# 11

# Advancing a Research Program

When I arrived in Berkeley, faculty and graduate students alike were puzzled by what seemed to be a combination of opposites: ethnography and Marxism. After all, they said, ethnography, or *participant observation*, concerned itself with micro-processes, social interaction in bounded situations, whereas Marxism concerned itself with macro-processes, large-scale historical transformations. They were irreconcilably opposed. My task, then, was to show just how micro and macro could be joined to each other, how they necessarily feed into each other.

There were reasons for their skepticism. In those days the conventional wisdom about ethnography, at least within sociology, was to be found in Barney Glaser and Anselm Strauss's *The Discovery of Grounded Theory* (1967) – an inductive view of social science that built up theory through constant comparison of observations. It was Chicago sociology's response to the ascendancy of grand theory that sprung from the head of Talcott Parsons rather than from the concrete experiences of real people in social interaction. Ethnography was, therefore, limited to micro-processes, paradigmatically represented by the "dramaturgy" of Erving Goffman or the early Chicago urban studies. The "external" context was bracketed as being beyond the focus of study or simply possessing no meaning.

Coming from Zambia, where the Manchester School had refused the insulation of the field site and pioneered the extended case method (van Velsen 1967), this didn't make sense – the wider context was composed of forces that were shaping face-to-face social interaction. The very meaning of sociology is bound up with linking the micro to the macro, recognizing that the micro is shaped by conditions beyond itself. There were those, such as the distinguished sociologist James Coleman, who, leaning on economics and rational choice theory, pursued the micro-foundations of a macro-sociology. The extended case method, by contrast, called for the study of the macro-foundations of micro-sociology. However, to explore that context ethnographic research required a conception of social science very different from the one that supported grounded theory.

I needed to be schooled in the philosophy of science. Here I was fortunate to learn from Tom Long, an extraordinary graduate student in sociology. Even when he was an undergraduate in Berkeley's philosophy department I attended the summer courses on "critical theory" that he voluntarily organized and led. As part of *his* qualifying examinations, he taught *me* the rudiments of the philosophy of science, the move from the conventional positivist view based on induction, that dominates sociology, to the historical view that pays attention to how science actually works. What I learned from Tom became the basis of an introductory course on methodology required of first-year graduate students. Rather than a rundown of the standard techniques used in sociological research – surveys, participant observation, experimental methods, archival work – my version turned on the question of whether sociology was a science. Drawing on examples of social research, I outlined a sequence of distinct perspectives on the meaning of science: John ~~Stuart Mill's induction (1888)~~, Karl Popper's (1963) falsificationism, Paul Feyerabend's (1975) anarchism, Michael Polanyi's (1958) personal knowledge, Thomas Kuhn's

(1962) scientific revolutions, and Imre Lakatos's (1978) scientific research programs. In the second half of the course we examined the critiques of sociology as a science, showing how they, too, usually assumed a limited and outdated positivist view of science.

What did this mean for conducting ethnography? Against the discovery of grounded theory, in which theory springs spontaneously from data, the post-positivist theories of science – Kuhn and Lakatos in particular – tell us that one cannot interpret the empirical without some sort of lens, some sort of prior theory that brings order to our observations, allowing us to make sense of what is an infinite manifold. But, as it shines a light on the empirical world, so theory also reveals its own shortcomings, generating expectations that turn out to be false – what we call *anomalies*. Faced with such an empirical challenge, we can either reject the theory or we can hold on to the theory by reconstructing it, maintaining its basic assumptions, but revising it by introducing new "auxiliary hypotheses." In *Manufacturing Consent,* I held on to Marxist assumptions about exploitation but reconstructed the theory of how it works – not through coercion alone but through consent backed up by coercion. Marxist theory also pointed me to the external forces shaping the dynamics on the shop floor. Specifically, markets and states as mediated by the industrial enterprise set the limits – changing limits – on class relations on the shop floor.

I developed this view of the extended case method through teaching a graduate practicum in participant observation. Students were thrown into a field of their choice and had to report on their observations in seminars that met twice a week. They submitted their field notes to me and their classmates, showing how they were grappling with a sociological literature that posed a set of questions to their fieldsite. As they engaged with the people they studied, they simultaneously developed a dialogue between theory and data that ended not in the discovery of theory but its reconstruction.

On two occasions, student papers became the basis of a book. The first, *Ethnography Unbound* (1991), was a collective project organized around studies in the Bay Area, focusing on social movements, education, work, and immigration. This became the occasion for advancing the idea of the extended case method with four components. The first component was to *extend the observer to the participant* – the observer would join participants in their time and space. The idea was not to pretend to be a fly on the wall, but to actually partake in the lives of those they studied. By itself this created multiple dilemmas, especially when the site involved antagonistic actors. Rarely was there a simple solution to these dilemmas, but discussing them collectively made us acutely aware of the challenges in being part of the world we studied.

The second component was to *extend observations over time and space*. Ethnography is not a one-shot event, but a succession of visits that could stretch over months or even years, often requiring the ethnographer to follow their subjects to different places. The idea here is to study the unfolding of social processes, as I did when I followed Zambianization as forced succession within an organization, or the dynamics of the shop floor at Allis. If these first two components are quite typical of participant observation, the third and fourth components are not.

The third component was *the extension of theory*. The extended case method takes the view that theory, understood as a parsimonious summary of the state of collective knowledge in a particular area, is the sine qua non for scientific advances – the extension of theory through the discovery of anomalies. If you start with theory, then a single case can advance that theory – reconstructing it to absorb the anomaly. Grounded theory, by contrast, is not grounded in theory but in the empirical world from which it *induces* empirical regularities, *seemingly* independent of the knowledge accumulated by the scientific community. Grounded theory is actually impossible. There is no way to see the world without a lens, without a cognitive map.

That being the case it's best not to strive for the impossible, but to start from a different premise – the priority of theory.

The extension of theory makes possible a final, fourth extension, the *extension from micro to the macro*, from social interaction to the forces shaping that interaction. Here it is necessary to work with social theory that contains an understanding of the relationship between micro and macro. Grounded theory, resting as it does on induction, cannot go beyond the observations made in the ethnographic field site. Grounded theory may have served its purpose in contesting grand theory, but it has no justification as a scientific method – although it appeals to the empiricist proclivities of US sociology. Sadly, grounded theory leaves theory to the theorist, perpetuating the division it was designed to dissolve.

*Ethnography Unbound* exemplified the extended case method with ten projects, embedded in divergent theoretical perspectives. As a second collaborative enterprise, *Global Ethnography* (2000) aimed to extend the extended case method to the global arena. I had been made chair of my department, thereby temporarily putting an end to my ethnographic projects. I proposed to the students whose doctoral research I was supervising at the time that we write a book together. They were a brilliant and disparate group, studying different phenomena in different parts of the world. Our task was to forge their studies into a common perspective on globalization. We started, therefore, as a reading group, tackling the most notable theories of globalization associated with such figures as Immanuel Wallerstein, Stuart Hall, Saskia Sassen, David Harvey, Nancy Fraser, Manuel Castells, Anthony Giddens, James Clifford, Arjun Appadurai, Fredric Jameson, and Janet Abu-Lughod. Taking up the loose framing of Stuart Hall, we came up with three approaches to globalization: extranational forces shaping lived experience within nations, transnational connections binding people across national boundaries, and postnational imaginations that informed an emergent global social consciousness. We could ground lofty theories in lived experience but we had greater difficulty working from the lived experience up to the global.

Alongside these ethnography seminars I was developing an alternative research program that extended the theory advanced in *Manufacturing Consent*. I have already pointed to the way I examined changes in production regimes, comparing my own observations and experiences with those of Donald Roy thirty years earlier that led to the contrast between hegemonic and despotic regimes of production. Another serendipitous breakthrough came with the discovery of Miklós Haraszti's riveting book, *A Worker in a Worker's State* (1977). As a political dissident Haraszti had been consigned to work as a machine operator in the Red Star Tractor Factory. He took revenge on his "jailers" with a lurid sociography of life in the socialist factory. As luck would have it, Red Star's machine shop was similar to the one at Allis-Chalmers, with its array of drills, mills, and lathes. But with one striking difference: he worked twice as hard as we did, running two machines at once. This was a dizzying pace, defying the stereotype that workers under state socialism had retained only one right – the right not to work hard. Here was another anomaly, an intriguing puzzle to be explored.

Haraszti's goal was to represent Red Star Tractor Factory as the typical socialist workplace, marked by a despotism driven by piece rates. He did not investigate whether it was, indeed, a typical socialist workplace. That would have entailed recognition of the particular context – time and place – of Red Star as well as his own peripheral vision from within the workplace. It turned out, on further exploration, that Red Star was one of the early factories to be subject to Hungary's New Economic Mechanism of the 1970s that brought market forces to bear on state enterprises. It involved speed-ups and tightening worker discipline. Still, there was always a latent despotism in the state socialist workplace, governed as it was by the collaboration of party, union, and management, each an extension of the state. I called this *bureaucratic despotism* in contrast to the hegemonic regime at Allis, where management had been constrained by the collective contract negotiated with the union and, more broadly, by state regulation of labor relations. In advanced capitalism the state regulates at a distance; it does not have an institutional presence on the shop floor.

One theoretical-conceptual advance immediately called forth another: to distinguish between the bureaucratic despotism at Red Star and the market despotism that Marx had described for nineteenth-century England. I was therefore led to accounts of the nineteenth-century workplace and discovered different despotic regimes – patriarchal and paternalistic. Examining historical accounts from other countries, I showed how the nineteenth-century textile industry exhibited different regimes in Russia and the US, as compared to England. I had to distinguish all of these from the despotic regimes of colonialism – a form of racial despotism – and here I delved into the transitions taking place in the Zambian copper mines, based on my fieldwork there. In every case I not only tried to show how the combination of states and markets created distinctive despotic regimes but also to examine the consequences those regimes had for class formation and the organization of class struggle.

Having shown that states and markets shaped despotic regimes of production, I then had to demonstrate how they shaped different hegemonic regimes under advanced capitalism. The hegemonic regime at Allis-Chalmers exhibited features that were distinctive to the US, as I learned when I began comparing the US with Sweden, Japan, and the UK. Based on studies of factories in these countries, I argued that two factors were crucial: on the one hand, the support states gave to workers when they lost their jobs, and on the other hand, the extent to which the state regulated the relations between capital and labor.

I slowly built up a research program for production regimes, or what I called the *politics of production* (Burawoy 1985), by drawing on secondary accounts that ranged across advanced capitalism, state socialism, and the Global South. But my experiences at Allis-Chalmers had inspired this reconstruction of the Marxist theory of politics and production.

Throughout my time at Berkeley I have had the privilege to work with exceptional students who would push the idea of production politics in different directions by identifying different dimensions of production regimes and how they vary with the labor process, by discovering how regimes differ by economic sector and by national context, and by looking at them from the standpoint of their effects as well as their causes, especially their contribution to working-class mobilization. Let me illustrate these developments with a few of these studies.

Much of the research drew attention to the gendering of production regimes. Ruth Milkman's *Gender at Work* (1987) examines the politics of the gender division of labor in the US electronics and auto industries before, during, and after World War II. She discovers that the distinction between men's work and women's work is rarely contested, but the line between the two moves as a function of the type of industry and managerial interests, rather than because of pressure brought to bear by trade unions or the interests of male workers. Linda Blum's *Between Feminism and Labor* (1991) continues the study of the gender division of labor, comparing the politics of comparable worth that elevates the value of women's work with the politics of affirmative action that promotes women into men's jobs.

Ching Kwan Lee's *Gender and the South China Miracle* (1998) compared production regimes of electronics plants in Hong Kong and Shenzhen: in one there was "familial hegemony" and in the other single women are subjected to "localistic despotism." Ching Kwan attributed their divergence to the wider political economy. Leslie Salzinger's *Genders in Production* (2003) pushed the gendering of production regimes even further through a comparison of four maquiladoras – assembly plants just south of the US/Mexico border. In each plant, management adopted a particular gender strategy: Panoptimex had a patriarchal order sustained through sexualized surveillance, in Anarchomex conflicts around legitimate masculinity continually disrupted managerial control, in Particimex women were incorporated through autonomy and responsibility, and in Andromex all workers were addressed through a putatively "masculine" rhetoric.

Could the idea of production regime be extended from industry to the service sector, and what consequences would ensue? Rachel Sherman studied two luxury hotels where she worked in multiple jobs. *Class Acts* (2007) shows how each hotel is a complex configuration of games in which workers sustain and even create the class identity of guests. Jeff Sallaz's *The Labor of Luck* (2009) compares the regulation of work in casinos in Nevada and Gauteng (South Africa). Despite a strong labor union and government regulation, the production regime in South Africa assumed a despotic form while in Nevada, where the union was nonexistent and government regulation was weak, the production regime was more hegemonic. This puzzling discovery could only be understood by reference to racialized legacies and the wider political context.

Others examined the production politics of state employment. One place to begin was socialist societies. Linda Fuller studied how management, party, and unions shaped workplace politics in Cuba. *Work and Democracy in Socialist Cuba* (1992) shows how decentralized planning allowed for greater worker participation in decisions that affected their daily lives. Starting from his own experiences organizing in the 1980s, Paul Johnston's *Success While Others Fail* (1994) saw the public sector as favoring the building of solidarity between service workers, such as teachers and nurses, and the community they served, while the private sector was governed by a market logic that allowed far less room for such solidarities.

Building on these ideas, Steve Lopez examines union organizing in nursing homes in Pennsylvania: starting at the level of a single senior home, he proceeds to a city-wide campaign and then state-wide organizing. *Reorganizing the Rust Belt* (2004) uncovers distinctive obstacles to unionization at each level: lived experience of prior union campaigns, bureaucratic hierarchies within the union, and employer offensives that take advantage of a permissive legal order. More than two decades later, after the consolidation of neoliberalism, Josh Seim's vivid portrait of the emergency medical technician paints a very different picture. His *Bandage, Sort, and Hustle* (2020) focuses on how the ambulance labor process, embedded in the local state and caught between the hospital and the police, is deployed to govern poverty on the streets.

As the studies of the labor process gave way to studies of the labor movement, greater attention was paid to the *effects* of production regimes. In *Manufacturing Militance* (1994) Gay Seidman traced the 1980s upsurge of working-class struggles in South Africa and Brazil to their similar place in the global order that gave rise to a particular production politics tied to community social movements. Mona Younis's *Liberation and Democratization* (2000) undertakes a historical comparison of the African National Congress (ANC) in South Africa and the Palestinian Liberation Organization, attributing the relative success of the ANC to South African capital's dependence on the colonized. In South Africa, Black workers had accumulated both organizational capacity and structural power, whereas the Israeli state encouraged the importation of labor from elsewhere, expelling Palestinians from the labor market. Palestinians were oppressed but not exploited – they did not have the leverage of South African workers.

Continuing the interest in mobilization, Jennifer Chun's *Organizing at the Margins* (2009) compares the success of organizing among marginalized workers in the US and South Korea, pointing to the importance of a symbolic politics – public shaming of employers – a politics beyond the workplace. Taking us further afield, Ofer Sharone's *Flawed System/Flawed Self* (2013), conceives of unemployment as the hard work of job search. Comparing Israel and the US, he shows how job search is best conceptualized as a labor process game with different dynamics, so that in the US the unemployed blame themselves but in Israel they blame the system. He traces the divergence to the institutional context: the self-help and human resource industry in the US and the ubiquitous private employment agency in Israel.

This embryonic research program was not planned, it emerged spontaneously. Only now do I indulge in a rational reconstruction of what was a largely anarchic process. Graduate students gravitate to particular faculty for different reasons, which often have nothing to do with a common research interest. Many, if not most, of the dissertations I have supervised are beyond my own area of expertise. When common frameworks and questions did emerge, they were not forced upon students but gradually developed through immersion in six to ten years of graduate school. Early on I established a dissertation seminar that has met ever since, every week or two, at which students present their chapters and papers for discussion. Here students learn to discuss one another's work; they are as influenced by one another as they are by myself. I would sometimes present my own work to the group and in one way or another I, too, was influenced by them. Research programs are not necessarily planned; they can just as easily develop spontaneously and imaginatively under multiple influences. Forcing them into a straitjacket only makes them sterile. I suspect that the authors I've identified here would deny that they are part of a research program, just as my fellow workers at Allis denied they were working hard.

Outsiders are often more aware than insiders of an emerging program, labeling students by the reputation of their supervisor or of their department – a reputation that can be derisive as well as flattering. For many years, and indeed to this day, association with me has often been a liability – students would be identified, often unfairly, with my stances on ethnography, public sociology, or Marxism. It continues to cause me much anxiety, especially when it comes to the job market. Inevitably, research programs attract followers but also a lot of critics, and it is easier to criticize vulnerable graduate students than established professors. As long as a research program is confined to a small group within a single department, it is not a disciplinary threat, but when it appeals to followers, especially graduate students, in other departments, then a lively guerrilla warfare unfolds.

In determining the influence of research programs, departmental ranking can have an outsize effect. Had he not been at Harvard, I doubt whether Talcott Parsons would have been able to establish the dominance of structural functionalism. Even within a department, there can be tension between rival research programs competing for dominance. From being a productive tension, competition can tip over into something quite destructive. With its multiplicity of research programs rather than one single dominant research program, combat within US sociology is perhaps less intense and more institutionalized, channeled into different journals, departments, or sections of the American Sociological Association.

When competition moves to the global level it inevitably favors research programs emerging from countries with the deepest research infrastructure. Research programs emanating from the US can hide their provinciality behind bogus claims to universalism, propped up by status and funding. In recent decades the supremacy of US sociology – and to a lesser extent, European sociology – has galvanized transnational opposition. Such collaborations across the Global South have their own originality, but they too may be limited, to the largest countries and even to cosmopolitan intellectuals within them. Such is the nature of Northern academia that leading Southern opponents of

Western thought may find themselves absorbed, co-opted, and celebrated in the metropolis.

Given the hierarchy of global knowledge production, scholars from the South are often lured away by tempting offers from universities in the North, even as they maintain one foot in their home countries. They become authoritative representatives of perspectives on the South within Northern academia. But there are also many who refuse the temptations of the North, and remain embedded in universities and institutes in the South. They often undertake dangerous projects, putting their own lives at stake, developing research programs in collaboration with oppressed communities, generating new visions of what sociology might be and what sociology can prefigure.