

# 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 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 (M. Polanyi 1958). 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.”<sup>1</sup> 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.<sup>2</sup> 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; V. 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.<sup>3</sup>

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 recon- densing 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 re nascent 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 T. 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





























































































