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Michael Burawoy



Annual Review of Sociology

Living Sociology: On Being in the World One Studies

Michael Burawoy

Department of Sociology, University of California, Berkeley, California 94720, USA;
email: burawoy@berkeley.edu

Annu. Rev. Sociol. 2021. 47:9.1–9.24

The *Annual Review of Sociology* is online at
soc.annualreviews.org

<https://doi.org/10.1146/annurev-soc-072320-101856>

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Keywords

scientific research program, ethnography, extended case method, Marxism, Bourdieu, Polanyi, Du Bois

Abstract

Living sociology refers to the life of sociology, seen as a field of competing scientific research programs. The dynamism of each program requires, on the one hand, engaging internal contradictions and external anomalies and, on the other hand, extended dialogue among the programs themselves. Living sociology also refers to the life of sociologists as they participate in the society they study. My understanding of these two dimensions of reflexive science—the scientific and the hermeneutic—developed through the interaction of teaching and research. I trace the way I learned the extended case method in Zambia and reformulated it through collaborations with students at Berkeley, arriving at the idea of the scientific research program. I show how I tried to contribute to the Marxist research program by wrestling with anomalies that sprung from my experiences working in factories in the United States, Hungary, and Russia. Finally, I describe how teaching social theory led me to Marxist conversations with structural functionalism and with the work of Pierre Bourdieu as well as prefiguring an extended conversation between W.E.B. Du Bois and the sociological canon.



INTRODUCTION

As social scientists we think of the university as an elevated platform from which we launch observation and investigation, a protected arena from which we contemplate and write about the external world. It is also a zone of autonomy, designed to transmit inherited wisdom won through sustained scholarship. This academic freedom comes with responsibility to collectively enforce rigorous standards as we go about research and teaching. It requires us to oppose the assault on truth whether from inside or outside the university.

Yet, even as we are increasingly forced to defend our autonomy, it has become ever more difficult to pretend the university is outside the world we study. If anything should convince us of this, it is the coronavirus disease 2019 (COVID-19) that does not recognize geographical or institutional boundaries as it lays bare and amplifies the social inequalities that sociologists have been analyzing for decades, including inequalities embedded in the university. The transition to remote instruction and the degradation of learning that follows turn crevices into chasms: separating students who can study at home from those who have no space or even access to reliable Internet service, distinguishing tenure-track faculty who have security of employment from ever more numerous adjuncts who are on temporary contracts, making it clear to graduate students that their academic careers are becoming more precarious, and deepening the divide between professionals who work from home and essential service workers who endanger their own lives on pain of job loss.

COVID-19 exposes and accelerates the fallout of a half-century of privatization of the public university. Following the postwar expansion of public universities and the turbulent campus politics of the 1960s, public funding declined, first slowly and then more rapidly. Once a sacrosanct public good, university education has increasingly become a private good under siege. The superstructures are those of a profit-centered corporation: an ever-expanding administration that pursues revenues from student tuition, online courses, and private donors, even as it cuts costs by replacing tenure track faculty with contingent lecturers, outsourcing service labor, and indulging in such desperation gambles as public-private partnerships.

The transformation of the university has so far sustained teaching and research but at an escalating cost. The fiscal crisis cascades into a governance crisis as power is expropriated from faculty, a governance crisis becomes an identity crisis as money becomes the arbiter of disciplinary survival, and an identity crisis becomes a legitimacy crisis as the public questions the value of research and education in the ever-more-costly university. Students are paying more for less, not just in the education they receive but also in the credentials they acquire. As passports to stable employment, credentials are being devalued in a precarious labor market. At the same time, for many, debt becomes a lifelong burden.

University administrators throw the ball back into our court, expecting departments to be creative with the resources they command or simply to do more with less. Sociologists invent new courses that appeal to the vocational interests of students. We link up with the professional schools, or chart new collaborations with criminology or the health sciences or public policy. We invent new credentials and expand into online education. Those who can, trade on their brand to draw in international students at home or abroad. These are short-term survival strategies that push the university and its academic units in an instrumental direction.

Faced with these challenges we can make a virtue of necessity and call for an entrepreneurial university that relishes the new opportunities and challenges. Or we can lament the passing of the old, seeking a return to a mythical golden age. In this article I have chosen a third alternative, seizing the opportunity to reconsider the foundations of our discipline by drawing out the implications of living in the world we study and bringing to the fore the inescapable interpenetration and



interaction of the academic and the extra-academic. This point of departure raises two questions: first, how the external world we study enters into and shapes the practice of sociology—that is, the question of reflexive sociology—and second, how the practice of sociology enters and shapes the external world, or the question of public sociology. In this article I focus on how reflexive sociology lays the foundation for public sociology.

I start by describing two axes of reflexive sociology—the scientific and the hermeneutic—that evolved from teaching ethnography, leading me to the idea of the scientific research program that develops through the absorption of anomalies. I show how my successive factory ethnographies in the United States, Hungary, and Russia tackled three abiding puzzles: working class acquiescence in capitalism, the durability of state socialism, and then the latter's collapse. Each of these challenges to the Marxist research program led to its reconstruction. A reflexive sociology also recognizes the coexistence of alternative research programs, each reflecting different standpoints within society. Complementary and competing research programs develop not only by tackling anomalies but through mutual engagement that identifies and clarifies the blind spots of each. In this article, I illustrate that engagement with conversations: first, between Karl Polanyi and Marxism, prompted by the unexpected collapse of communism; second, between Marxism and Pierre Bourdieu as sociology's latest challenge to Marxism; and third, between W.E.B. Du Bois and the anointed founders of sociology, a conversation that leads to the radicalization of sociology and/or a Black Marxist tradition.

In this way I elaborate the meaning of reflexive science through a retrospective reconstruction of my own intellectual biography, a journey that oscillates between teaching and research. Conventionally, teaching and research are regarded as running on separate but unequal tracks. In the research university that dominates the system of higher education, teaching has always played second fiddle to research. Whether as graduate students or young faculty, we are told to minimize time spent teaching and get on with the truly creative work of publishing, a habit that becomes second nature if and when we get tenure. COVID-19 has now turned things upside down, as the inessential becomes essential. Without teaching there is no public university—teaching is not only a *raison d'être*, it is the material basis of the university's existence. The pandemic offers an opportunity to recalibrate the unequal relation between teaching and research. I argue that research poses questions to be explored through teaching, and teaching prompts new directions of research. This two-way circulation between teaching and research is essential to so many academic careers, yet it is rarely examined—one facet of the denial that we inhabit and are inhabited by the world we study. In a reflexive science teaching and research are in perpetual conversation.

FOR A REFLEXIVE SCIENCE

The transformation of the university is more than an interesting object of study or a minor subfield within the sociology of education; it defines the underlying conditions that shape the principles and practices of research and teaching. Recognizing that we are ineluctably part of the world we study calls for a reflexive science constituted by the articulation of two axes: a scientific axis involving a dialogue between theory and data and a hermeneutic axis involving a dialogue between observer and participant.¹

¹The notion of a hermeneutic axis derives from Giddens's (1984) double-hermeneutic. In contrast to the single hermeneutic of the natural sciences that studies inanimate objects, social science appears in the world it studies. The double hermeneutic, therefore, implies a dialogue between observer and participant. Of course, the distinction between the natural and social sciences is not so simple. Following Heisenberg's uncertainty



This position rejects two extremes. On the one hand, it rejects the positivist temptation that elevates the scientific axis at the expense of the hermeneutic axis. The positivist impulse emulates natural science, conceived of as an induction machine based on a (presumed) solidity of facts. Positivists don't regard their relation to the world as integral to their comprehension of that world. Nor do they consider their own practice of science as subject to the laws that govern the people they study. Instead, they follow what Gouldner (1970) termed methodological dualism—while we know what we are up to, those whom we study do not. On the other hand, a reflexive science rejects the postmodern temptation that reduces scholarship to the hermeneutic axis and dismisses science as an exercise in futility, an exercise of power, or just another discourse. Postmodernism denies its own premises since, despite itself, it makes claims about the world and the way it works; it is shot through with unexamined causality and unjustified explanations. The rhetoric of antisience conceals bad science.

Each pole mirrors the other's self-contradiction: If positivism privileges itself by claiming to stand outside society, postmodernism privileges itself by claiming there are no privileges. Transcending both extremes, I argue that a science without hermeneutics is blind, while hermeneutics without science is empty. Let me describe how I came to this view.

The Extended Case Method

I arrived in Berkeley in 1976 as an assistant professor. The sociology department was still recovering from the divisive 1960s, when the free speech movement, antiwar movement, and civil rights movement had thrown the university into disarray. I was recruited by graduate students who thrust my candidacy onto a fractious department. I was their token Marxist. But students couldn't figure me out. How could I be an ethnographer as well as a Marxist? After all, Marxists deal with large-scale, macro history while ethnography deals in small-scale, micro processes of the present. They could not be further from each other. This, indeed, was then the current view of ethnography, or participant observation, as it was called—a view held by the so-called humanist left in the department, including Troy Duster, David Matza, and Arlie Hochschild. Behind them stood the towering personality of Herbert Blumer, who had brought his prodigies, Erving Goffman and Tamotsu Shibutani, from Chicago after he was appointed to lead the Berkeley department in 1952. When I arrived Blumer had retired, Goffman had left, and Shibutani had been denied tenure, but the Chicago legacy of participant observation remained, focused on social interaction in bounded situations.

I, too, had just come from the University of Chicago, where I had received my PhD, only too aware of its constricted view of ethnography. Actually, during the years I was there (1972–1976), ethnography was in abeyance as the “macro” men with their big data began to take over the department. I was dismayed by the narrow framing and provincialism of both camps. I had just arrived from central Africa, which had been a laboratory for studying the industrial revolution overtaking southern Africa (Gluckman 1961). Conventional anthropology of the isolated, self-reproducing village was cast aside. Colonialism had to be figured into the analysis of rural life, analysis that attended to indirect rule and labor recruitment. Studying a village in southern Africa could no longer avoid the wider political and economic fields within which it was embedded and which disrupted its most intimate relations. The symmetrical kinship patterns, such as those described by

principle, we know that in quantum physics the observer also affects what is observed, analogous to the dialogue between observer and participant. Feminist epistemology of natural science also emphasizes the location of the scientist in relation to the object of observation (Keller 1985).



Evans-Pritchard in his classic study of the Nuer made little sense in the new context. Rules governing marriage or the gender division of labor—rules gleaned from informants who described what should happen—were regularly flouted, especially when men were absent, working on the distant mines. In the classical functionalist framework, such exceptions to normative patterns were shoved under the rug, but in the new situational analysis pioneered by the Manchester School of social anthropology, led by Max Gluckman, exceptions instead became the focus of attention. Social action came to be understood not as the blind execution of unambiguous norms, but rather as the manipulation of discrepant norms in pursuit of political and economic interests.

This perspective had been instilled into me by my teacher Jaap van Velsen (1960, 1964), an ardent member of the Manchester School. It informed my studies of the Zambian copper industry and then student politics at the University of Zambia. I supplemented reliance on informants with direct observation of strategic action as shaped by broader forces—the national class forces that gave rise to the reproduction of the color bar on the copper mines (Burawoy 1972) and those that shaped the dynamics of the student movement (Burawoy 1976). The Manchester School of social anthropology called this the “extended case method” (van Velsen 1967).

Against this background, Chicago ethnography seemed impossibly parochial—as if the world began and ended in Chicago. I decided to lay aside my interest in Africa and engage Chicago sociology on its own grounds. Just as Max Gluckman had instructed his students to find sites in England parallel to their studies in Africa, so I searched for employment in a factory in South Chicago. I eventually landed a job as a machine operator in the engine division of Allis Chalmers, a multinational that produced agricultural and construction equipment. By a twist of fate, it turned out to be the same plant Donald Roy, one of the great Chicago ethnographers, had studied as a participant observer 30 years before. I discovered this by pulling his thick and dusty dissertation off the shelves in the Regenstein Library (Roy 1952). The connection was unmistakable but disconcerting. As an account of the shop floor, his was so much more sophisticated and engaging than anything I could ever conjure up. He was the genuine article, with a long resume as an industrial worker, whereas I was a middle class neophyte who had never worked in a factory before. To make matters worse, very little had changed over the intervening 30 years, so I feared my study would be not only second-rate but redundant.

But then I built up a head of steam against isolationist ethnography—plant sociology—that treated the factory as anthropologists would treat an isolated village. I planned to launch a frontal attack on Roy’s study (and the famous articles he had published from his dissertation) for ignoring the political and economic context in his explanation of restriction of output. It took me some time to cool down and realize I was sitting on a gold mine, namely the possibility of an overtime study. Accordingly, I turned the ethnographic genius of Donald Roy to my advantage, conceptualizing the changes on the shop floor between 1944 and 1974 as a movement from a despotic to a hegemonic social order. In trying to explain those changes, I was forced beyond the factory: shop floor relations had shifted with the company’s move from the competitive to the monopoly sector with its tighter state regulation of management-worker relations. But on what grounds could I justify this extension to the wider economy and the state? Was it just an arbitrary extension? These were the questions that my colleagues wanted me to answer. These were the questions that would preoccupy me in teaching ethnography.

Ethnography Unbound

Confronting the skepticism of Berkeley graduate students that participant observation was compatible with comparative and historical analysis, I devoted my first graduate course in participant observation to studies that extended out from the micro to the macro. I drew on anthropology



from Africa and India, but these monographs were far removed from the substantive interests of students. They were irritated with me and I with them. My first teaching disaster! So the next time I taught the course I borrowed Howard Becker's technique of sending students straight into the field. That worked much better and I never looked back. With a monopoly of knowledge over their fieldsite, this approach gave students an authorial voice, the confidence that they too could be producers of knowledge. Now their own field research gave meaning to everything they read, even the most arid texts. Twice-weekly seminars were organized around the presentation of reports from the field, excerpts from field notes, analytical memos, literature reviews, drafts of papers. Students quickly learned to interrogate and learn from one another's field notes, offering astute comments on each other's research. Instead of one project, they were involved in 10 projects, directly in their own and vicariously in 9 others.

Learning to be an ethnographer is not like learning statistical techniques; there are no set rules as to how to behave in the field. Each fieldsite generates its own challenges and so only together could we learn the craft of ethnography. I wanted students to appreciate what it meant to be the subject of the ethnographic gaze and so replicated the dialogic method I extolled by asking for a volunteer to study the class itself. Anyone who thought this might be an easy task, was in for a surprise. They sat in the class taking notes, were known to the members of the class, and had to grapple with the three-way power relation among themselves, the other students, and myself. Like all participants in the seminar, they had to offer a sample of their field notes for collective discussion that often led to pandemonium, as they had to interpret the class to itself. Everyone learned just how problematic ethnography could be, especially if the ethnographized were consulted at every step. It also upended the idea that the university, the seminar, or pedagogy was outside the scope of sociological investigation, dissolving the divide between the university we inhabit and the world we study.

I made up for my increasing marginality in the seminar meetings by working with students individually. I would not accept field notes unaccompanied by an ongoing analysis of some sociological question. I wanted students to conceive of their fieldwork as a running experiment in which analysis generated in one visit would lead to questions for the next. This began in the first week of the course when I required students to propose a fieldsite, explain why it was interesting, and outline what they expected to find. As long as the fieldsite was unfamiliar, I knew their anticipations would be off the mark, creating a surprise—a puzzle that would set them off on their sociological journey. My role was to keep them on task, pushing them toward a convergence between the data they were collecting and some body of sociological knowledge to which they were contributing, encouraging them to explore the world beyond their fieldsite, revealing the macrofoundations of microsociology.

At the end of the course we read Glaser & Strauss's (1967) *The Discovery of Grounded Theory*, the bible of inductive qualitative research. Rejecting the structural functionalism that poured out of the brain of the grand theorist Talcott Parsons, Glaser and Strauss resurrected the Chicago tradition by insisting that theory comes out of the ground, out of the data. Clear your head of the theory you have learned, accumulate your field notes, then look for patterns of behavior within the fieldsite. In this view theory arises tabula rasa from systematic examination of the fieldsite. It suppressed the existence of a scientific field—a community of scientists, competing with one another to advance knowledge—in favor of the lone discoverer. Like a sprouting plant, sociology springs spontaneously out of the ground—never mind the roots cultivated by the nutritious soil. Along with induction came the stern prohibition against making claims that went beyond the data, beyond the boundaries of the fieldsite.

In the fall of 1988 the seminar attracted a particularly convivial group, pursuing a variety of interesting projects in the Bay Area. When the semester ended and papers were drafted,



we decided to continue meeting and a book began to emerge. What bound it together was an unbound ethnography that explored the wider forces shaping the fieldsite: the school that required a community, the welfare agency embedded in the state, the workplace situated in a market, the social movement located in a political field, the ethnography seminar housed in a university. In pursuing these extensions, we couldn't rely on direct observation alone; we drew on secondary sources and on existing theory guiding the connection between micro and macro, situation and context. It was neither the discovery of new theory nor the mechanical application of old theory, but the extension of existing theory by making it relevant to new cases.

This seminar subjected budding ethnographers to an intense circulation between (a) a dialogue between participant and observer—getting into the field, facing the ethical dilemmas of studying others in their space and time, and then getting out of the field—and (b) a dialogue between theory and data, finding a relevant theory, showing its limitations, and reconstructing it in the light of fieldwork observations. The seminar dramatized the tension between the scientific and hermeneutic dimensions of research. The resulting compendium, *Ethnography Unbound* (Burawoy et al. 1991), explicated and developed the extended case method; its successor, *Global Ethnography* (Burawoy et al. 2000), a more ambitious collaboration with more advanced students, further extended the extended case method to trace everyday experiences to forces, connections, and consciousness in a globalizing world. We sought to ground world systems analysis in everyday life as well as ascend from everyday life to its global determinations.

While teaching ethnography, my own research faced a barrage of criticisms from the positivist wing of sociology—how could I make such wild claims on the basis of an N of 1? How representative was my case? I knew the extended case method would remain a marginal approach in a marginal mode of investigation—so long as it did not grapple directly with the nature of social science. Here the intuitive approach of the Manchester School was of little use.

Scientific Research Programs

I needed an introduction to the philosophy of science. I had the good fortune to encounter Tom Long, a brilliant philosophy undergraduate at Berkeley who later enrolled in our graduate program. As part of his PhD qualifying examination, he agreed to induct me into the new philosophy of science that moved beyond positivism. What I learned with and from Tom would provide the foundation of an introductory methods course.

Most introductory methods courses take first-year graduate students through the techniques of data collection: survey research, experimental methods, archival research, interviewing, and participant observation. Instead I organized the course around the question: Is sociology a science? There were two seminars each week—one devoted to a conception of science and the other to its application in social research.

The first half of the course examined the different understandings of science—from the induction of John Stuart Mill and August Comte to the falsificationism of Karl Popper, the epistemological anarchism of Paul Feyerabend, the personal knowledge of Michael Polanyi, the scientific revolutions and normal science of Thomas Kuhn, and finally, the scientific research programs of Imré Lakatos. Each approach to science proposed a different relation between theory and data. With Kuhn and Lakatos, however, the analysis of the relation between theory and data has to be situated in a historical context, exposing the hermeneutic dimension of science.

The second half of the course explored critiques of sociology as a science, beginning with Weber's combination of interpretation (the hermeneutic dimension) and explanation (the scientific dimension), followed by Peter Winch's repudiation of causality, elevating the self-understanding of subjects; Paul Rabinow's centering of the dialogue between participant and



observer; Michel Foucault's restoration of the scientific axis only to make it the foundation of domination; and Pierre Bourdieu's defense of a reflexive social science. The course culminated with Jürgen Habermas's plurality of interests behind positivist, interpretive, and critical knowledge. My goal was to show that the critics of sociology-as-science assumed an old-fashioned positivist view of science, ignoring the historicist views that recognized the hermeneutic dimension of science.

This course led me to ground the extended case method in Lakatos's (1978) notion of research programs (Burawoy 2009). Lakatos begins with Popper's critique of induction—science advances not through confirmations but through refutations—but then takes it one step further: Science actually advances through the refutation of refutations. Studying the practice of scientists and mathematicians, Lakatos argues that they don't abandon their theories in the face of refutations, for that would lead to perpetual chaos. They either simply turn a blind eye to refutations or show how they can be accommodated, with the help of an auxiliary conjecture or theory, within a set of hard core assumptions. In other words, progress is measured by the way exceptions are made to prove the rule. For example, perturbations in the orbit of Uranus seemed to be a refutation of planetary theory, until physicists conjectured that it could be explained by the existence of a hitherto unknown planet, which then led astronomers to search for and find the planet Neptune. An apparent falsification was turned into an astounding corroboration of Newtonian theory.

A research program advances, therefore, when anomalies are absorbed by the development of an expanding belt of auxiliary theories—what Lakatos called the positive heuristic—that are built on and reinforce a hard core of inviolable premises—what he called the negative heuristic. The scientific field is an arena of competing research programs wherein progressive programs displace degenerating programs. A research program is progressive if it leads to novel and parsimonious theories that absorb anomalies but also lead to successful predictions. It is degenerating if it has to patch up anomalies with cumbersome, arbitrary conjectures with no predictive power. The distinction between a progressive and degenerating program is elastic; a degenerating program can recover and become progressive.

To demonstrate Lakatos's research program approach as against the conventional method of induction, I compared Theda Skocpol's and Leon Trotsky's theories of revolution (Burawoy 1989). Skocpol's *States and Social Revolutions* (1979) proceeds inductively. Comparing successful revolutions (French, Chinese, and Russian) with failed revolutions (English, German, and Japanese), she singled out two factors that make for a successful revolution: on the one hand, tensions between dominant classes and the state, on the other hand, peasant revolt. She assumed, as the method of induction requires, that the same factors operate in all successful revolutions and are absent in all failed revolutions, concluding, therefore, that peasant revolt was essential to the Russian Revolution while working class revolt was incidental. Controlling for history, she assumed that the causes of revolutions were the same in the seventeenth century as in the twentieth.

Skocpol's approach is positivist: She stands outside history but on the slippery ground of historical facts, inducing a singular theory of revolution that arbitrarily separated the modern period from the classical period ending with the Chinese Revolution. By contrast, Trotsky's approach to science is reflexive. He is a full and self-conscious participant in history, attentive to its social processes within a changing capitalist world order. He grounded himself in Marxist theory, reconstructing it in the face of an unexpected anomaly—the Russian Revolution. His auxiliary theory of the combined and uneven development of capitalism, first advanced in *Results and Prospects* [Trotsky 2010 (1906)], brilliantly anticipated the outbreak, dynamics, and dénouement of the Russian Revolution of 1917. The theory was later elaborated in *The History of the Russian Revolution* [Trotsky 1977 (1933)] and *The Revolution Betrayed* [Trotsky 1972 (1937)].

In this way Trotsky contributed to a progressive research program as well as to a particular (albeit tragic) revolutionary practice. Trotsky's failing was an overly optimistic theory of the

revolutionary potential of the Western working class. Indeed, the failure of revolution in the West became the defining anomaly addressed by twentieth-century Western Marxism—for example, the Frankfurt School focused on the psycho-dynamics of repression, Lukács on reification and Gramsci on hegemony. If Western Marxism became a progressive branch in the Marxist research program, Soviet Marxism as a ruling ideology epitomized a degenerating branch that would fall off the Marxist tree. For those who couldn't see the tree for the branch, the end of the Soviet Union marked the end of Marxism; for those of us who saw a recovering tree, it signaled the possibility of Marxism's revival.

MARXISM AND ITS ANOMALIES

Lakatos's idea of a scientific research program implicitly informs the design of PhD programs in the United States. Whether to make up for ground lost in a relatively unfocused undergraduate education or because of genuine commitment to the growth of knowledge, the US PhD is unique in devoting three or four years to coursework, culminating in qualifying examinations. Graduate students study the existing literature to appreciate both its accomplishments and its limits. Our fields of specialization are, in effect, embryonic research programs. When it comes to the dissertation itself, students are expected to lay out the relevant literature in order to identify a flaw, a problem, or an anomaly and to mark out their own distinctive contribution, rejecting some theories while building upon others. We ask ourselves, "What's the puzzle, and how are we going to tackle it?" That is how I proceeded in my own research.

An Anomaly Redefined: Manufacturing Consent

The history of industrial sociology was long dominated by the question of restriction of output—why workers don't work harder—as in Donald Roy's celebrated studies. But this was a managerial perspective; a Marxist perspective would ask instead why workers work so hard as to produce surplus value that is the basis of capitalist profit. This question struck home when I arrived on the shop floor, astonished by the intensity of work and the ingenuity of machine operators, all to maximize output. Marxists had assumed it was coercion, fear of being fired, that drove work intensity. But I saw few signs of despotism. As my day man used to say, "No one pushes you around here." So why, I asked, do workers consent to their own exploitation?

Having rejected the disappearing research program of industrial sociology, I turned to an emergent Marxist research program, propelled by Braverman's (1974) *Labor and Monopoly Capital*, published toward the end of my field work. A celebration, elaboration, and update of volume one of *Capital*, it was a detailed historical account of the expropriation of control from wage laborers. Yet Braverman paid no attention to the subjective side of deskilling—how it was possible to get laborers to apply effort and ingenuity to meaningless work. Here I drew on an unlikely source: Marxist theories of the state that I had imbibed in a transformative seminar given by Adam Przeworski in 1973. We studied the fashionable French structuralist Marxism of the time, an indirect response to the upheavals of 1968. We read Louis Althusser, Etienne Balibar, Nicos Poulantzas, and Maurice Godelier through the lens of Gramsci's (1971) theory of hegemony, understood as a particular relation between state and civil society and based on class compromise. The assumption was that hegemony—consent backed up by the armor of coercion—was organized outside production, containing working class mobilization and counteracting class consciousness forged in production.

I was struck by one sentence in Gramsci's *Americanism and Fordism* claiming, without elaboration, that in the United States, "hegemony is born in the factory" (Gramsci 1971, p. 285) What



could this possibly mean? On the basis of my year-long sojourn as a machine operator, I argued that the factory had its own political order, composed, first, of an internal state in which grievance machinery constituted workers as industrial citizens with rights and obligations, while the collective bargaining agreement was a class compromise coordinating the material interests of capital and labor. A second component was the internal labor market—a seniority-based bidding system for vacant positions that gave workers power over their supervisors while attaching them more to firmly to the company. I added a third component: the constitution of work as a game whose agreed-upon rules are enforced by workers themselves, giving a shared meaning to (hard) work, making time pass more quickly, and transforming boredom into heroic displays of conquest. Vertical antagonisms between management and workers were thus transformed into horizontal conflicts between operators and auxiliary workers. I called this the hegemonic regime of production.

I had redefined an old anomaly—the absent working class revolution—as an anomaly within the Marxist theory of production. Under advanced capitalism, the site from which class struggle was supposed to emanate turned out instead to be the locus of “manufacturing consent.” Working class struggles in the New Deal had led to a degree of employment security in the form of unemployment benefits and state regulation of labor relations. But now the working class was not the grave digger of capitalism, as Marx had predicted, but its savior, imposing constraints on managerial despotism. If, under competitive capitalism, workers could be fired at will and coercion reigned on the shop floor, under advanced capitalism management was constrained in its exercise of coercion and, therefore, had to persuade workers to work hard via the internal state, internal labor market, and the organization of work as a game. In the 30 years since Roy’s study, the factory regime had moved along the continuum from despotism to hegemony.

Critics of *Manufacturing Consent* (Burawoy 1979) doubted that the hegemonic regime was a feature of capitalism rather than a function of industrialism, broadly conceived. I knew that in the nonunionized competitive sector of advanced US capitalism, despotism still reigned and there was yet another regime in the public sector. But what about other countries? I went on to explore the politics of production in Japan, England, and Sweden. I discovered variants of the hegemonic regime, depending on the way the state set conditions for the reproduction of labor power through dispensing welfare and regulating relations between management and unions. The hegemonic regime could also be contrasted with despotic regimes of various types: the market despotism of nineteenth-century capitalism (here I compared the United States, Russia, and England) and colonial despotism (the labor process in the Zambian copper industry). The result was *The Politics of Production* (Burawoy 1985), which gave the ideas of *Manufacturing Consent* a comparative and historical dimension.

Still, I was not answering the deeper question as to whether the hegemonic regime was a peculiarity of capitalism or a general feature of industrialism. It was not enough to demonstrate the varieties of hegemonic regimes under capitalism; I had to compare them with regimes of a noncapitalist society. The obvious candidate was state socialism, but this was a fraught terrain. Marxists conveniently treated Soviet society as a deformed capitalism, state capitalism, or simply statism—suppressing an anomaly by simply defining it out of existence.

Yet the Soviet Union’s fateful experiment in the realization of socialism on earth was key to twentieth-century history. It could not be reduced to a tragic mistake—socialism attempted under the most unfavorable circumstances—and shoved under the rug as though it were irrelevant to the socialist project. Just as we study the way capitalism departs from its own idealization, so we have to recognize Soviet society to be a form of socialism—state socialism—and study how, why, and with what consequences it departed from its promise.

An Inconvenient Anomaly: State Socialism

My interest in state socialism was piqued by Miklos Haraszti's (1977) *A Worker in a Worker's State*. Consigned to work in the Red Star Tractor Factory as punishment for dissidence, he turned his experience into a devastating portrait of despotism in a Hungarian factory. As it happened, he was working as a machine operator at the very time I was working at Allis Chalmers. I knew enough about machine shops to know his description was authentic, but it also presented a new puzzle. According to conventional wisdom, the one right Soviet workers had secured was the right not to work hard—yet Haraszti was working twice as hard at Red Star as we were at Allis, running two machines where we were running one. As a socialist factory, I wondered, what was typical and what was anomalous about Red Star? Haraszti made it difficult to answer this question. His unflinching indictment of socialism presumed that Red Star was simply a typical factory. Confining his attention to the shop floor, he ignored the wider forces that might have made it anomalous.

A study of the socialist workplace, such as Haraszti's, was very rare. In the Soviet order few sociologists were interested in ethnographic studies; the survey was the principle instrument of scientific investigation. Moreover, in a regime that prided itself on being a workers' state, critical views of the workplace were unpublishable. Indeed, the only person to violate this code in the Soviet Union was Andrei Alekseev, who became a worker-sociologist in a St. Petersburg industrial plant, which cost him his job at the Institute of Sociology and much calumny.

While I was puzzling over Red Star, the Polish Solidarity movement erupted. Like so many, I was riveted to reports of this self-limiting revolution and astonished by its longevity, from August 1980 to December 1981. It presented a second puzzle: How was it that the first nationwide working class revolution took place in state socialism rather than advanced capitalism? The Solidarity movement opened up the possibility of undertaking research in Poland, but I had no sooner laid down plans than it came to an abrupt end with the declaration of martial law.

It was at that time in 1982, I fortuitously met the great Hungarian sociologist, Ivan Szélenyi. He was about to return to Hungary after six years of exile in Australia and invited me to accompany him. That led to 10 days that shook my world and demolished my preconceptions about state socialism. Hungary was not some gray despotic order but a radiant society with all the trappings of a welfare state and an administered economy supplemented by markets. With my two puzzles—one to do with the socialist labor process and the other to do with working class mobilization—I spent the next seven years on and off working in Hungarian factories. The rural areas were most readily accessible: With the help of friends I found my way into a champagne factory on a state farm and a small textile cooperative on a collective farm. Then, in the summer of 1984, I got a job in another machine shop at Csepel Auto, making gear boxes for Ikarus buses. By another coincidence the technology and piece rate system was similar to the one on which I had worked at Allis Chalmers. And finally, in the following year, I made my way into the heart of the socialist working class: With the help of my friend and collaborator, János Lukács, I landed a job as a furnace man at the Lenin Steel Works. I worked there for over a year in three stints between 1985 and 1987.

Compared to Allis Chalmers, I discovered, workers at Csepel Auto had employment security but wage insecurity. When workers at Allis found piece rates on a particular job to be too tight, they simply reduced their work tempo, content to get the basic wage. But at Csepel Auto, without a guaranteed basic wage, operators had to increase their work tempo, although on occasion, they might simply quit and storm home in protest. I could now understand how Haraszti's experience was possible. It turned that Red Star had been the target of market reforms in the early 1970s. On the shop floor that meant tightening the rates so that workers had to run two machines to make a living wage. At Csepel Auto, however, the rates had not been screwed down.



The Lenin Steel Works was very different. An integrated steel mill employing some 15,000 workers, it dominated the industrial city of Miskolc. As a furnace man I found myself at the heart of steel production in a newly installed complex and semiautomated process that depended on the collective self-organization of workers. There Kornai's (1980) theory of the shortage economy came alive—the inputs and operation of the 80-ton converter could not be effectively controlled without the ingenuity of the shop floor operators. Managers resented the usurpation of their authority, leading them to impose quasi-military rule, but that only magnified the dysfunctions of production.

So much for the socialist labor process, but what about the political regime of production? The state, far from exercising its power indirectly as in the United States, had an overbearing presence in the workplace: The triumvirate of management, trade union, and party were all extensions of the state. Here I elaborated Konrád & Szélenyi's (1979) argument in *The Intellectuals on the Road to Class Power* that state socialism rested on the central appropriation and redistribution of surplus that had to be legitimated as being in the interests of all. I argued that when party rituals claimed to pursue efficiency, equality, and justice, workers responded with comic and sometimes angry tales of inefficiency, inequality, and injustice. They turned the dominant ideology against the party-state for failing to deliver on its promises, sparking the development of class consciousness with socialist aspirations. Or so I claimed.

The combination of collective self-organization of production and the development of socialist consciousness might explain why a sustained nationwide working class mobilization could take place in state socialism rather than advanced capitalism. But why in Poland rather than Hungary, especially given the legacy of Hungary's 1956 workers' revolt? Here I argued that Hungary's economic reforms, unique within the Soviet bloc, channeled dissent into the individualizing second economy, whereas in Poland an embryonic civil society supported dissident collectivities, be they the networks joining workers and intellectuals or the umbrella of the Catholic Church. As state socialism began to disintegrate in the late 1980s, I anticipated a movement toward democratic socialism. My optimism was misplaced: While that movement did exist, it was at best a minor tendency. Instead, most workers had become cynical about any socialism: Critique had turned into contempt for the party-state, and they were ready to exchange its false promises for the false promises of capitalism. Inasmuch as they were engaged in such issues at all, a rapid transition to capitalism seemed to be the best alternative to the woes of state socialism. As Lukács and I anticipated in *The Radiant Past* (Burawoy & Lukács 1992), they didn't know how good they had had it under state socialism.

An Unexpected Anomaly: Transition to Capitalism

Now I confronted another puzzle: Why did state socialism, which had seemed so stable, crumble so easily and so unexpectedly? In Eastern Europe nationalist dissent had fired opposition to the Soviet order, so a more fundamental answer might lie in the exploration of the Soviet workplace. I abandoned Hungary for Russia. I arrived in 1990 to witness the upsurge of civil society, and then in the first half of 1991 I unknowingly witnessed the collapse of this mighty empire.

Only at that time was it conceivable for a foreigner to conduct an ethnography of a Soviet enterprise. Kathryn Hendley, then a graduate student in the political science department at Berkeley, managed to negotiate our entry into a Moscow rubber factory. There we watched an escalating civil war in which insurgent young engineers fighting for liberal democracy, the market, and Russian independence were arrayed against the old guard, led by the General Director and the Chief Engineer, defending Soviet planning and the party-state. The disintegration of the planned economy led to desperate measures to garner resources but also allowed a few to make a killing from hidden cooperatives that channeled state resources into private profit (Burawoy & Hendley 1992).

I observed something similar when, later that spring, I accompanied my colleague Pavel Krotov to Syktyvkar, near the Arctic Circle. There we wormed our way into a furniture factory that had a regional monopoly in the production of wall systems—an arrangement of shelves, drawers and cabinets found in every Soviet apartment. While I observed the shop floor as a machine operator, Pavel interviewed the higher ups. We studied the commodity chain that presented management and trade union officials with the opportunity to barter wall systems for such scarce items as sugar, alcohol, and even places in vacation homes. A primitive system of exchange was replacing the broken planned economy (Burawoy & Krotov 1992).

I left in July 1991. In August Yeltsin famously repelled an attempted coup to restore the old regime. By the end of the year, the Soviet Union was no more. When I returned the following year, the furniture factory and many other enterprises were heading for bankruptcy. The shock therapy approach to the transition from state socialism to capitalism was all shock and no therapy. The pseudomarket reigned supreme, but without regulation, it simply destroyed everything in its path. The mafia and the banks milked what was left. This was a reversion to merchant capitalism, dominated by profit from exchange at the expense of production. Together with my colleagues Pavel Krotov, Svetlana Yaroshenko, and Tatyana Lytkina, we studied the disappearance of the Soviet economy, tracking the suffering of workers cast out from Syktyvkar's industries. While the economists were debating whether revolution or evolution was the path forward, Russia was making a deep dive into involution (Burawoy et al. 2000).

Here, indeed, was an unexpected challenge for a Marxist research program—the transition from socialism to capitalism. Trotsky had speculated on this possibility in the 1930s, but he had no premonition of its concrete form. To tackle this anomaly, Marxism needed an infusion of new blood, and it came from the rediscovery of Karl Polanyi's (1944) *The Great Transformation*. Opposing shock therapy, sociologists and a few economists invoked Polanyi's warning that there is no market road to a market society. As China's incubation of a market economy under the direction of the party-state demonstrated, the transition to capitalism does not appear spontaneously but requires political and social planning.

What happens when Polanyi's sociological message is ignored? The answer lies in the dynamic side of Polanyi's theory, namely the reaction to market fundamentalism. Based on the nineteenth century, he expected social movements to defend society; based on the twentieth century, he expected state regulation, whether in the form of the New Deal, social democracy, fascism, or Stalinism. But in post-Soviet Russia we found another response, not a counter-movement but a generalized retreat into the family and subsistence agriculture that dovetailed with the rise of a new authoritarianism.

Polanyi's elevation of commodification over exploitation, exchange over production, and the market over the labor process offered some an exit strategy from Marxism, but others took it as an opportunity to enrich Marxism's account of the destructive powers of the market (Burawoy 2000). The monster anomaly in Polanyi's own account was his foreclosure of a new round of marketization, specifically the one that took off in the 1970s. He thought humanity would never again experiment with a market utopia and that it would never again follow the misguided musings of utopian economics. He failed to see how capitalism solves its crises of accumulation through the commodification of labor, money, and nature, Polanyi's so-called fictitious commodities. Rather than capitalism involving one single historical wave of marketization that stretched from the seventeenth to the twentieth century, giving rise to counter-movements, I argued that it has called forth three waves of marketization, identified by changing articulations of the commodification of labor, money, and nature (Burawoy 2013). Rather than being a precursor to capitalism, as in Marx's primitive accumulation, these successive commodifications are a sine qua non of capitalism's durability. Moreover, to turn labor, money, and nature into objects of exchange requires



their violent expropriation from their social integument, what Harvey (2003) calls accumulation through dispossession.

Each wave of marketization would have destroyed society were it not for counter-movements—local reactions to nineteenth-century marketization, state-organized reactions to twentieth-century marketization, and a global reaction to twenty-first-century marketization. The jury is out on whether such a global response to the commodification of nature (climate change), labor (migration, refugees, precariatization), and money (finance capital)—each commodification intensifying the others—will be feasible or durable, reactionary or emancipatory. We are in the midst of local and national responses to third-wave marketization, but global reactions are far weaker. And now we have a fourth fictitious commodity to add to the mix—knowledge. Zuboff (2019) shows how the digital revolution has turned us into voluntary accomplices in a new regime of accumulation: surveillance capitalism. The universal enthusiasm for digital technology delivers surplus information that, behind our backs, turns our everyday life into a commodity, in a perfect marriage of control and profit.

In rejecting Marxist teleology, Polanyi lost sight of the continuing centrality of accumulation, but by the same token he offered a new understanding of class struggle, built around the tangible experience of commodification. Under third-wave marketization, considered globally, exploitation is a sought-after privilege and a declining source of protest; yet commodification affects everyone, suggesting a lens through which to understand the new social movements of today. The vision of socialism shifts accordingly: from the elimination of exploitation to the decommodification of labor, money, nature, and knowledge, perhaps best captured in Wright's (2010, 2019) "real utopias."

Third-wave marketization set the context for COVID-19, which has exposed but also magnified inequalities, concentrating the power of big capital, bankrupting small businesses, expelling great swaths of workers from the labor market, and leaving only essential workers plunged into excessive servitude, compelled to sacrifice their lives to save society. As we move from the protection of commodities to their expulsion into wastelands—that is, from decommodification to ex-commodification—the state desperately but uncertainly intervenes to contain the rebellion of the excluded and to forestall the specter of socialism.

CONVERSATIONS WITH SOCIOLOGY

For over a century and a half, the Marxist research program has advanced in response to a series of historical and geographical challenges. Theoretical responses were contested, subject to debate, and put to the test of political interventions (Burawoy 1990). My own explorations of the research program were directed at postcolonialism's racial order, the hidden abode of capitalist hegemony, the longevity and demise of state socialism, and neoliberal forms of commodification. Delving into laboring lives, dwelling in academic Marxism, the hermeneutic and the scientific encircled each other.

But Marxism is not the only research program. How does it relate to other programs? For Lakatos (1978), progressive research programs tackle anomalies with parsimonious auxiliary theories that lead to the discovery of novel facts. Progressive research programs displace degenerating research programs that can only patch up anomalies with convoluted rescue hypotheses with no predictive power. This is how I saw Marxism's academic renaissance in the 1970s, a progressive scientific program supplanting a degenerating sociology and reverberating beyond the university. I was not alone. Reflecting back on those early years, my close friend and colleague Erik Wright (2005, p. 338) wrote of "visions of glorious paradigm battles with lances drawn and a valiant Marxist knight unseating the bourgeois rival in a dramatic quantitative joust."



As Marxism waned once again, we drew back from this triumphalist scenario. Wright (2009) spoke of a pragmatic realism synthesizing Marxism and sociology, while I saw them as interdependent antagonists. It was no longer a battle to the end but respectful critique, a conversation between adversaries. Gouldner (1973, p. 401; emphasis in original) once wrote that Marxism and sociology are like Siamese twins: “The demise of one presages the demise of the other. They have a common destiny not *despite* the fact that they have developed in dialectical opposition but precisely *because* of it.” While Lakatos’s demarcation between progressive and degenerating research programs is relevant in evaluating the internal development of an individual research program, I now see it as less useful for comparing research programs that necessarily coexist within a single discipline. It is not a matter of one research program displacing another but of putting them into dialogue.

We have already seen how my dialogue with Karl Polanyi led to the reconstruction of a Marxism that paid more attention to the lived experience of commodification. More generally sociology is distinctive in the recognition of discrete research programs that reflect divergent standpoints—values, assumptions, questions. This peculiarity follows from the consecration of canonical thinkers who laid the foundations of divergent research programs. These founders came from and ranged over a variety of disciplines—history, economics, politics, law, and philosophy—to constitute, from different vantage points, the new discipline of sociology at the end of the nineteenth and the beginning of the twentieth centuries. As Stinchcombe (1982) writes, the founders provide exemplary research, offer models of theoretical thinking, bring unity to our fragmented discipline, and help us define our identity.

The centrality of a canon is contested by positivists and postmodernists alike. Positivists, who repress the hermeneutic axis, consider the founders dead and gone, at best of quaint historical interest, holding back the unification of sociology as a true science. They join Alfred North Whitehead, who famously wrote that a science that hesitates to forget its founders is lost. Postmodernists, skeptical of the idea of social science, also reject the canon but for a different reason: It reflects the arbitrary imposition of dominant powers. In my own vision the canon exemplifies a reflexive science that recognizes a plurality of research programs rooted in different standpoints. Instead of forging a single research program that can forget its founders, sociology has multiple research programs that cannot forget their founders.

The canon may be essential, but it cannot be fixed because standpoints shift with changes in society, politics, and public discourse as well as within our disciplinary field. Thus, the modern sociological canon originated with Talcott Parsons (1937), who tried to show that Weber, Durkheim, Marshall, and Pareto all converged on a single voluntaristic theory of action that would morph into a singular research program: structural functionalism, an evolutionary model in which four basic functional prerequisites of all societies map onto specific interconnected institutions. Parsons consigned Marx to the nineteenth century as an outdated utilitarian thinker. Structural functionalism itself died a silent death when its assumptions clashed with the world it purported to describe. Reflecting the turbulent 1960s, a new, reconstructed canon arose, restoring Marx alongside a rereading of Weber and Durkheim. Unlike in structural functionalism, which tried to forge a singular research program, in the successor vision of sociology Marx, Weber, and Durkheim represented divergent standpoints, unified by their antagonistic relations. In these circumstances the proliferation of conversations among adversaries is necessary not only for self-clarification but to define the boundaries of sociology and to prevent it flying apart into fragments.

Already, in my first graduate seminar at Berkeley, I had set structural Marxism against the structural functionalism, much to the dismay of expectant students. Not surprisingly they were not keen to get into the weeds of Parsonsian writing, an obscure language unto itself, when they were expecting a course on Marxism. Another teaching disaster! But I persisted, arguing that the



two frameworks shared a certain functionalism, although they diverged in their definitions of the problem of social order, making different underlying assumptions and positing distinct mechanisms of social cohesion. No less significant were their opposed understandings of social change. For structural functionalism, societies either were in equilibrium or would collapse: There were no dynamics, only synchronics and diachronics. Marxism, in contrast, regarded the mechanisms of social reproduction as simultaneously mechanisms of social transformation: Reproduction leads to crises that in turn lead to transformation. That was the focus of my first conversation between Marxism and sociology. Later I developed conversations between Marxism and Karl Polanyi to which I have already alluded, and subsequently between Marxism and Pierre Bourdieu to define the limits and possibilities of each. I am now constructing successive conversations between W.E.B. Du Bois and Durkheim, Weber, and Marx in order to rethink the canon. But this conversational approach all began with teaching social theory to undergraduates.

Ethnographic Pedagogy

Back in 1976, Neil Smelser, then chair of the Berkeley department, asked me to teach the required one-quarter undergraduate course in social theory. Fresh out of graduate school, with lousy grades in theory courses, I was uniquely unfit for this task. Elated at having a job of my dreams, I did not think to protest. Thus began my life with theory. Within a few years the course moved from one quarter to two quarters to two semesters; from 50 sociology majors, it expanded to over 200 and then doubled again so that it had to be taught twice a year. Whereas in the early years I had to teach it myself, now younger colleagues have joined me in making it the signature course of our undergraduate major.

When I began I designed the course as a survey from Adam Smith to Jürgen Habermas via Marx and Engels, Comte, Spencer, Durkheim, and Weber. As my own ideas changed and the course was extended to two semesters, it developed into an elaboration of a Marxist research program in dialogue with sociological critiques of Marxism. For the first semester I experimented with different Marxists: Trotsky, Luxemburg, and Bernstein, until I settled on Lenin, Gramsci, and finally Fanon. We spent six weeks on Marx and Engels, building up their theory of capitalism to highlight three fundamental limitations: a flawed theory of class struggle, an undeveloped theory of the state, and an absent theory of transition to socialism. Subsequently, Marxists tackled these limitations by developing auxiliary theories, prompted by the political challenges of their time—Lenin facing the impending Russian Revolution, Gramsci facing the durability of capitalism, and Fanon facing the transition to a postcolonial world. The Marxist tradition advanced not through biblical recitation but by recognizing and addressing the defects in the original theory. We don't demolish the old house but rebuild it on the existing foundations.

For the second semester I dropped Comte and Spencer, started with Durkheim, introduced Foucault as a bridge to Weber, and experimented with various feminist texts. It was in Berkeley that I was first introduced to feminism by my colleagues—Arlie Hochschild, Nancy Chodorow, Kristin Luker, and Barrie Thorne—but especially by graduate students. It had been shockingly absent in my education in Zambia and Chicago. As far as the theory course was concerned, the breakthrough came with my study of the life and work of Simone de Beauvoir. The scope and brilliance of *The Second Sex* (de Beauvoir 1953) posed a series of challenges to Marx, Weber, and Durkheim, a fitting end to the course that turned sociology upside down and inside out.

Gradually I came to see the relationship between the first and second semesters as a series of conversations with Marxism, between Marx–Engels and Durkheim on the future of the division of labor, between Lenin and Weber on bureaucracy, between Gramsci and Foucault on state and civil society, and between Fanon and Beauvoir on intersecting oppressions. The course



ended with an embryonic feminist research program: from Simone de Beauvoir to Catharine MacKinnon to Patricia Hill Collins, calling attention to masculinist lacunae of previous theorists while also compelling students to reflect on their own experiences of gender and sexuality. Feminism radically questions the objectivity of canonical theorists, showing how their ideas are rooted in divergent standpoints within society and driving home the idea of a reflexive sociology bursting with different, antagonistic research programs.

The central challenge was to make dead white men come to live in the minds of undergraduates. As I was developing the scientific dimension of teaching—that is, the relation among the theorists—I became increasingly self-conscious of its dialogic or hermeneutic dimension. I eventually developed a distinction between survey and ethnographic pedagogies. In the former students are viewed as empty vessels into which we pour pearls of wisdom, gleaned from a succession of disconnected theories, arranged in chronological order, represented in textbook excerpts and summaries. The survey approach constitutes the student as intellectually limited, programmed to respond to limited questions. It has the advantage of offering a panorama of theories, but it is a distant panorama. Students don't have the chance to climb up any one mountain or to experience the vistas it offers on the way. In contrast, the ethnographic model is inspired from the outset by the project of compelling students to use their own common sense—the tacit theories that guide their everyday worlds—to engage with the canonical texts of sociological theory. The goal is to induce students to live inside theories, putting them to work in the world around. They get a chance to climb a few mountains.

To this end the course focuses on a theme—the division of labor—familiar to students and at the core of social theory. From original texts, I select short, manageable extracts to be read and reread, pieces in a jigsaw puzzle that we collectively assemble in the theater of the lecture hall, illuminating the drama of life beyond.² We examine the pieces that don't quite fit—anomalies and contradictions—gradually absorbing them by reconfiguring the final picture. Students learn theory not through rote examinations or quizzes but through practice: writing synthetic memos, short papers comparing theorists in relation to concrete issues, and a final pictorial reconstruction of the course presented orally to their devoted teaching assistants. It is a course in thinking, writing, speaking, and reading, helping students develop skills in reading newspapers, joining conversations with a sociological eye, and identifying theoretical perspectives in both mundane and consecrated communications.

Over the years I have become clearer about another goal of this course, namely to demonstrate to students that the world does not have to be the way it is. In their own way, each theorist contributes to that project, either by opening up new visions or by arguing against such visions. Sociology, then, becomes a dialogue between the utopian and the antiutopian. I have gone on to apply this understanding of sociology to imagine conversations between Marxism and Bourdieu and to the expansion of the very meaning of sociology through canonizing W.E.B. Du Bois.

Marxism Versus Bourdieu

My conversations with Polanyi were driven by the need to understand my experience of the Soviet transition to capitalism. My conversations with Bourdieu were driven by graduate students. For many years I had refused to admit the work of Pierre Bourdieu into the pantheon of great

²That was in the old days, before COVID-19. As I write I'm about to begin remote teaching. I have to unlearn 40 years of theatrical production and turn to the medium of film. I'm terrified and excited by the challenges and possibilities that lie ahead.



contemporary theory. What I had read of his work struck me as derivative, contradictory, and incomplete: a functionalism without a theory of history. But in the 1990s, as Bourdieu became the contemporary theorist, graduate students demanded I take him more seriously, which prompted me to enroll in Loïc Wacquant's boot camp course on Bourdieu. This opened up horizons I had never anticipated.

It was striking but not surprising, given the formative milieu of Paris in the 1960s and 1970s, that Bourdieu's hostility to Marxism carried within it so many Marxist tropes. Like Talcott Parsons before him, the strategy of combat was to ignore Marxism or reduce it to the flaws in Marx. Thus, Bourdieu refused to acknowledge the originality of Lukács's theory of reification, Gramsci's theory of hegemony, the Frankfurt School's psycho-analytic theory of domination, Freire's theory of education, Beauvoir's theory of masculine domination, and Althusser's theory of structuralism—despite their transparent presence in his thinking. When they were too close for comfort, then he would occasionally lash out at them or, more likely, dismiss them in a few tart phrases. The convergences are not surprising since, like Bourdieu, these Western Marxists began with a critique of Marx for his failure to deal with the durability of cultural domination, or in another language, they were theorists of the superstructures. So I set about recovering Marxists, one by one, by putting them into conversation with Bourdieu (Burawoy 2019).

Convergences notwithstanding, what separated Bourdieu from twentieth-century Western Marxists was his belief that the dominated can never understand the conditions of their domination. The problem with the concept of false consciousness, he wrote in *Pascalian Meditations* (Bourdieu 2000), is not the idea of falseness but the notion of consciousness, which he argued was too shallow to grasp the psychic depths of domination. Through the inculcation of habitus, the dominated inure themselves to domination, naturalize and legitimate their subjugation, and misrecognize their place in the world. By contrast, Marxists never give up the idea and the possibility of the working class becoming a subject-object of history, however difficult that might be.

While Bourdieu (1984) is especially emphatic in denying the possibility of working class insight into its own subjugation, he also claims that the petite bourgeoisie and the dominant classes suffer from misrecognition, leaving only intellectuals with the capacity and interest to expose domination—but not all intellectuals. Sociologists are particularly well placed to do so because their participation in the world allows them to understand the specific conditions of the production of their own knowledge. Too many other disciplines—philosophy and economics are especially guilty—don't appreciate that their theories are as much a reflection of their academic privileges as they are an engagement with the confined lives of their subjects. They mistakenly project their own world onto their objects of study. For Bourdieu, rational choice theory, existentialism, communicative action, and Marxism are all fanciful theories removed from the realities of subject populations.

With their feet on the ground, however, sociologists are less likely to succumb to such scholastic fallacies. In exposing domination, sociologists become the vanguard of the universal, the organic intellectuals of humanity. To give political force to this idea, Bourdieu (1989) called for an “international of intellectuals.” He took full advantage of his lofty position as professor at the Collège de France to put himself at the helm of progressive social movements, from the defense of human rights to opposing neoliberal reforms. During the 1990s he could be found in the streets, supporting workers in their struggles against austerity and neoliberalism as he slid from a reflexive sociology to a public sociology, from the world in sociology to sociology in the world.

Here, however, is the paradox: If the working class could not understand the conditions of their own oppression, why was Bourdieu addressing them? According to his theory his remonstrations should fall on deaf ears. One answer to this contradiction between Bourdieusian theory and practice lies in the interviews that make up *The Weight of the World* (Bourdieu 1999), one of his more

popular books in France. Here the subjects do, for the most part, have an accurate understanding of the worlds they inhabit. But why? Bourdieu doesn't tell us, but there are at least two possibilities, both disruptive of his theory of symbolic domination. First, the interviewers formed a small team of collaborators, many chosen for their knowledge of the life-worlds of the interviewees. They were organic intellectuals, able to elicit sociological understanding from their subjects. Second, the interviewees experienced a mismatch between expectations and resources, between habitus and field, and between disposition and position that, as Bourdieu acknowledges elsewhere, can destabilize symbolic domination. Perhaps this disruption was a feature of the emerging world of precarity in the early 1990s when the interviews were conducted. More recently, the collision between habitus and field has been magnified by the coronavirus, exposing once-hidden inequalities and dominations, and fostering social protest—even if structural change is still far off.

Here is just one example of an anomaly—the exceptions to symbolic domination—that adherents of Bourdieusian sociology must tackle if their research program is to be progressive. They should take seriously the interrogation of adversaries, Marxists included. But interrogation also leaves its mark on the interrogator. My own engagement with sociology shaped the selection of the salient anomalies I tackled within the Marxist research program, just as my development of auxiliary theories borrowed from sociology. My conversations with Bourdieu focused on the significance of symbolic violence for social change; my engagement with Polanyi drew on the importance of commodification for society. That is what makes my Marxism a sociological Marxism, but it is still Marxism, situated within the Marxist tree, pollinated by sociology's attention to the social. Any research program swims in an ocean of anomalies; choosing which ones to pursue is the result of dialogue within the wider political field and/or dialogue within the scientific field. W.E.B. Du Bois was a virtuoso in both types of dialogue.

Du Bois Versus the Canon³

As a historian W.E.B. Du Bois earned a PhD from Harvard in 1895; as a sociologist he received his formative education in Germany (1892–1894). His writings lie at the intersection of an unusual biography of public engagement and nearly a century of history. His doctoral dissertation was an analysis of unsuccessful efforts to suppress the slave trade; his magnum opus, *Black Reconstruction* (Du Bois 1935), transformed previous understandings of the antecedents, meaning, and aftermath of the American Civil War. Beginning in 1919, he was also involved in organizing and writing

³To include Du Bois in the sociological canon is now commonplace. The question is how to include him? And the related question is, What shall we do with the canon? For some this means replacing the canon with a Du Boisian sociology, (Itzigsohn & Brown 2020); for some it means adding another perspective, pluralizing and thereby diluting the canon (Go 2016); for yet others it means deconstructing the canon to be replaced by an epistemic pluralism, let a hundred flowers bloom (Connell 2007). For me it means reconstruction, which implies the following. First, while canonical thought is, like all social thought, an expression of its times, what makes it canonical is the transcendence of its times. Second, what makes the authors canonical is their engagement with a set of methodological, philosophical, historical, and scientific questions as well as offering original and exemplary empirical research. Third, each canonical figure has profound internal contradictions that drive their thought in different directions. There is no singular Marx, Weber, or Durkheim, just as there is no singular Du Bois. Fourth, reconstructing the canon is not a matter of simply adding Du Bois, but of putting him into a conversation with Marx, Weber, and Durkheim, highlighting their contradictory relations as well as their commonalities. Reconstructing the canon, therefore, necessarily involves rereading each. With the addition of Du Bois, we discover another Marx (Anderson 2010), another Weber (McAuley 2019), and another Durkheim (Fields 2002). The idea of reconstruction may appear conservative but, as we see with Du Bois, it can have radical consequences.



about Pan Africanism that culminated in African independence movements; his engagement with Soviet and Chinese socialism gave him a positive vision of what the world could be, making him an enemy of the US state.

Du Bois's standpoint throughout derived from his personal experiences of marginalization, illuminating not only race but also class and gender oppressions, which he expressed in literary as well as sociological texts. The corpus of his writings calls for a dramatic expansion of the very meaning of social theory. He is an avatar of reflexive sociology that also stimulated a public sociology. The world impressed itself on the way he conducted sociology, even as he sought to impress sociology upon the world.

Faced with his immense contributions and shifting politics, Du Bois scholarship has often forced him into a singular framework. Until recently the recognition of Du Bois in sociology was largely limited to two works: *The Philadelphia Negro* (Du Bois 1899) and *The Souls of Black Folk* (Du Bois 1903). The first is a foundational study in urban sociology; the second is a set of literary essays that explore the experience of racism in the South, including Du Bois's formulation of double consciousness. Morris (2015) brought the early writings to the fore with his book *The Scholar Denied*, celebrating Du Bois as the real founder of US sociology.⁴ While at Atlanta University (1897–1910), Morris shows, Du Bois pursued careful empirical research with colleagues and students long before Robert Park established the Chicago School. Starved of resources, the Atlanta School nonetheless pioneered case studies of African American society, superior to the later Chicago studies influenced by Park's social Darwinism and speculative race cycle theories. Morris attributes the denial of Du Bois's foundational status to the racism of the sociological establishment.

As Ferguson (2015) points out, however, to compare Du Bois to Park is to belittle Du Bois. After all, who now reads Robert Park? To set the record straight on the history of US sociology is important, but not at the cost of overlooking Du Bois's subsequent work. Marginalized in academia, Du Bois entered the public sphere to become a founder of the NAACP (National Association for the Advancement of Colored People), the editor of one of the most influential magazines of the twentieth century, *The Crisis*, an organizer of Pan-African Congresses that attracted future leaders of independent Africa, and a persecuted leader of the peace movement during the Cold War. All the while, his writings expanded in vision to embrace a global sociology. He moved from scholar denied to scholar unbound—the greatest public sociologist of the twentieth century.

If the claim that Du Bois was the founder of US sociology is too modest, then how can we grasp the enormity of his accomplishments? One approach is to insert him squarely in the sociological canon, putting him into dialogue with Marx, Weber, and Durkheim. This is not just a matter of adding another figure; rather, the goal is to explore his relations to the others and how his inclusion disrupts the reading of each. There is a conversation to be had between Du Bois and Durkheim as *The Philadelphia Negro* (Du Bois 1899) diagnoses urban society along lines parallel to those found in Durkheim's *The Division of Labor in Society* [Durkheim 1984 (1893)], specifically the anomic and forced division of labor, but now highlighting the division of labor's racial moment. This conversation continues with the *Souls of Black Folk* (Du Bois 1903), where Du Bois appeals to the shared morality of black and white, stressing the importance of education and religion, and the contributions of African Americans to US culture. Again the parallels to Durkheim are unmistakable—the development of a collective consciousness instilled through education and expressed in religion. Even the celebrated concept of double consciousness has its parallels in the relation between collective and individual consciousness. This is the conservative Du Bois, calling

⁴Wright (2016) similarly celebrates Du Bois and the Atlanta School as the foundation of US sociology.

on the white world to recognize and cultivate the African American talented tenth. At this point Du Bois, like Durkheim, sees the sociologist as the architect of elitist reform. Du Bois breaks with this vision of social change as early as 1909, when he writes the biography of *John Brown* (Du Bois 1909) that celebrates a militant abolitionist's attempt to overthrow slavery by force of arms. The cost of liberty, writes Du Bois, is less than the price of repression.

In bolstering the case for Du Bois as the true founder of US sociology, Morris makes much of Max Weber's respect for Du Bois and his disregard of Park. Weber did meet Du Bois for a few minutes at the St. Louis World's Fair in 1904 and there followed a short correspondence, but as McAuley (2019) argues, it is the brevity that is significant. Weber was certainly interested in Du Bois's study of the legacies of slavery for inequality after Reconstruction as it dovetailed with his own interest in the feudal legacies found in Germany's rural capitalism. Indeed, Weber solicited an article from Du Bois [2006 (1906)] for his journal, *Archiv für Sozialwissenschaft und Sozialpolitik*. But then the communication stops, as do their shared interests. In their explanations of the origins of modern capitalism, the one focuses on the contribution of Protestantism while the other attends to the racial-imperial, world historical element. The radical Du Bois could have little sympathy for Weber's enthusiasm for imperialism. Again, both had to be only too aware of their divergent understandings of the relations between science and politics: the one placing them into watertight compartments, the other demonstrating their inseparability. For Du Bois science shapes politics but, as Muller (2015) points out, no less important, politics shapes science—which leads directly to a consideration of his Marxism.

The last 40 years of Du Bois's intellectual-political life can be seen as an engagement with Marxism, marked early on by his essays in *Darkwater* (Du Bois 1920) that offer a stirring account of imperialism as the cause of World War I, an analysis of the class bases of the race riot at East St. Louis, an argument about the central role of African American women in the abolition movement, and a call for a radical democracy based on popular participation. This phase of his development marks a sharp break from the Durkheimian and Weberian phases and laid the ground for his magnum opus, *Black Reconstruction* (Du Bois 1935), a radical reinterpretation of Reconstruction that centered on the dynamics of class and race and the lost potential for an inter-racial democracy. Inspired by Marx's own writings on the American Civil War, Du Bois offered a daring reinterpretation of American history, an interpretation scorned or ignored by most historians until the revisionism of the 1960s.

Even more controversial, to this day, was Du Bois's embrace of the progressive side of the Soviet Union and China. In their twentieth-century socialism, Du Bois saw the appeal of rational administration through state planning and the equality to which they aspired in sharp contrast to the destructive and exploitative character of US capitalism, the hypocrisy of America's claim to democracy, and the racist original sin upon which it was founded. Inevitably, he was influenced by his persecution at the hands of the US state in contrast to the hero's welcome he received in the Soviet Union and China. He would leave the United States in 1961 to take up an invitation from President Nkrumah to establish a new home in Ghana, waving farewell to the United States by finally joining the Communist Party. Undoubtedly, Du Bois's communist sympathies and his militant antiracism made him persona non grata in the US academy, especially in the conservative 1950s. Can sociology finally accept Du Bois into its pantheon—not just his early Durkheimian uplift and Weberian stratification but his later Marxist radicalism, not just his scientific empiricism but his historical fiction, not just his indictment of a colonial world but his indictment of a racialized academia, not just his scholarship but also his politics? These contradictory elements are inseparable in the thought and legacy of W.E.B. Du Bois.

Conversations between Du Bois and Durkheim, Weber, and Marx reveal the anatomy of Du Bois's successive theoretical frameworks, elevate what is implicit in his scholarship and his politics,



and thereby pose critical challenges to the sociological canon. Bringing Du Bois into that canon requires a reflexive sociology that recognizes not only how the world inhabits us but also how sociology inhabits the world, acknowledging that we are, indeed, actors in the world we study—certainly not as influential as Du Bois was, but actors nonetheless. As the thin line between the academy and politics dissolves, as science and politics intermingle, reflexive and public sociology become one. In his successive autobiographies, Du Bois (1920, 1940, 1968) shows himself to be acutely aware of the historical formation of his theories, shaped by his journey between academia and the public sphere, imprinted by his struggles against Booker T. Washington and Marcus Garvey, his dedication to civil rights, and his support for anticolonial struggles, starting as early as the first Pan-African meeting in 1919. The jury is still out as to whether sociology can embrace such a radical thinker, but Du Bois will always find a home in the tradition of Black Marxism (Robinson 1983) alongside such notable figures as C.L.R. James, Frantz Fanon, and Stuart Hall—a tradition sociology ignores at its peril.

EPILOGUE: LIVING SOCIOLOGY

What, then, is this living sociology? It is a sociology that lives by continually reassessing itself, that grows through focusing on significant anomalies and abiding contradictions within established research programs, that doesn't push exceptions under the carpet but engages them, that wrestles with inconvenient truths. It is a sociology that relishes arguments between opposing perspectives, that seeks out opponents not so much to vanquish them as to strengthen itself, to recognize its own blind spots as well as help others recognize theirs; it is a sociology that is always settling accounts.

Living sociology is an arduous journey up an endless mountain toward an invisible summit covered in clouds, a journey urged on by surprising new vistas that appear at every twist and turn. The journey starts out on the ground, with inherited bodies of knowledge, accumulated by colleagues and parsimoniously packaged into theories, but unpacks and repacks them along the way. It is a journey that is open to ambush by students who have little investment in well-worn paths and who begin to tread their own. We have to nurture them and follow them, or we will be left behind. The educators, too, have to be educated.

Living sociology, therefore, has two interrelated meanings—it refers to a sociology that lives but only through the sociologist living in and for sociology. It is an outlook, a habitus, a common sense dictating the way we confront the world. To live such a life is a privilege that is still permitted in the inner sanctum of the corporate university, not yet suffocated by its increasingly controlling superstructures. As sociologists our autonomy is limited, but there's still enough to enable self-defense, to extend sociology to others in and beyond the university. Our task is to make sociology more open as well as more inclusive, giving substance to the idea that the world does not have to be the way it is, extending the boundaries of the possible by imagining the impossible.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

ACKNOWLEDGMENTS

I thank the editors of the *Annual Review of Sociology* for honoring me with the invitation to write this retrospective article. I am grateful to Wendy Espeland, Elena Ferrante, Mike Levien, Chris Muller, Kathleen Schwartzman, Rachel Sherman, and COVID-19 for helping me see things I didn't see, for helping me clarify what I want to say, and for raising issues that I can't answer.



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