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Epilogue

On Public Ethnography

As social scientists we are part of the world we study. Typically, we insulate ourselves from the dilemmas this creates. We barricade ourselves in the ivory tower, relying on data gathered by others, accessing the empirical world at a distance, burying ourselves in archives, or even corralling our subjects into laboratories. As participant observers we cast these protections aside and plunge into the world beyond, which forces us to think more deeply about our relations to that world—relations that both are specific to the immediacy of the communities we study and extend to our responsibilities and obligations as social scientists more generally, independent of the techniques we use. As an extreme form of research, participant observation helps us think about the tensions between accountability to the world we study and obligations to the academic community.

Caught between the world of the observer and the world of the participant, the ethnographer faces a host of practical problems. In gaining entry to a site ethnographers have to justify

hidden abode of production and working-class communities, I too easily adopted their view of the state, either exaggerating or underestimating its power. I was taken in by the spontaneous common sense of production and paid too little attention to social theory. The state and the ruling class are not the mechanical objects but strategic actors, well versed in exploiting crises, absorbing challenges, and, when necessary, taking offensives against subordinate classes. In each of my cases of great transformation I would have done well to have adopted and adapted Gramsci's concept of passive revolution—a molecular transformation orchestrated from above to absorb challenges from below.

Even as I underestimated the strategic sense of the state and ruling classes, I also exaggerated the power of these macroforces. With the exception of Trotsky's analysis of the Russian Revolution and its aftermath, the ethnographies that I describe here are all concerned with changes in the microcontext, occurring as a result of changes in the macrocontext, that is, the macrofoundations of microprocesses. There is a presumption that the logic of history is given at the macrolevel, that there is a rationality to that history, and that history is on our side. If only we understood those laws of social change, we would be able to assure ourselves a better world. There are no such guarantees, and history never escapes its dark side, the result of intended and unintended consequences of deliberate action. Under third-wave marketization the early gains of postcolonialism, organized capitalism, and even post-Stalin state socialism are reversed. We would do well to dispense with laws of history, whether Marxian or liberal, and leave behind the associated ruptural or catastrophic theories of social transformation.

So what, then, are the implications of this analysis for the ethnography of social transformation? Given that twentieth-century attempts to achieve freedom, justice, equality, and democracy were blocked and even reversed, how should we approach their realization in the twenty-first century? If we cannot rely on laws of history to deliver a better society, and if ruptural revolutions endanger the values we treasure, we have to foster prefigurative institutions that instantiate desirable values. The ethnographer now assumes the special role of social archeologist.

Instead of looking at the field site as a way to understand the laws of history, we look upon it as the foundation of an alternative order. Therefore we don't look for typical institutions but for those odd institutions such as cooperatives or experiments in democratic governance that harbor greater freedom and security. The ethnographer as archeologist concentrates on digging up the subaltern, understood as alternative institutions, and examines their internal contradictions, their dynamics, and their conditions of existence. In seeking out such concrete experiments—real utopias, as Erik Wright calls them—we keep alive the ideals of social and political justice that are in danger of being lost or neutered in capitalist accumulation.¹

In this context ethnographic extension can move in two directions. On the one hand we can extrapolate small-scale institutional innovation to the national or even global level. If participatory budgeting can occur in one city, say, Porto Alegre, how can we think of this at a national or even global level? What are the implications and possibilities of extending its scale? On the other hand, we can think of extension as joining together different microexperiments in an emerging transnational movement.

Can cooperatives, for example, be linked together to form a global cooperative movement, and, if so, what entity might perform this bridging task? In both tasks of extension—extrapolation and bridging of real utopias—intellectuals have an analytical task, diagnosing what is possible, but they also have an ideological function, galvanizing the critical imagination—simultaneously diagnosing the limits of capitalism and sustaining the idea that another world is possible.

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Caught between the world of the observer and the world of the participant, the ethnographer faces a host of practical problems. In gaining entry to a site ethnographers have to justify

their intense surveillance, their prying into the lives of others. Not surprisingly, communities often put up barriers to the outsider, causing us to create elaborate justifications for our presence. Gatekeepers effectively shield their sites from intruders, so that sometimes entry can be accomplished only through covert means, as in the case of my *Zambian study*, or through elaborate negotiation with "authorities," as in my *Hungarian studies*. The rich and the powerful have more to hide and do not readily consent to our scrutiny. It is easier to study the poor and weak, who are defenseless against our encroachments in their space and time. There is a deep bias in the human subjects protocols.

Not just in entry but in day-to-day involvement, our presence is continually being questioned, both by those with whom we interact and by us, because we are aware that our business is symbolic violence, exploiting the goodwill of others for our own ends. We can assuage our guilt by developing a gift economy, bribing our subjects, offering advice, but also by representing the community to the outer world. Here we are entering dangerous territory since communities are rarely unified, so which faction should we represent—workers or managers? Brahmins or Dalits? Teachers or students? In the end, to whom are we accountable? We can easily lose our moral compass and, like the military anthropologist who helps stabilize foreign occupation, become a hired expert for dubious causes.

These questions are no less salient once we leave our site, and leaving often proves as problematic as gaining entry. Having established relations, it is difficult to cut them off just because the study is technically complete. We might say there are two types of ethnographers, those who return to their communities and those who don't, those who establish an enduring human

connection and those who negotiate a more instrumental relation. There are, of course, also those who create such turmoil in their sites that they are expelled, never to be allowed to return.

There are no simple answers to such ethical and existential dilemmas that the participant observer confronts in the field. The participant observer cannot escape the contradictory poles of participation and observation. Whereas texts on participant observation see these dilemmas in narrow individualistic terms, as problems faced and resolved by the individual ethnographer, I want to embed these dilemmas in two broad questions—questions connected to the fourth element of the extended case method, that is, in the extension, elaboration, reconstruction of theory. From the side of observer we must ask whose theory we refute or reconstruct, and from the side of the participant we must ask which audience we address with our reconstructed theory.

The first question, then, is, whose theory do we reconstruct? I take the view that everyone is a theorist in the sense that some coherent account of the world is necessary to live in community with others. We all have tacit theories of how the world works, leading us to anticipate the behavior of others. We stop at a red light because a watching police officer might give us a ticket, or because we don't want to get hit by a car coming the other way, or because we have learned to do this ever since we could walk. Contained in that very act is a theory or range of theories of how the world functions, a theory that remains tacit and unexamined. As ethnomethodologists have taught us, such nondiscursive, taken-for-granted theories are exposed when our anticipations are violated, when, for example, a driver runs through a red light. Such tacit theories may be short on mechanisms, indeed may be

shorthand for inaccessible processes, processes that are the concern of the explanatory theory of social science.

Therefore we can distinguish two types of theory. On the one hand there is the theory of the people we study, namely, folk theory, buried in common sense and sometimes elaborated into ideology. On the other hand, there is the theory of the philosophers and social scientists, that is to say, of intellectuals, what I call analytical theory, which we can also call science. I assume that folk theory, while it has to contain some truth, a practical truth, is not as adequate as the truth of analytical theory, scientific truth. This is an act of faith, perhaps, but also the *raison d'être* of our scholarly existence. As sociologists, therefore, we may think of ourselves as breaking with or elaborating folk theory, but in either case we are moving from folk theory to analytical theory.

Just as academics too often take for granted that the object of transformation is our own analytical theory rather than the folk theory of our subjects, so equally we cannot assume that the people we address are also social scientists. The second question, therefore, is, to whom is our theory addressed? Are we addressing academic audiences—our home community of scholars, specialists in the production of theory—or are we focused on lay audiences? In principle one can be doing both simultaneously, but these discursive communities tend to be distinct, calling for different strategies of engagement. Within each category there is a broader and a narrower audience. Within the academic world one can address a narrow community of specialists or a broader community of scientists, just as beyond the academy we may be focused on the people we studied or on wider publics.

Table 8. *Public and Academic Ethnography*

	Academic Audience	Extra-academic Audience
Analytical theory (science)	Professional	Prophet
Folk theory (ideology)	Critic	Interpreter

Answers to the two questions—theory from whom? and theory to whom?—are independent of each other, which means we can draw up a two-by-two table (table 8). The true public ethnographer, what I have called the interpreter, uses his or her science to elaborate and transform the folk theory of the participants but makes the result accessible and relevant to publics. Here lies Nancy Scheper-Hughes's (1992) report on the desperate circumstances of mothers in the favelas of Brazil, Margaret Mead's (1928) interpretation of the sexual practices of adolescents in Samoa, or Diane Vaughan's (2006) revelations about the organizational biases of NASA. The professional ethnographer does the opposite. Here the object of our social science is to transform, build, and improve academic theory. Examples abound, but they would include the classic anthropologists of kinship such as Radcliffe-Brown or Evans-Pritchard. Within the world of sociology professional ethnographers often also become interpreters—think of Kathryn Edin and Laura Lein's (1997) work on welfare mothers, Mitchell Duneier's (1999) work on street vendors, Elliot Liebow's (1967) work on unemployed men, Arlie Hochschild's (1989) work on the domestic division of labor, or William Foot Whyte's (1943) work on gangs.

Professional ethnographers become prophets when they seek to transmit their analytical theory to wider audiences, turning

social science insight into the fate of humanity. We think here of the anthropologists Clifford Geertz, Max Gluckman, or Edmund Leach or the sociologist Robert Bellah, when they took on the role of public educator. Finally, we have the critics, who take folk theory as the object of elaboration but deploy it against existing analytical theory held by academics. The critic tries to demonstrate that not just lay knowledge but social science itself is in the grip of folk theory. One thinks of Karl Marx's analysis of commodity fetishism, a generalized lived experience of the market that holds in thrall not just workers but academics, too. Equally, Pierre Bourdieu shows how the mystification of domination through distinction affects social science as well as everyday life.

Of the four studies that I examined in this book, the study of Zambianization is the only truly public ethnography, where I played the interpreter. The study was constructed as an engagement with the government's Zambianization report, which declared all was well in the copper mines—Africans were replacing expatriates. But these figures hid from view the organizational manipulations that maintained the color bar. I drew on and reconstructed the sociological theories of Alvin Gouldner and Frantz Fanon to understand the social forces working to uphold the racial order, but *The Colour of Class on the Copper Mines* (1972a) was written for a public audience. In addition to being accessible, it touched different interests, as demonstrated by the initial opposition of the mining executives and the enthusiasm of the government's chief of Zambianization. Indeed, when it did appear, it attracted considerable media attention.

Yet in the end, the mining executives used the report to discipline their own mine managers, instructing them to get their

Zambianization house in order. The effect of the report was thereby neutralized. Here lies the problem with "traditional" public ethnography in which the scientist broadcasts her or his discoveries and interpretations but has no control over their deployment on behalf of the powerful. If I had worked more closely with grassroots organizations, and followed Dorothy Smith's (2007) institutional ethnography or participatory action research, my broad criticism of Zambian economic development might have been sustained. Still, for all its shortcomings, this was a case of public ethnography, taking government ideology as its point of departure, discovering the social processes behind it, and bringing those findings into the public realm.

Manufacturing Consent (Burawoy 1979) took the diametrically opposite approach. I adopted an instrumental approach to the field site. As a graduate student I was preoccupied with the academic world, bent on a critique of sociology and developing Marxism based on my experiences on the shop floor. I was a disciple of structuralist Marxism and saw science as simultaneously a rupture with and explanation of common sense. I was intrigued by my coworkers, who labored so hard yet at the same time denied they were doing so. If industrial sociology had conventionally focused on restriction of output, why workers don't work hard, I inverted the question—why do they work as hard as they do? If traditional industrial sociology didn't ask the question, Marxism assumed the answer lay with coercion and material incentives. Marxists missed the organization of consent on the shop floor, thinking this took place only in political and civil society.

I never thought of convincing my coworkers that they were working hard or that Marxism bore any relation to their lives.

Marxist structuralism, a product of the French *Grandes Écoles* and a reaction against the intellectual sterility of the French Communist Party, became the rationale for devoting myself to the transformation of social science. I was not alone in this endeavor but part of a 1960s generation, in the United States and elsewhere, that considered the immediate task was to bring sociology out of the dark ages or to simply replace it with Marxism. Following Alvin Gouldner's *The Coming Crisis of Western Sociology* (1970), we claimed that the sociology we inherited from the 1950s and 1960s was out of sync with the mobilization of society, manifested, especially, in third world movements both within the United States and elsewhere. Academic Marxism was a project of intellectuals that made little effort to directly address the working class. Somehow scientific truth would benefit all. In this regard it was different from much of second-wave feminism and the new interdisciplinary programs (African American studies, Chicano studies, Native American Studies) whose *raison d'être* lay in publicizing and addressing the plight of marginalized communities.

Marxism has not always been cut off from the subjects of its analysis. Leon Trotsky is a case in point. He would find academic Marxism an oxymoron or, more likely, a petty bourgeois deviation. Immersed in the Marxist debates of the time, Trotsky was, at the same time, always determined to address the broadest possible audience. He was a spectacular orator and, of course, a virtuoso organizer, most notably of the Red Army during the civil war. Marxism's claim to knot theory and practice together for social transformation made it an appealing philosophy. Trotsky was unusual among the classical Marxists in that his writings were indeed accessible to all. But this did not mean

they were any less theoretical. Wrestling with the specificity of the Russian experience, immersed in a project of revolutionary change, he gave an original twist to Marxism by recognizing the rhythm of world capitalist development and its implications for national transformations.

As Isaac Deutscher portrays him in his majestic three-volume biography (1954, 1959, 1963), Trotsky was indeed a rare prophet. His magnum opus—*The History of the Russian Revolution* ([1933] 1977)—is shaped by his theoretically inspired participation in those events, allowing him to see macrotransformations in the microprocess. He was committed to an emancipatory socialism and recognized this could be accomplished in the Soviet Union only if it triggered revolutions elsewhere. There could be no socialism in one country. His perspective was no innocent academic conclusion but the essence of his ideological and political difference with Stalin. For Trotsky the fate of the revolution rested on the adoption of strategies informed by theory, one might say a naive belief in the correctness of a refashioned Marxism. His arguments still rested on the illusory belief that the Western working class was ripe for revolution—an assumption he never abandoned but that other Marxists—Gramsci in particular—would call into question.

This brings me to the last stance—the critic who starts with folk theory but aims its interrogation and demystification at other academics. Many reasons combined to drive me to Eastern Europe, but the theoretical impulse came from my own claims that a capitalist working class could not arrive at revolutionary self-understanding. Was this true of all working classes or only the capitalist working class? Solidarity suggested that the socialist working class might have such a revolutionary potential.

Furthermore, whether the West's working class was revolutionary or not, it was surely incumbent on Western Marxists to understand the character of actually existing socialism and not simply dismiss it as a statism, irrelevant to the Marxist project. It was simply too easy to harp on the pathologies of capitalism and assume that they would be rectified in a socialist heaven, letting socialism on Earth off the hook. So I ventured to Eastern Europe in the hope of finding a working class with socialist aspirations. Sure enough, I found traces of socialism, nurtured in the womb of production and expressed as a critique of the party-state for failing to live up to its promises—failings to which it drew attention through its political and ideological practices of self-justification.

In Hungary, as in Chicago, I was not keen to display my Marxism since for critical intellectuals it was a bankrupt ideology and for workers it announced itself as betrayal. There was a moment of genuine optimism in 1989 when I thought that the collapse of state socialism prefigured the rise of a democratic socialism, as I wrote in an article titled "Marxism Is Dead: Long Live Marxism!" (Burawoy 1990a). I still believed this as late as 1990, when I lectured on the contradictions and paradoxes of state socialism to South African audiences. The South African Communist Party had just begun a painful interrogation of its past and launched a debate about the possibilities of socialism, a debate that became dying embers once the African National Congress assumed power. My own hopes for socialist renewal took a beating in Russia's catastrophic transition to capitalism. I took up cudgels against the new reigning ideology of market fundamentalism, watching powerlessly as unbridled commodification devoured the productive forces, leaving large swaths of the population in desperate poverty and degradation. I had no

audience in Russia, so I became a Marxist critic at home, a critic of market ideology within social science, showing how untamed markets led to disaccumulation, or what I called the great involution. Yet it was becoming difficult enough to defend Marxism within the academy, let alone outside.

For twenty years—from 1982 to 2002—I had taken a detour through socialism. It was time to return to capitalism, perhaps a little wiser but no less Marxist. Marxism after communism would have to finally jettison its laws of history—history as the succession of modes of production, history as the rise and fall of any given mode of production, history as the history of class struggle. The transition to socialism can no longer be understood as rupture with capitalism but instead as the emergence and stringing together of smaller-scale alternatives, what Gramsci called a war of position. Marxist social science has now to base itself in the trenches of society, seeking out embryonic institutions, real utopias that might challenge capitalism, keeping alive the very idea of alternatives. Marxism thereby necessarily becomes public ethnography in so far as it enters into a dialogue and collaboration with the organizers of real utopias. Marxists forsake their grand theories of history to become interpreters and transmitters of the conditions of possibility of alternatives to capitalism, alternatives struggling for survival in the interstices of society.

The collapse of communism may not spell the end of socialism, but it has certainly intensified third-wave marketization around the globe. As third-wave marketization erodes civil society, it threatens all institutions outside market and state, the foundations of real utopias. The very idea of the social is in abeyance, threatening the existence of the *social* sciences, including human

geography, anthropology as well as sociology, but not the economic sciences or political sciences that have become ever more asocial or antisocial. In the postcommunist era Marxism and sociology become collaborators in the defense of the social. Indeed, we can go further and declare that sociology itself is fast becoming a real utopia, providing a concrete imagination for an alternative world incompatible with capitalism. As such we need a reflexive ethnography to propagate the sociological imagination, the prophetic glue that can bind real utopias together while holding at bay the destructive forces of market and state. Sociology, if it is to survive, may have no alternative but to go public.

NOTES

INTRODUCTION

1. Outside sociology Victor Turner and Raymond Smith knew all about the Manchester School, and, just as I was leaving the University of Chicago, John Comaroff and Jean Comaroff arrived.

CHAPTER ONE

Acknowledgments: This chapter was twenty years in the making. Earlier versions are unpublished and barely recognizable. Two people in particular helped me bring this endeavor to a close. Erik Wright plied me with dozens of pages of intense argumentation to the effect that there can be only one model of science, while Peter Evans insisted that I persist despite all opposition. And opposition there was plenty, from hostile receptions in talks to dismissive reviews from journal referees. My ideas took shape in heated courses on participant observation and while working with Berkeley graduate students on two books, *Ethnography Unbound* and *Global Ethnography*. Teresa Gowan, Leslie Salzinger, Maren Klawiter, and Amy Schalet were intent on