Part Five
Professional Sociology

The limitations of my naïve policy sociology led me to public sociology; the limitations of my public sociology led me to a critical sociology – a sociology based on the premise that the world could be other than it is, while recognizing there are powerful forces thwarting that possibility. But critical sociology also harbors a critical perspective toward consecrated, professional sociology, complacent in its anti-utopian leanings at the cost of utopian imagination, justifying what exists as natural and inevitable. Why then would a Marxist want to become a professional sociologist?

At the time I was in graduate school, there was a renaissance of Marxism within academia. If there were few signs of such Marxism in Chicago's sociology department, in the neighboring political science and anthropology departments there were Marxist tremors. As I have indicated, in other sociology departments such as Berkeley and Madison, Marxism was a going concern, at least among graduate students. Moreover, I had come from the Third World, as it was then called, where Marxism was flourishing; the same was happening in Western Europe, too. I imagined that if and when I got a job I would be able to advance Marxism through research and through teaching. It was not only that sociology seemed to be a discipline ripe for transformation, but the

academic system itself created the spaces for dissident paradigms.

Reflecting on those early years, Erik Wright (1987: 44) later wrote of "visions of glorious paradigm battles with lances drawn and a valiant Marxist knight unseating the bourgeois rival in a dramatic quantitative joust." We both believed that Marxism could stand the test of science. We were politically naïve about the implications of winning such a paradigm battle, thinking it would spontaneously carry over into the world beyond. More immediately, we were sociologically naïve – not appreciating the capacity of the academy to both repress and channel dissent, and how the exigencies of careers can surreptitiously defang radicalism. If we survived would our Marxism survive? It was a risky venture.

Looking back now I'm surprised at how successful we were. In the 1970s and 1980s a cohort of graduate students made its way into tenure-track positions. Marxists were advancing into key departments, especially in the public universities, publishing in flagship journals of the sociology profession as well as creating their own journals. Symptomatic of the times, the *American Journal of Sociology*, one of the two leading professional journals, invited Theda Skocpol and myself to edit a special issue on Marxism (Burawoy and Skocpol 1983). Her instant classic *States and Social Revolutions* (1979) was heavily influenced by Barrington Moore's (1966) brilliant class analysis of different roads to democracy and dictatorship, blazing a trail for young Marxists. While she was deeply ambivalent about Marxism, her early work was inspired by Marxist debates of the time.

The trajectories of this upstart generation were rarely uncontested. Skocpol herself entered a determined, extended, and eventually successful struggle for tenure at Harvard, but not before taking a position at the University of Chicago. I entered the job market in 1975–76. Berkeley had three openings that year, the result of many years of failed hiring, itself due to deep divisions within the

department. The previous year Berkeley had shockingly denied tenure to Jeffrey Paige, winner of the American Sociological Association's best book award for his Marxist-inspired *Agrarian Revolution* (1975). Initially my own application to Berkeley was thrown out – my work was too "ideological."

My job application included a red-baiting letter of "recommendation" from my old benefactor, Edward Shils. He had thought that Chicago professionalism would either straighten me out or cast me out. No such luck. His letter traced my biography from grammar school, where any imagination I may have possessed was snuffed out by cramming in mathematics. Damning with faint praise, he concluded his letter: "Either the security of sectarianism or a juvenile antinomianism seems to have got the better of him. I first noticed the latter in Cambridge. At the time he was an undergraduate and I thought it would pass. Thus far it has not." For such an anti-communist éminence grise as Edward Shils it was especially important to keep Marxism out of the top universities, especially Berkeley, already tainted by a dangerous radicalism.

Rejection by the faculty, however, did not deter Berkeley's graduate students. Led by Erik Wright, still a graduate student at Berkeley but already on his way to Wisconsin, students invited me to visit when I was interviewing at the University of California–Los Angeles. At UCLA the chair had torn up Shils's letter, whereas Berkeley had used it to defame me. As it turned out, my visit to Berkeley became an informal job interview with several sympathetic faculty and students. After I left students mobilized. When the candidates for the urban slot didn't meet expectations, I was moved out of the "comparative" position to become a surrogate urbanist based on my Chicago factory study. Log-rolling ensued and I was offered the job without a formal interview. Had there been a formal interview I have no doubt there would have been enough opposition to veto my candidacy. Needless to say, with today's strict

rules regulating recruitment such hiring manipulation would be impossible.

I became a token Marxist assistant professor in the Berkeley department when Marxism and feminism had become major influences among graduate students. If that was not challenging enough, it was made more daunting by being thrown into a den of warring colleagues. Students had largely given up on the faculty. Constituting their own study groups and courses, they were teaching themselves the latest twists and turns of critical theory. The very best could thrive in such a laissez-faire atmosphere, but many were so disenchanted as to never complete their degrees. My first six years at Berkeley were dogged by a mounting conspiracy to deny me tenure. The battle reached its climax with a series of underhand tactics: stacking committees, unsolicited damning letters from prominent sociologists, and the discrediting of my teaching. Fortunately, Robert Bellah, then chair of the department, incensed by the foul play, resolutely went to bat for me and so did the highest committee in the university. The overkill of my enemies backfired.

The skullduggery suggested, at least, that there was something important at stake within the discipline – a new generation with new paradigms was threatening to displace the old. But it was not reducible to a crude struggle for power – although it often felt that way. It took place on a shared terrain of scientific standards. Erik Wright used the latest statistical models to demonstrate that his vision of class was better equipped to understand changing patterns of inequality than the more conventional models of stratification. I tried to show how industrial sociology was simply asking the wrong question and organization theory had the wrong answer. We used the techniques of sociology to present an alternative vision of US society and a different sociology.

As a professional sociologist I took the criticisms of *Manufacturing Consent* to heart. And there were serious criticisms. One of the most abiding attacks came from

the guardians of generalizability. How could I possibly make any general claims about capitalism based on a single case study of a single plant in a single corporation? I needed to have conducted a number of such case studies to discover a common pattern. I responded to the criticism in two ways. First, drawing on the philosophy and history of science, I advanced a methodology, "the extended case method," that gives priority to theory, so that a single case study can stimulate the extension of theory (Burawoy 2009). Second, on the basis of my case study of Allis-Chalmers I developed the concept of "production regime," that is, the mode of regulating the relationship between capital and labor within the workplace. This was a theoretical intervention within Marxism, proposing that there is a politics at the point of production as well as at the level of the state. Based on secondary sources I showed how production regimes varied between early and advanced capitalism, within and among different advanced capitalist societies, between advanced capitalism and state socialism, and finally what it looked like in the colonial and postcolonial context (Burawoy 1985). This created a research program that others could advance with their own case studies.

If the first criticism was about *generalization*, the second was about *extension*. My critics questioned the seemingly arbitrary way I extended out from my experiences on the shop floor to forces beyond the plant that were shaping those experiences. Specifically, they questioned the imputation that the hegemonic organization of work was a product of capitalism rather than a system of "industrial relations" typical of progressive industrialization. It required, therefore, that I show that production politics were profoundly different within noncapitalist industry. It seemed to me that the most critical comparison was between advanced capitalism and state socialism – actually existing socialism, or, as I liked to call it, "socialism on earth" as opposed to an imaginary socialism in heaven. While it did point to a

distinctive socialist production politics, the material at my disposal on the Soviet order, whether in the Soviet Union or Eastern Europe, was decidedly thin. So during the 1980s I embarked on research into Hungarian factories, again through observant participation, showing the way production politics differed under state socialism, and, indeed, how it contributed to the collapse of state socialism (Burawoy and Lukács 1992). I followed this with a decade of research into the Russian transition from state socialism to capitalism, a transition that had never been seriously imagined within Marxism or sociology. This required me to shift my lens from production to markets, from exploitation to commodification. To frame my arguments I turned to Karl Polanyi's *The Great Transformation* (1944), that was fast becoming a canonical work. The direction of Marxism was shifting once again.