boundary between fact and fiction, and summoning readers to replicate findings from their own experiences. Still, unperturbed, he holds on to the 4R's. I take the opposite tack, forsaking positive science for reflexive science, which is more appropriate to the extended case method. I justify invoking and elaborating this alternative by first showing how the extended case method violates the 4R's and then how even survey research fails to live up to those same positive principles. My intention here is not to reject positive science but to show how positive science rejects the extended case method and in particular my study of Zambianization.

Positive Science Violated

The extended case method makes no pretense to positive science but, to the contrary, deliberately violates the 4R's. My Zambianization research broke the injunction against reactivity. I was anything but a nonintervening observer. I entered the Personnel Research Unit just as it was undertaking a mammoth job-evaluation exercise to categorize the complex industrywide occupational structure with a view to bringing white and black pay structures into a single hierarchy. It was critical that the job hierarchy already established within each racial group be maintained. In order to give the impression of fairness, integrating the two pay scales was based on a joint team of "experts" from union and management, which "evaluated" each job according to a pregiven set of characteristics, experience, education, dexterity, effort, and so forth. An English consulting company, brought in to attempt to match the evaluation of jobs with the preexisting hierarchy, failed abysmally. With my mathematical training I was able to turn the task into a simple problem of linear programming and thereby helped to reproduce the very racial order that became the focus of my research in *The Colour of Class on the Copper Mines* (1972a).

Reliability was also violated. Having a fixed coda or prism through which to observe and extract information makes one unresponsive to the flux of everyday life. Living in the time and space of those one studies makes it difficult to fit the world into a predefined template. One begins with one set of questions and ends with very different ones. Thus I entered the mining industry in search of some company policy guiding relations with the Zambian government. It was only by working for the company executives that I realized that there was no such policy. Nor was it rational, as I subsequently realized, to follow a predetermined strategy in situations of great uncertainty—political uncertainty (frequent government crises, changes in ministerial personnel, or surprise moves such as the nationalization of the mines); economic uncertainty (especially the volatile world price of copper); and technical uncertainty (unexpected problems of excavation, mine disasters). In such a turbulent environment managers need to be flexible, not hamstrung by detailed plans. As I discovered, those policies that did exist were constructed in posthoc fashion, by "experts" like myself, to justify decisions already made. Had I not been a participant in these processes, I would still be looking for that elusive company policy or, more likely, would have concocted a policy from company rationalizations. In short, with the extended case method dialogue between participant and observer provides an ever-changing sieve for collecting data. This is not to deny that we come to the field with presuppositions, questions, and frameworks but that they are more like prisms than templates and they are emergent rather than fixed.

By the same token, replicability was also problematic. The data I gathered was very much contingent on who I was—a white male recently graduated from a British university with a degree in mathematics, a newcomer to colonialism, and an idealist to boot. Every one of these characteristics shaped my entry and performance in social situations and how people spoke to me of racial issues. More than that, anyone who subsequently replicated my study of Zambianization would come up with very different observations. History is not a laboratory experiment that can be replicated again and again under the same conditions. There is something unique about the ethnographic encounter. It certainly would have been interesting for someone else to repeat the study, either simultaneously or subsequently, not as a replication but as an extension of my own study.[5]

And so, finally, we come to the inevitable question of representativeness that dominates the positivist critique of ethnography. How representative were my observations of the process of Zambianization within my two cases? How representative were my case studies of all the case studies at the one mine I studied, let alone of the six other mines or indeed of industries beyond? How could I draw any conclusions beyond my two unique cases? And if I could not generalize, why did I bother to devote three and a half years to the study?

These are valid criticisms from the standpoint of positive science, and if this were the only model of science, I would indeed have wasted my time. However, there is a second approach to science, a reflexive approach that also seeks generalizable and falsifiable explanations. This alternative does not appear magically but, true to its own principles, arises from a critical engagement with positive science. But first I must show that no method, not even the best survey research, can live up to positive principles, for the principles of reflexive science spring from this irrevocable gap between positive science and its practice.

Positive Science Delimited

Survey research is avowedly positive in its method. It tries to live up to the 4R's by delivering the 4S's. In order to overcome the problem of reactivity, the interview is constructed as a uniform, neutral stimulus that elicits varied responses. The respondent is supposed to react to the question and the question alone, stripped of the medium in which it is posed. To confront the problem of reliability and achieve a consistent set of criteria for the selection of data, the interview is standardized; identical questions are asked in identical ways of each respondent. For replicability not only has the question to be a stimulus, "isolated" from the interview, but the external conditions must be controlled, that is, stabilized or deemed irrelevant. Finally, for representativeness the respondents must be a carefully selected sample of the broader targeted population.

Despite their best efforts survey researchers have always and inevitably fallen short of their positive goals. The interview is a social context, embedded in other contexts, all of which lend meaning to and are independent of the question itself. There are four types of context effects. The well-documented interview effects create the problem of reactivity, in which interviewer characteristics (for example, race or gender) or the interview schedule itself (for example, order or form of questions) significantly affects responses (see Hyman et al. 1954; Converse and Schuman 1974; and Schuman and Presser 1981). There are also respondent effects in which the meaning of questions has an irreducible ambiguity, dependent on the different worlds from which the respondents come. Standardizing the questions cannot eliminate respondent effects (see Cicourel 1967, and Forsyth and Lessler 1991). Field effects simply recognize that interviews cannot be isolated from the political, social, and economics contexts within which they take place. Responses to interviews conducted at different points in time or in different places will be shaped by such extraneous conditions. Replication is thwarted by external factors we do not control. We cannot even disentangle their unmediated impact from their mediated impact on the respondent during the interview itself.[6] Finally, situation effects threaten the principle of representativeness. Insofar as meaning, attitudes, and even knowledge do not reside

with individuals but are constituted in social situations, we should be sampling from a population of social situations and not a population of individuals.[7] But we have no idea how to determine the population of relevant social situations, let alone how to draw a sample.

There is nothing new here—serious survey researchers spend their lives trying to minimize and/or control for context effects, assuming them to be noise that can be investigated if not expurgated. If early research into survey research simply revealed interview effects, more recent work has begun to theorize those effects (Suchman and Jordan 1990; Schaeffer 1991; and Tanur 1992). The interview is viewed as a distorted conversation in which one of the interlocutors is absent (the researcher), in which the conversation follows a predetermined trajectory with prescribed responses, and in which dialogue is precluded.[8] Unable to establish common ground with the respondent, the interviewer cannot avoid misunderstandings and mistakes. One response, therefore, is to move toward a more "narrative" interview. Instead of foisting the standardized interview on respondents, the interviewer allows respondents to tell their own story, to offer their own narrative (Mishler 1986). The interviewer proceeds through dialogue, reducing distortion but incurring reactivity and violating reliability, replicability, and often representativeness.

In other words, no one denies the importance of context effects. Survey researchers look upon them as a challenge—they must be measured, reduced, and controlled. However, if one takes the view that context is not noise that disguises reality but reality itself, then improving survey research is tackling the wrong problem with the wrong tools. Thus many regard the ineluctability of context effects as a demonstration of the irremediable flaws of positive science, justifying abandoning science altogether in favor of an interpretive approach to the social world. We can find influential representatives of this "hermeneutic" school across the disciplines: philosophers such as Hans Gadamer (1975) and Richard Rorty (1979) reduce social science to dialogue and conversation; anthropologists such as Clifford Geertz (1973, 1983) regard the art of ethnography as thick description or the excavation of local knowledge; sociologists such as Zygmunt Bauman (1987) argue that intellectuals should abandon their legislative pretensions for an interpretive role, mediating between communities; feminists such as Donna Haraway call for networks of "situated knowledges" (1991, chap. 9).

This is not the approach I propose to follow here. Faced with the ineluctable gap between positive principles and research practice, I neither abandon science altogether nor resign myself to refining practice in order to approach unachievable positive principles. Instead I propose an alternative model of science, a reflexive science, that takes context as a point of departure but not a point of conclusion.

REFLEXIVE SCIENCE DEFINED

Reflexivity in the social sciences is frequently regarded as the enemy of science. Long ago Peter Winch (1958) argued that individual reflexivity, that is, the self-monitoring of behavior, leads to an irrevocable uncertainty in human action, making scientific prediction impossible. All social science can do is reveal the discursive and nondiscursive worlds of the people it studies. Similar views have become common in anthropology wherever the "linguistic" or "interpretive" turn has taken hold. In its extreme form we are so bound by our own preconceptions that we can do little more than gaze into our biographies. Within sociology reflexivity has been put to more positive use. Alvin Gouldner (1970) turned sociology onto itself to discover the "domain assumptions" of reigning paradigms in "Western" sociology, arguing that they were increasingly out of sync with the world they claimed to mirror. More recently, Pierre Bourdieu (1977; 1990; Bourdieu and Wacquant 1992) invites us to a reflexive sociology that explicitly seeks to deepen the scientific foundations of sociology. Recognizing our own place within the disciplinary field enables us to objectify our relation to those we study, which will make us better scientists.

I take a slightly different approach. Rather than arguing that there is one model of science that is best carried out with reflexive awareness, I propose a methodological duality, the coexistence and interdependence of two models of science—positive and reflexive.[9] Where positive science proposes to insulate subject from object, reflexive science elevates dialogue as its defining principle and intersubjectivity between participant and observer as its premise. It enjoins what positive science separates: participant and observer, knowledge and social situation, situation and its field of location, folk theory and academic theory. The principles of this reflexive science can be derived from the context effects that pose as impediments to positive science.

Intervention

The first context that I discussed was the interview itself, which is not simply a stimulus to reveal the true state of the interviewee but an intervention into her life. The interview extracts her from her own space and time and subjects her to the space and time of the interviewer. In the view of reflexive science intervention is not only an unavoidable part of social research but a virtue to be exploited. It is by mutual reaction that we discover the properties of the social order. Interventions create perturbations that are not noise to be expurgated but music to be appreciated, transmitting the hidden secrets of the participant's world. Institutions reveal much about themselves when under stress or in crisis, when they face the unexpected as well as the routine. Instead of the prohibition against reactivity, which can never be realized, reflexive science prescribes and takes advantage of intervention.

Process

The second context is the multiple meanings attached to the interviewer's "stimulus," which undermines the reliability of research. One can standardize the question but not the respondent's interpretation of the question. Respondents come to the interview with multiple experiences derived from different situations that they are then asked to collapse into a single data point. Even asking someone's race or gender can turn out to be complicated, requiring that the respondent reduce a diverse array of experiences to a single item on a check list. There is a double reduction: first aggregation and then the condensation of experience.

Reflexive science commands the observer to unpack those situational experiences by moving with the participants through their space and time. The move may be virtual, as in historical interpretation; real, as in participant observation; or some

combination of the two, as in the clinical interview. But there is another complication. Not only does each situational experience produce its own "situational knowledge," but that knowledge may be discursive or nondiscursive. If the discursive dimension of social interaction, what we may call narrative, can be reached through interview, the nondiscursive, that is, the unexplicated, unacknowledged, or tacit knowledge, sometimes referred to as practical consciousness, which underlies all social interaction, calls for more. It may be discovered through "analysis," for example, or through participation, "doing" things with and to those who are being studied (Garfinkel 1967).

The task of reflexive science does not stop with situational comprehension, with the recovery of situational knowledge. First, there are always multiple knowledges, reflecting the position of different actors within a social situation. Reflexive science would be impossibly cumbersome if its goal were the display of multiple narratives, multiple voices. But worse still, situational knowledge is knowledge located in a specific space and time. Neither space nor time can be frozen, and so situational knowledges are in continual flux. Therefore, like any other science, reflexive science has to perform some reduction. In this instance the reduction is an aggregation—the aggregation of situational knowledge into social process. Just as survey research aggregates data points from a large number of cases into statistical distributions from which causal inferences can be made, reflexive science collects multiple readings of a single case and aggregates them into social processes. The move from situation to process is accomplished differently in different reflexive methods, but it is always reliant on existing theory. Later in this chapter I will discuss how it works with the extended case method.

Structuration

The third context is the external field within which the interview occurs. The field cannot be held constant, so the purpose of replication is thwarted. It is not simply that social scientists shape the world they study in idiosyncratic and therefore nonreplicable ways but that the external field has its own autonomous dynamic. This wider field of relations cannot be bracketed or suspended, yet it is also beyond the purview of participant observation. We therefore look upon the external ~~field~~ as the conditions of existence of the locale within which research occurs. Accordingly, we move beyond social processes to delineate the social forces that impress themselves on the ethnographic locale. These social forces are the effects of other social processes that for the most part lie outside the realm of investigation. Viewed as external to the observer, these social forces can be studied with positive methods that become the handmaidens of reflexive science.[10]

Reflexive science insists, therefore, on studying the everyday world from the standpoint of its structuration, that is, by regarding it as simultaneously shaped by and shaping an external field of forces.[11] This force field may have systemic features of its own, operating with its own principles of coordination and contradiction, and its own dynamics, as it imposes itself on multiple locales.

Reconstruction

The fourth context effect relates to the second, the priority of the social situation over the individual, which problematizes sampling on the basis of individuals. If representation is not feasible,

is there any other way of producing generality? Instead of inferring generality directly from data, we can move from one generality to another, that is, to a more inclusive generality. We begin with our favorite theory but seek not confirmations but refutations that inspire us to deepen that theory. Instead of discovering grounded theory, we elaborate existing theory.[12] We do not worry about the uniqueness of our case since we are not as interested in its representativeness as its contribution to reconstructing theory.[13] Our theoretical point of departure can range from the folk theory of participants to any abstract law. We require only that the scientist consider it worth developing.

But what distinguishes a "progressive" from a "degenerate" reconstruction? Following Karl Popper (1963, chap. 10) and Imre Lakatos (1978), we seek reconstructions that leave core postulates intact, that do as well as the preexisting theory upon which they are built, and that absorb anomalies with parsimony, offering novel angles of vision. Finally, reconstructions should lead to surprising predictions, some of which are corroborated. These are heavy demands that are rarely realized but ones that should guide progressive reconstruction of theory.

Dialogue is the unifying principle of reflexive science, which is dialogical in each of its four dimensions. It calls for intervention of the observer in the life of the participant; it demands an analysis of interaction within social situations; it uncovers local processes in a relationship of mutual determination with external social forces; and it regards theory as emerging not only in dialogue between participant and observer but also among observers now viewed as participants in a scientific community. Theories do not spring tabula rasa from the data but are carried forward through intellectual debate and division. They then reenter the wider world of participants, there to be adopted, refuted, and extended in intended and unintended ways, circulating back into science.[14] Science offers no final truth, no certainties, but exists in a state of continual revision.

THE EXTENDED CASE METHOD

Reflexive science is to the extended case method what positive science is to survey research—the relation of a model to method, legitimating principle to situated practice. Just as we codified survey research so we must now do the same for the extended case method. In this section I return to my Zambianization study to illustrate the extended case method, pointing to ways in which it might have benefited from greater methodological self-consciousness. In the section that follows the one on Zambianization, I will use my case study in the opposite way, to cast light on inherent limitations of reflexive science.

Extending the Observer to the Participant

In the positive view participant observation brings insight through proximity but at the cost of distortion. The reflexive perspective embraces participation as intervention precisely because it distorts and disturbs. A social order reveals itself in the way it responds to pressure. Even the most passive observer produces ripples worthy of examination, while the activist who seeks to transform the world can learn much from its obduracy.[15]

The most seismic interventions are often entry into and departure from the field. Any group will often put up a great deal of formal and informal resistance to being studied at close

quarters—resistance that discloses much about the core values and interests of its members as well as its capacity to ward off danger. Leaving the field is also an intervention since it is then that participants often declare well-kept secrets or pose revealing questions that they had never dared ask the ethnographer before. But the biggest bombshell often comes when outsiders return their findings to the participants. Few people like to be partialized, reduced to reified forces or in any other way made an object of sociological research. Furthermore, most communities are riven by conflicts so that it is impossible to navigate them to everyone's satisfaction, no matter how careful the observer. However painful, ethnographers always learn a great deal from their final intervention.

When I had completed my study of Zambianization, I decided to seek permission for publication from the top executives of Anglo American who had first employed me and then sponsored the research that I had conducted on the mines. They had no idea that I had been studying Zambianization for three years. When I showed them my report, they were shocked and dismayed that I had dared to broach such a sensitive issue. After reading the manuscript, they bluntly refused to allow publication on the ground that it was politically explosive. I countered that the report was based on their own data. They finally threw me a token concession. Since the mines had just been nationalized, the publication decision was no longer theirs but a government responsibility. I took my manuscript to the person in the Ministry of Mines who was responsible for Zambianization. He was an expatriate, new to the job but not to the mines, who saw the report as a way of making his mark by challenging the practices of the mining companies. Based as it was on careful, detailed, inside research, he considered it a powerful weapon to advance Zambianization. "Because it criticizes the government, the trade unions, the Zambian successor, the expatriates and the corporations; because it criticizes everyone, it must be objective," he said.

The monograph was duly published under the title *The Colour of Class on the Copper Mines* by the Institute for African Studies at the University of Zambia. It received a lot of publicity. Its class analysis was hostile to the mining companies as well as to the government and expatriates. Yet corporate managers in Lusaka used it to discipline mine management on the Copperbelt. The stamp of academic certification made it an effective weapon in the hands of the mining companies—a happy marriage of science and power.

No claims to impartiality can release us either from the dilemmas of being part of the world we study or from the unintended consequences of what we write. What we write circulates into the world we seek to comprehend and from there sprays dirt in our face. As I suggest in the next section, this response represents both a confirmation and a challenge to the theory expounded in *The Colour of Class on the Copper Mines.*

Extending Observations over Space and Time

Such dramatic culminations of research happen in miniature every day. Ethnographers join participants for extended periods of time as well as in different places. Each day one enters the field, prepared to test the hypotheses generated from the previous day's intervention. Fieldwork is a sequence of experiments that continue until one's theory is in sync with the world one

studies. It is a process of successive approximation that can, of course, go awry. Wild perturbations between observations and expectations signify poor understanding, while occasional shocks force one into a healthy rethinking of emergent theorizing. At this level theorizing is compiling situational knowledge into an account of social process. How does this work?

Situations involve relations of copresence, providing the conditions for practices that reproduce relations. The archetype of this conceptualization of social situations is the Marxian treatment of production. As workers transform nature into useful things, so they simultaneously produce their own means of existence (necessary labor) and the basis of profit (surplus labor), that is, they reproduce the worker on one side and the capitalist on the other. But this process continues: laborers return the next day, because they have no alternative source of survival. They are therefore subject to the power of capital, or what I have called the political regime of production, which regulates the division of labor, the mobility between positions in the division of labor, rewards, and so on. The point is simple: Production becomes reproduction only under a particular structure of power. We can compile situational knowledge into an account of social process because regimes of power structure situations into processes.

This can be applied to my case study. Zambianization takes place under the erosion of "colonial despotism" toward a less punitive production regime but one still based on the color bar. Working with the vocabulary of Anthony Giddens and William Sewell, one can say that, within this political regime, resources (money, skill, education, prestige, etc.) are distributed along racial lines supported by schemas (norms, beliefs, theories, etc.)

of racial supremacy.[16] The Zambianization process is set in motion when a Zambian is promoted to replace an expatriate. The expatriate seeks to preserve his job (a resource) and looks upon the new incumbent as inferior (schema). Management intervenes to open a new job for the expatriate, who takes with him some of his old authority and responsibility, leaving his successor with few resources. The successor's subordinates, seeing him as a diminished version of his predecessor, withdraw their support and confidence. Unable or unwilling to seek support from his white boss, the new Zambian supervisor resorts to more authoritarian rule, which confirms his subordinates' worst suspicions. In their view the new Zambian successor is worse than his white predecessor—he is trying to re-create the despotism of the past. Subordinates further withdraw cooperation, and the cycle continues until a new equilibrium of force and consent are reached. The regime of power, that is, the color bar, is reproduced.

Three issues are noteworthy. A social situation becomes a social process because social action presupposes and reproduces its regime of power. By participating in terms of the color bar, the color bar is reproduced. Next, in the struggles around the regime of power, history and macrostructures are invoked as resources and schema *within* the social situation. The Zambian successor complains that whites continue to rule the roost, that independence has brought no change. Zambian workers see their new black boss as re-creating the despotic past or imposing a new tribal supremacy. Finally, interventions from outside the social situation have consequences structured by the regime of power. Management may create positions for displaced expatriates as "aids" to the Zambian successor, but the effect is to weaken him. Management may recruit high school graduates to improve the quality of personnel managers, but the effect is to exacerbate conflict between old-timers and young Turks.

The reproduction of the color bar causes hierarchical social relations to change: relations between black and white become more distance and indirect, while relations between black and black become more tense and conflictual. Reproduction of the regime of power is assured from the inside through the deployment of resources and schemas. It is also reproduced from the outside, beyond the realm of participant observation, but this requires the analysis of social forces.

Extending out from Process to Force

I could have closed my study of Zambianization with a demonstration of the general law of the color bar: however the organization changes, authority always flows from white to black. I could have given the law even more power by drawing on evidence from the very different context of the United States, where gender and racial lines also have an uncanny way of reproducing themselves.[17] This would be the strategy of inductive generalization, namely, to seek out common patterns among diverse cases, so that context can be discounted. This might be called the segregative or horizontal approach, in which cases are aggregated as though they were independent atoms. The extended case method, on the other hand, deploys a different comparative strategy, tracing the source of small differences to external forces. This might be called the integrative or vertical approach. Here the purpose of the comparison is to causally connect the cases. Instead of reducing cases to instances of a general law, we make each case work through its connection to other cases.

The Colour of Class on the Copper Mines offered two such connected comparisons. The dominant one was a comparison of Zambianization after independence with African advancement under colonial rule. The second, much less developed, compared the bottom-up Zambianization of the mines with the top-down Zambianization of government. In order to understand why the color bar remained on the Copperbelt despite democratization and the formal dissolution of racism, I dug back into history. Under colonial rule the mining companies had persistently tried to "advance Africans" into positions hitherto monopolized by whites. What little was accomplished took place through job fragmentation and deskilling of white jobs. African trade unions were always ambivalent about this view of African advancement since the majority of their members were more interested in wage increases and improved working conditions. The colonial regime was pressured by the mining companies and the colonial office in London to support gradual African advancement, as much as a safety valve for frustrated aspirations as for profit. The white settler community was an influential counterweight that opposed any upward mobility for Africans. For the most part the colonial state tried to keep out of the fray, entering only as adjudicator when the machinery of industrial relations broke down.

The successor Zambian government, no longer tied to London, became even more beholden to the mining companies as a major source of revenue. While white managers lost their formal political power, their leverage remained since the mines depended upon their expertise. For its part the Zambian political elite

retained expatriates in the commanding heights of the copper industry because it did not want to depend on an indigenous, potentially rival, economic elite. Still, the postcolonial government had to respond to nationalist clamor that Zambians run their own country. It did so, not by a more vigorous pursuit of Zambianization but by nationalizing the mines, which left internal organization untouched. Zambianization from above in the capital propelled Zambianization from below on the Copperbelt.

Far from being independent, the two cases inversely determine each other. The roots of color bar persistence on the Copperbelt lie in its erosion within government. This is the principle of structuration—locating social processes at the site of research in a relation of mutual determination within a field of social forces. But can we go further and ask whether these extralocal forces exhibit a processual character of their own? Do they have a certain "systematicity" that tends to reproduce itself? Once more we can only proceed to such questions with the aid of theory, in this case, Marxist theory. *The Colour of Class on the Copper Mines* partook in a debate about the capitalist state, arguing that the postcolonial state preserved the overall class structure not because it was an instrument of capital but because it was institutionally autonomous from but dependent upon capital. Here was an emergent understanding of the structuring of class forces—a tendency for them to be reproduced domestically on the basis of a national regime of power.

I could have extended the principle of structuration by regarding the arrangement of state and classes within Zambia as a structured process nested in an external constellation of international forces. Instead I stopped at the national level and looked upon international forces not as constraints but as resources mobilized by the ruling elite to legitimate its domination. The new African elite focused on forces beyond national control—terms of trade, price of copper, Western experts, transnational corporations—in order to obscure the class character of post-colonialism. The African governing class deployed neocolonialism in its own version of the extended case method, denying its own class power by claiming impotence before external forces. This perspective of the new elites found its representative within academic discourse as underdevelopment theory, popularized by Paul Baran and then Gundar Frank. Later it would be challenged by comparative studies that focused on the capacity of the state to engineer "dependent development" within a changing world economy. The debate continues today with the emphatic rejection of the entire "developmentalist" project as destructive of underdeveloped countries (Escobar 1995; Ferguson 1990). However, my interest at the time lay in confronting neocolonialism and underdevelopment theory with class analysis, which confined both the local and the extralocal to national boundaries. Looking back now, I underestimated the importance of international forces. Zambia's dependence on a single commodity, copper, whose price has continued to fall on world markets, brought it under the spell of the International Monetary Fund and its structural adjustment programs. Twenty-five years after nationalizing the copper mines, the Zambian government was trying to sell them off to reprivatize them. The government brought back expatriate managers to make the mines more attractive to foreign investors. The Zambian economy is being recolonized at the behest of its own African government.

Extending Theory

The first three "extensions"—intervention, process, and structuration—all call for existing theory. But our stance toward theory itself is kamikaze. In our fieldwork we do not look for confirmations but for theory's refutations. We need first the courage of our convictions, then the courage to challenge our convictions, and finally the imagination to sustain our courage with theoretical reconstruction. If these reconstructions come at too great a cost, we may have to abandon our theory altogether and start afresh with a new, interesting theory for which our case is once more an anomaly.

I was not methodologically self-conscious about theory extension in *The Colour of Class,* but the strategy pervaded the monograph. The very concept of succession was drawn from Alvin Gouldner's (1954) case study of the organizational reverberations of a managerial succession.[18] But where his was a "natural succession," Zambianization was a case of "forced succession," imposed from above and resisted from below. The Zambian successor had to contend with suspicion from his subordinates and resistance to or indifference from his supervisor, as well as his own doubts about his abilities.

Theorization of social process was extended to theorization of the broader social forces. First, I deconstructed the government's Zambianization report. Hidden behind its data lay the real processes of forced succession under the color bar principle. Contrary to the implications of the report, expatriates were as firmly in control of the industry as ever. On the other hand, I drew back from the neocolonial thesis that blamed Zambia's continued backwardness on a conspiracy of international forces.

Again, the point was not that the claims were wrong—obviously, Zambia was held in the vise of multinationals and international trade—but rather that their partiality obscured the class interests of the new ruling elite.

I was more forthright in rejecting theories that attributed underdevelopment to the cultural backwardness of Zambian workers or, as was more common, to their anomic and undisciplined industrial behavior. Robert Bates (1971), for example, claimed that the postindependence Zambian government had failed to discipline the mineworkers. However, careful examination of his and other data on productivity, absenteeism, turnover, disciplinary cases, and strikes provides no basis for his claims. He simply adopted management's and government's class ideology of the "lazy Zambian worker," blaming workers for the inefficiencies and conflicts whose sources lay elsewhere, such as in the continuing color bar (Burawoy 1972b).

Frantz Fanon's theory of the "postcolonial revolution" guided my analysis (Fanon [1952] 1968a, [1961] 1968b). Although I was not explicit in my reconstruction, as I would be now, I sought to extend his theory to Zambia, a colony without a peasant-based national liberation struggle. My analysis of the multinationals, mineworkers, Zambian managers, and expatriates paralleled his dissection of the class interests of the national bourgeoisie, intellectuals, and the peasantry. I turned the government's claims of worker indiscipline, indolence, and anomie against the new ruling elite itself, whose extravagance and self-indulgence emanated from rapid upward mobility. As to the mineworkers themselves, they were the prototype of Fanon's labor aristocracy. They pursued their narrow economic interests, showed little concern for the color bar, and saw nationalization of the mines as a government ruse to impose harsher discipline. *The Colour of Class on the Copper Mines* did more than recast Fanon's class categories; it set the class map in motion by connecting the macroforces, propelling the movement from African advancement to Zambianization, to the microprocesses of succession.

Theory is essential to each dimension of the extended case method. It guides interventions, it constitutes situated knowledges into social processes, and it locates those social processes in their wider context of determination. Moreover, theory is not something stored up in the academy but itself becomes an intervention into the world it seeks to comprehend. Indeed, *The Colour of Class on the Copper Mines* became its own self-refuting prophecy. My man in the ministry, then the media, and finally the mining companies all set out to change the world I had described. They sought to overturn the new governing elite's interest in reproducing the color bar on the Copperbelt.

This refutation, like any other, is not cause for theoretical dejection but an opportunity for theoretical expansion. The forces revealed in my publication efforts corroborated the view of the mining companies as flexibly adapting to government initiatives. Yet they also showed that the government did not always turn a blind eye to the continuation of the color bar, that the interests of the postcolonial state were not as homogeneous as I presented them, and that social forces are themselves the contingent outcome of social processes. In the positive mode social science stands back and observes the world it studies, whereas in the reflexive mode social theory intervenes in the world it seeks to grasp, destabilizing its own analysis.

THE EFFECTS OF POWER

In defending reflexive science and the extended case method, I am not laying claim to any panacea. Just as there is an insurmountable hiatus between survey research and the positive model it seeks to emulate, so a similar hiatus separates the extended case method and the principles of reflexive science. Whereas in the positive model hiatus is the result of context effects, in the reflexive model it is the result of the effects of power. Intervention, process, structuration, and reconstruction are threatened by domination, silencing, objectification, and normalization. However, the self-limitations of reflexive principles resulting from the ubiquity of power are no more reason to abandon the extended case method than context effects are reason to abandon survey research. The goal is to examine those limitations in order to take them into account and perhaps even reduce them.

Domination

The intervening social scientist cannot avoid domination, both dominating and being dominated. Entry is often a prolonged and surreptitious power struggle between the intrusive outsider and the resisting insider.[19] As I hunted through the mining companies' records and participated in high-level negotiations, I deceived them as to my true purpose. To penetrate the shields of the powerful, the social scientist has to be lucky and/or devious; the powerless are more vulnerable. But even they have their defenses. Thus, in making my way to the other side of the color

bar, I had to use the pretext of a survey to make contact with Zambian personnel officers and enlist the help of Zambian students to discover the views of unskilled and semiskilled workers. But this introduced another layer of power within the research team—my whiteness, with all its resources, and their blackness. The students worked underground, in the smelter, and laying railroad tracks, while I conducted interviews with the managers. There was no doubt that I was the bwana, and they worked to rule, delivering field notes but holding back their views. I was replicating the color bar within the research team.

Nor do domination and resistance miraculously evaporate on entering the field. The intervening social scientist faces two interrelated moments of domination, first as participant and second as observer. As participants in sites invested with hierarchies, competing ideologies, and struggles over resources, we are trapped in networks of power. On whomever's side we are, managers or workers, white or black, men or women, we are automatically implicated in relations of domination. As observers, no matter how we like to deceive ourselves, we are on "our own side," as Alvin Gouldner (1973) would say. We are in the field for ulterior reasons. Our mission may be noble—broadening social movements, promoting social justice, challenging the horizons of everyday life—but there is no escaping the elementary divergence between intellectuals, no matter how organic, and the interests of their declared constituency. In short, relations of domination may not be as blatant as they were in the raw racial and class order of the Zambian Copperbelt, but they are nevertheless always there to render our knowledge partial.

Silencing

This brings up the second face of power—silencing. Ruling ideology presents the interests of the dominant class as the interests of all. The nationalist rhetoric of the Zambianization report concealed diverse class and racial interests. How does one disclose this underlying configuration of interests? As participant observers in various workplaces in and away from the mines, we registered the discordant voices of workers, expatriates, and Zambian successors. This is the meat and potatoes of fieldwork. As I compiled our extended observations made in different situations into a social process—the process of Zambianization understood as forced succession—so these voices were reduced to, congealed into, interests. I was able to disclose the specific and conflictual interests that stood behind the rhetoric of nationalism. But this new crystallization of interests inevitably excluded, marginalized, and distorted other voices.

Thus, if I had been truer to the earlier Fanon of *Black Skin, White Masks* rather than the later *The Wretched of the Earth,* I might have explored the formation of colonial subjectivities, especially the Zambian successor, who is the prototype of Fanon's "colonial Negro," caught up in a white world that rejects him as a racial inferior. If my own color had not prevented it, I could have examined the way the colonial and postcolonial regimes induce pathologies that incapacitate the successor and thereby reproduce the Manichean world of white and black, turning African against African. Since silencing is inevitable, we must be on the lookout for repressed or new voices to dislodge and challenge our artificially frozen configurations

and be ready to reframe our theories to include new voices but without dissolving into a babble.

Objectification

In the extended case method the second extension—from voices in social situations to interests in social processes—is followed by a third extension, from interests in social processes to the forces of social structure. Structuration involves locating social processes in the context of their external determination. Thus Zambianization followed the color bar, despite being antithetical to nationalist ideology, because of the balance of external forces, which appear all-determining. Objectification, that is, hypostatizing social forces as external and natural, is an inherent danger of this approach. There are simply limits to the temporal and spatial reach of participant observation, beyond which we substitute forces for processes.

Objectification is more than a methodological device, however; it also reflects the very real power exercised by political, economic, and cultural systems over lifeworlds (Habermas 1987). But their power should not be exaggerated. Forces are always the hypostatized effects of concealed processes, that is, each system depends upon the shifting processes of its own internal lifeworld. Also, lifeworlds—both those we observe directly and those we reduce to forces—are themselves traversed by power, generating needs that escape into the social sphere. Around such discursive need formation congeal social movements that can dislodge systemic forces (Fraser 1989). Finally, systemic forces contain their own contradictions, which burst forth unexpectedly, as when my man in the ministry encouraged a public attack on the mining industry's conduct of Zambianization. Even as we embrace objectification, we should be always prepared for subterranean processes to erupt and break up the field of forces.

Normalization

Finally, reconstructing theory is itself a coercive process of double fitting. On the one side, complex situations are tailored to fit a theory. The field site is reduced to a case, albeit one that is anomalous vis-à-vis theory. On the other side, theory is then tailored to the case, recomposed to digest the anomaly. This mutual fashioning creates an apparatus for reducing the world to categories that can be investigated, sites that can be evaluated, people that can be controlled.[20]

In order to assimilate Zambianization to a form of managerial succession, I expanded Gouldner's theory by introducing the distinction between natural and forced succession. Usual attrition leads to "natural" succession, but Zambianization was a forced succession. In normalizing what was in effect a transfer of control, I played straight into the hands of the mining companies. Racial succession gave them the conceptual arsenal to discipline their own managers. In his review of my book Ben Magubane picked up on this normalizing effect of "succession," which overlooked the "intense but silent class struggle of decolonization," the fact that Zambia was being held to ransom by expatriates (1974: 598).

Magubane overlooked the other side of my analysis, the application of Fanon's theory of decolonization to the Zambian case, the extension beyond the microdynamics of Zambianization to

the class forces upholding the color bar. But here too normalization was at work. It was astonishing to see how a refashioning of Fanon's theory of postcolonialism could be harnessed politically by the very forces it condemned. Yet one should not be entirely surprised, given Marxism's history as a tool of despotism.

Some formal features of Fanon's analysis of colonialism, however, do lend themselves to adoption by multinational capital. He presumes, for example, the destruction of precolonial cultures and thus the fragility of "local" or subjugated knowledges (Lazarus 1993). I too gave scant attention to cultural contestation that drew sustenance from beneath colonial regimes of power, modes of resistance discovered and celebrated by subaltern and postcolonial studies. Challenging or tempering normalization would have required embedding the analysis in perspectives from below, taking subaltern categories more seriously, and, in short, working more closely with those whose interests the study purported to serve.[21]

These four power effects only add grist to the mill of postmodern critics. If context effects demonstrate the impossibility of science, power effects show how dangerous and self-defeating it is. But abandoning science altogether leaves power unaffected and the hegemony of positive science untouched. Postmodernism's dismissal of all science ignores the pivotal distinction between positive and reflexive models.[22] A self-critical positive science concentrates on context effects but thereby obscures the functioning of power. Constructing "detachment" and "distance" depends upon unproblematized relations of power. A self-critical reflexive science, on the other hand, takes context for granted but displays the effects of power so that they can be better understood and contained. The limits of reflexive science lay the basis for a critical theory of society, by displaying the limits of human freedom.

THE IMPLICATIONS OF TWO MODELS OF SCIENCE

Methodological thinking can bring more than Weber claims, more than reflective understanding of already proven practice. In codifying positive science, we subject it to immanent critique, highlighting the gap between principles and practice. This directs our attention not only to the possibilities of improving positive methods but also to formulating an alternative conception of science. Table 1 summarizes my argument, describing the two models of science and corresponding methods, and in each case points to the gap between model and method. There is a circularity in the models: Each takes as its own basis the limits of the other. Positive science is limited by context, which supplies the foundation of reflexive science, while reflexive science is limited by power, the hidden premise of positive science. Knowing the liabilities of each model-method, we can work toward their containment. If we accept this framework, then we have to confront a new set of questions and implications.

Technique, Method, and Model

What is the relationship between techniques of data gathering and model-methods? Does the technique of participant observation, that is, the study of others in their space and time, have to follow the extended case method and reflexive science? Does the technique of the interview, that is, the study of others in the

Table 1. *The Gap between Principles and Practice of Science*

Positive Science			Reflexive Science		
Positive Principles	*Survey Research Method*	*Context Effects*	*Reflexive Principles*	*Extended Case Method*	*Power Effects*
Reactivity	Stimulus/response	Interview	Intervention	Extending observer to participant	Domination
Reliability	Standardization	Respondent	Process	Extending observations over time and space	Silencing
Replicability	Stabilization of conditions	Field	Structuration	Extension from process to forces	Objectification
Representativeness	Sample to population	Situation	Reconstruction	Extension of theory	Normalization

Table 2. *Four Methods of Social Science*

	Models of Science	
Techniques of Research	*Positive*	*Reflexive*
Interview	Survey research	Clinical research
Participant observation	Grounded theory	Extended case method

interviewee's space and time, have to follow survey research and a positive model of science? In each case the answer is obviously no. The techniques of participant observation and interviewing can be conducted according to either reflexive or positive methods, as presented in table 2.

Participant observation, conducted according to positive principles, becomes grounded theory, which brackets involvement as bias and concentrates on deriving decontextualized generalizations from systematic analysis of data (see Glaser and Strauss 1967; Strauss 1987; Becker 1958; Becker et al. 1961; and Gans 1968). Here theory is the result and not the precondition of research. Social scientists are outsiders, and ethnographers are outsiders within, strangers whose objectivity is vouchsafed by distance. Nonparticipant observation is preferred to participant observation. In other words, reactivity is proscribed. To achieve reliability ethnographers gather and analyze their data in a systematic fashion. Coding and recoding field notes into emergent categories provide the prism for further observation. Replication enters as a call for clarity in how categories are derived from data and is less concerned with the replicability of data collection. It creates pressures to suspend context so as to make cases comparable. Finally, to establish the representativeness of their

results, ethnographers should maximize variation within the field through constant comparison, searching for extreme cases in what is called theoretical sampling.[23]

Just as participant observation can follow positive principles, interviews can follow the precepts of reflexive science, in what I call the clinical method. The psychoanalytic variant is a prototype, especially when the analyst is seen as reflexive anthropologist (Chodorow 1999). The relation between analyst and analysand is dialogic and interventionist. Each reconstitutes the other. The analyst tries to recover and work through situationally specific experiences using dream analysis and free association. Process is the leitmotif of psychoanalysis. The element of structuration, that is, locating psychological processes in their wider social context, may not always be present. But here Fanon is an exemplar. His brilliant essays on colonialism, which derive from clinical work in Algeria, demonstrate the interdependence of psychic processes and economic, political, social, and cultural contexts. Finally, the analyst works with an existing body of theory that is continually evolving through attention to concrete cases. Theory is reconstructed.[24] The clinical interview not only instantiates the principles of reflexive science but thematizes its limitations—domination of analyst over analysand, silencing of the past, objectification of personality structures, while the theory itself is heavy on normalizing.

Extending to Historical Research

Can this binary view of science be extended to techniques other than interviewing and participant observation? What does it mean to extend reflexive science to historical research? I deal with this question in chapter 3, where I compare the approaches of Theda Skocpol and Leon Trotsky with the study of classical revolutions. Both are concerned with a comparison of successful and failed revolutions. Beyond that their approaches are diametrically opposed—the one following positive principles and the other reflexive principles. Where Skocpol situates herself outside history to discover the necessary conditions of revolution, Trotsky stands at the center of history to reconstruct Marx's theory of revolution. Where Skocpol standardizes revolutions in order to discover the universal factors that make for their success, Trotsky makes every revolution distinct in revealing its defining social processes. Where Skocpol develops a single explanation of revolution that spans three centuries as though historical time were of no importance, Trotsky shows how the movement of world history—combined and uneven development of capitalism on a world scale—sets off different processes for each revolution. In the one case detachment, factor analysis, decontextualization, and induction; in the other case intervention, process, structuration, and reconstruction. Once more we have two models of science and two methods.

I chose Skocpol and Trotsky to highlight the contrast between positive and reflexive methods. But one need go no further than Max Weber's analysis of the origins of capitalism for an illustration of the extended case method. In asking what it means to be a scientist in a disenchanted, rationalized world and then asking where that world came from, he is placing himself within history. Virtual participation gives him the psychological processes linking Calvinist predestination to the spirit of capitalism, which he then locates within a broad array of historical forces, including the rise of a legal order, systematic accounting,

and wage labor. Throughout he is engaging with and building upon materialist theories of the origins of capitalism. Of course, historians are usually less self-conscious in their methodological precepts, and their work cannot be so easily divided into one or other model of science. The purpose here, however, is to open up the imagination to different ways of doing social science rather than abandoning science altogether when the 4R's seem out of reach.

Industrial and Craft Modes of Science

Having established two models of science, we must now ask what the criteria are for each model that distinguish between "good" and "bad" science—science well executed and science badly executed. The regulatory principles of positive science—reactivity, reliability, replicability, and representativeness—define a procedural objectivity, a process of gathering knowledge. We can call it an industrial mode in which process guarantees the product. Conception is separated from execution, and engineers define each task in the division of labor so as to assure the quality of the final product. In the corresponding view of science, theory is separated from research practice so that the latter can be carried out according to predefined procedures. The prototype of the industrial mode is survey research where different tasks are parceled out in a detailed division of labor—the researcher, the designer, the interviewer, the respondent—ordered by a bureaucratic structure. The interviewer and the respondent are subordinated to the schedule, constructed by the researcher. The purpose is to obtain an accurate mapping of the world by delineating the procedures for gathering knowledge.

The regulatory principles of reflexive science—intervention, process, structuration, and reconstruction—rely on an embedded objectivity, "dwelling-in" theory. Here we have a craft mode of knowledge production in which the product governs the process. The goal of research is not directed at establishing a definitive "truth" about an external world but at the continual improvement of existing theory. Theory and research are inextricable. The extended case method is thus a form of craft production of knowledge wherein the conceiver of research is simultaneously the executor. The individual participant observer carries out all the tasks of the research process in collaboration with his subjects. The research process is not arbitrary, but it cannot be reduced to a set of uniform procedures. The weight of evaluation lies with the product, whether reconstruction pushes theory forward or merely makes it more complex, whether reconstruction leads to more parsimonious theories with greater empirical content, whether reconstruction leads to the discovery of new and surprising facts.

To put it another way, following Weber we can distinguish an objectivity based on formal rationality—what I have called procedural objectivity—from one based on substantive rationality—or what I have called embedded objectivity. We can even go so far as to say that underlying our two models of science are two different theories of action—instrumental action on the one side and communicative action on the other.

The coexistence of two models of science with their own regulative principles—their own notions of what is a good and bad science, that is, their own notions of objectivity—has profound consequences for evaluating any given piece of research. It means that we should be careful not to level positive criticisms at reflexive methods or reflexive criticisms at positive methods. It is as inappropriate to demand that the extended case method follow the 4R's as it is to impose intervention, process, structuration, and reconstruction on survey research. One cannot dismiss the extended case method because the practitioner alters the world she studies, because her data are idiosyncratic, because she extends out from the local to the extralocal, or because she only has a single case. The extended case method simply dances to another tune. Listen to the tune before evaluating the dance.

A Tale of Two Handmaidens

The coexistence of two models of science has important repercussions for the way we think of methodology. Because, conventionally, there is only one model of science, and it, moreover, usually remains invisible, method and technique are rolled into one.[25] In this monocratic scheme methodological thinking concentrates on the relative virtues of techniques. Some (for example, Sieber 1973) are ecumenical and argue that one chooses the technique or combination of techniques appropriate for the problem being investigated. Others claim that some techniques are superior to others. Thus, in the heyday of the Chicago School, participant observation of the detached, male, professional sociologist reigned over social surveys, sullied by their association with muckraking women reformers (see Bulmer 1984; Fitzpatrick 1990; Deegan 1988; and Gordon 1992). Only later, as quantitative sociology asserted itself, did survey research come to be regarded as more objective and scientific than methods based on participant observation. In the struggle for disciplinary hegemony each technique tried to demonstrate its own superiority by calling attention to the biases of the other. The elaboration of a binary view of science, however, turns the debate away from techniques and toward the explication of methods, tied to alternative models of science.

With only one model of science, techniques may vie for a place in the sun. With two models of science, any given method may be accompanied by a second method as its subordinate complement. Survey research suffers from context effects that can best be studied and minimized with reflexive methods. To minimize interview, respondent, field, and situation effects, survey researchers use clinical or extended case methods. Reflexive methods become the handmaiden of positive methods.[26] Can positive methods also be the handmaidens of reflexive science? Here too the answer would seem to be affirmative. The extended case method embeds social processes in the wider array of social forces. The latter are constituted as external to the observer and therefore can be studied with positive methods. Max Weber, after all, depended on the empirical generalizations he developed in *Economy and Society* in order to undertake the extended case analysis of the rise of capitalism in *The Protestant Ethic and the Spirit of Capitalism.* In extending out from the processes of Zambianization, I made use of surveys that portrayed miners as a social force bent on protecting their privileged status as an aristocracy of labor. Just as reflexive methods can serve survey research, so positive methods can serve the extended case method.

Impediments to Science: From Context to Power

It might be argued that the choice between positive and reflexive methods turns on the problem being studied—positive methods

are more appropriate to the study of enduring systemic properties, while reflexive methods are better attuned to studying everyday social interaction; positive methods are better deployed for the objective world and reflexive methods for the subjective world. Such an instrumental view of method misses deep differences between the two conceptions of science that orient us to the world we study—to stand aside or to intervene, to seek detachment or to enter into dialogue. Usually, it is not the problem that determines the method but the method that shapes the problem. Our commitment to one or the other model of science, it turns out, endures across the problems we choose to investigate.

We should ask, then, whether there are broad factors predisposing one to adopt one or the other model of science. Can we turn the extended case method on itself and locate each model historically? As I have shown, the challenge for positive methods is to minimize or control for context. Survey research becomes the less problematic the more interviews are stimuli unaffected by the character of the interviewer, the more respondents interpret questions in identical ways, the more external conditions remain fixed, and the more situations do not produce different knowledges. Survey research most closely approximates positive goals when the specifics of situations and localities are destroyed. It works best in a reified world that homogenizes all experience, when—to use Jürgen Habermas's vocabulary—the system colonizes the lifeworld (Habermas 1984, 1987). Positive science realizes itself when we are powerless to resist wider systems of economy and polity. Some analyses of the information society, postmodernity, and space-time distanciation do indeed suggest that we are moving toward a contextless world made for the social survey.

Reflexive science, on the other hand, takes context and situation as its points of departure. It thrives on context and seeks to reduce the effects of power—domination, silencing, objectification, and normalization. Reflexive science realizes itself with the elimination of power effects, with the emancipation of the life-world. Even as that utopian point may be receding, the extended case method measures the distance to be traveled. In highlighting the ethnographic worlds of the local, it challenges the postulated omnipotence of the global, whether it be international capital, neoliberal politics, space of flows, or mass culture. Reflexive science valorizes context, challenges reification, and thereby establishes the limits of positive methods.

TWO

The Ethnographic Revisit

Capitalism in Transition and Other Histories

Tacking back and forth through forty years of fieldwork, Clifford Geertz (1995) describes how changes in the two towns he studied, Pare in Indonesia and Sefrou in Morocco, cannot be separated from their nation-states—the one beleaguered by a succession of political contestations and the other the product of dissolving structures. These two states, in turn, cannot be separated from competing and transmogrifying world hegemonies that entangle anthropologists as well as their subjects. Just as Geertz's field sites have been reconfigured, so has the discipline of anthropology. After decades of expansion, starting in the 1950s, many more anthropologists now are swarming the globe. They come not only from Western centers but also from former colonies. Anthropologists are ever more skeptical of positive science and embrace the interpretive turn, itself pioneered by Geertz, that gives pride of place to culture as narrative and text. "When everything changes, from the small and immediate to the vast and abstract—the object of study, the world immediately