

The Disobedient Generation

SOCIAL THEORISTS IN THE SIXTIES



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drama. It broke all the rules. At the curtain call when the author appeared some of the audience cheered. But some of them yelled obscenities. One irate customer tore his theater seat from its moorings and hurled it onto the stage. It narrowly missed Pirandello's head" (Zeffirelli 2003). Clearly, some members of the audience concluded that the playwright had willfully concealed the very information they were expecting from him.

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Falling into Marxism; Choosing to Stay

Erik Olin Wright received his PhD from the University of California, Berkeley, and has taught at the University of Wisconsin since then. His academic work has been centrally concerned with reconstructing the Marxist tradition of social theory and research in ways that attempt to make it more relevant to contemporary concerns and more cogent as a scientific framework of analysis. His empirical research has focused especially on the changing character of class relations in developed capitalist societies. Since 1992 he has directed the Real Utopias Project, which explores a range of proposals for new institutional designs that embody emancipatory ideals and yet are attentive to issues of pragmatic feasibility. His principle publications include The Politics of Punishment: A Critical Analysis of Prisons in America; Class, Crisis and the State; Classes; Reconstructing Marxism (with Elliott Sober and Andrew Levine); Interrogating Inequality; Class Counts: Comparative Studies in Class Analysis; and Deepening Democracy: Innovations in Empowered Participatory Governance (with Archon Fung). He is married to Marcia Kahn Wright, a clinical psychologist working in community mental health, and has two grown daughters, Jennifer and Rebecca.

I have been in school continuously for more than fifty years: since I entered kindergarten in 1952, there has never been a September when I wasn't beginning a school year. I have never held a nine-to-five job with fixed hours and a boss telling me what to do. In high school, my summers were always spent in various kinds of interesting and engaging activities — traveling home from Australia where my family spent a year (my parents were Fulbright professors at the Uni-

versity of Western Australia); music camp (I played viola); assisting in a lab. And in college, it was much the same: volunteering as a photographer on an archaeological dig in Hawaii; teaching in a high school enrichment program for minority kids; traveling in Europe. The closest thing to an ordinary paying job I ever had was occasionally selling hot dogs at football games in my freshman year in college. What is more, the ivory towers that I have inhabited since the mid-1960s have been located in beautiful physical settings, filled with congenial and interesting colleagues and students, and animated by exciting ideas. This, then, is the first fundamental fact of my life as an academic: I have been extraordinarily lucky and have always lived what can only be considered a life of extreme privilege. Nearly all of the time I am doing what I want to do; what I do gives me a sense of fulfillment and purpose; and I am paid well for doing it.

Here is the second fundamental fact of my academic life: since the early 1970s, my intellectual life has been firmly anchored in the Marxist tradition. The core of my teaching as a professor has centered on communicating the central ideas and debates of contemporary Marxism and allied traditions of emancipatory social theory. The courses I have taught have had names like *Class, State and Ideology: An Introduction to Marxist Sociology*; *Envisioning Real Utopias*; *Marxist Theories of the State*; *Alternative Foundations of Class Analysis*. My energies in institution building have all involved creating and expanding arenas within which radical system-challenging ideas could flourish: creating a graduate program in class analysis and historical change in the Sociology Department at the University of Wisconsin–Madison; establishing the A. E. Havens Center, a research institute for critical scholarship at Wisconsin; organizing an annual conference for activists and academics, now called RadFest, which has been held every year since 1983. And my scholarship has been primarily devoted to reconstructing Marxism as a theoretical framework and research tradition. While the substantive preoccupations of this scholarship have shifted over the past thirty years, its central mission has not.

As in any biography, this pair of facts is the result of a trajectory of circumstances and choices: circumstances that formed me and shaped the range of choices I encountered, and choices that in turn shaped my future circumstances. Some of these choices were made easily, with relatively little weighing of alternatives, sometimes even without much awareness that a choice was actually being made; others were the result of protracted reflection and conscious decision making, sometimes with the explicit understanding that the choice being made would constrain possible choices in the future. Six such junctures of circum-

stance and choice seem especially important to me in shaping the contours of my academic career. The first was posed incrementally in the early 1970s: the choice to identify my work primarily as contributing to Marxism rather than simply using Marxism. The second concerns the choice, made just before graduate school at the University of California, Berkeley, to be a sociologist, rather than some other *ist*. The third was the choice to become what some people describe as multivariate Marxist: to be a Marxist sociologist who engages in grandiose, perhaps overblown, quantitative research. The fourth choice was the choice of which academic department to be in. This choice was acutely posed to me in 1987 when I spent a year as a visiting professor at the University of California, Berkeley. I had been offered a position there, and I had to decide whether I wanted to return to Wisconsin. Returning to Madison was unquestionably a choice that shaped subsequent contexts of choice. The fifth choice has been posed and reposed to me with increasing intensity since the late 1980s: the choice to stay a Marxist in this world of post-Marxisms when many of my intellectual comrades have decided for various good, and sometimes perhaps not so good, reasons to recast their intellectual agenda as being perhaps friendly to, but outside of, the Marxist tradition. Finally, the sixth important choice was to shift my central academic work from the study of class structure to the problem of envisioning real utopias.

To set the stage for this reflection on choice and constraint, I need to give a brief account of the circumstances of my life that brought me into the arena of these choices.

Growing Up

I was born in Berkeley, California, in 1947 while my father, who had received a PhD in psychology before World War II, was in medical school on the GI Bill. When he finished his medical training in 1951, we moved to Lawrence, Kansas, where he became the head of the program in clinical psychology at Kansas University (KU) and a professor of psychiatry in the KU Medical School. Because of antinepotism rules at the time, my mother, who also had a PhD in psychology, was not allowed to be employed at the university, so throughout the 1950s she did research on various research grants. In 1961, when the state law on such things changed, she became a professor of rehabilitation psychology.

Life in my family was intensely intellectual. Dinner table conversation would often revolve around intellectual matters, and my parents were always deeply en-

thusiastic and involved in their children's school projects and intellectual pursuits. My mother would carefully go over term papers with each of us, giving us both editorial advice and substantive suggestions. We were members of the Lawrence Unitarian Fellowship, which was made up of, to a substantial extent, university families. Sunday morning services were basically interdisciplinary seminars on matters of philosophical and social concern; Sunday school was an extended curriculum on world religions. I knew by about age ten that I wanted to be a professor. Both of my parents were academics. Both of my siblings became academics. Both of their spouses are academics. (Only my wife, a clinical psychologist, is not an academic, although her father was a professor.) The only social mobility in my family was interdepartmental. It just felt natural to go into the family business.

Lawrence was a delightful, easy place to grow up. Although Kansas was a politically conservative state, Lawrence was a vibrant, liberal community. My earliest form of political activism centered on religion: I was an active member of a Unitarian youth group called Liberal Religious Youth, and in high school I went out of my way to argue with Bible Belt Christians about their belief in God. The early 1960s also witnessed my earliest engagement with social activism. The civil rights movement came to Lawrence first in the form of an organized boycott of a local segregated swimming pool in the 1950s and then in the form of civil rights rallies in the 1960s. In 1963 I went to the Civil Rights March on Washington and heard Martin Luther King Jr.'s "I have a dream" speech. My earliest sense of politics was that at its core it was about moral questions of social justice, not problems of economic power and interests.

My family, also, was liberal, supporting the civil rights movement and other liberal causes; but while the family culture encouraged an intellectual interest in social and moral concerns, it was not intensely political. We would often talk about values, and the Unitarian Fellowship we attended also stressed humanistic, socially concerned values, but these were mostly framed as matters of individual responsibility and morality not as the grounding of a coherent political challenge to social injustice. My only real exposure to a more radical political perspective came through my maternal grandparents, Russian Jewish immigrants who had come to the United States before World War I and lived near us in Lawrence, and my mother's sister's family in New York. Although I was not aware of this at the time, my grandparents and the New York relatives were Communists. This was never openly talked about, but from time to time I would hear glowing things said about the Soviet Union, socialism would be held out as an ideal, and

America and capitalism would be criticized in emotionally laden ways. My cousins in New York were especially vocal about this, and in the mid-1960s when I became more engaged in political matters, intense political discussions with my New York relatives contributed significantly to anchoring my radical sensibilities.

My interest in social sciences began in earnest in high school. In Lawrence it was easy for academically oriented kids to take courses at the University of Kansas, and in my senior year I took a political science course on American politics. For my term project I decided to do a survey of children's attitudes toward the American presidency and got permission to administer a questionnaire to several hundred students from grades 1–12 in the public schools. I then organized a party with my friends to code the data and produce graphs of how various attitudes changed by age. The most striking finding was that, in response to the question, "Would you like to be President of the United States when you grow up?" there were more girls who said yes than boys through third grade, after which the rate for girls declined dramatically.

By the time I graduated from high school in 1964, I had enough university credits and advanced placement credits to enter KU as a second-semester sophomore, and that is what I had planned to do. Nearly all of my friends were going to KU. It just seemed like the thing to do. A friend of my parents, Karl Heider, gave me, as a Christmas present in my senior year in high school, an application form to Harvard. He was a graduate student at Harvard in anthropology at the time. I filled it out and sent it in. Harvard was the only place to which I applied, not out of inflated self-confidence but because it was the only application I got as a Christmas present. When I eventually was accepted (initially I was on the waiting list), the choice was thus between KU and Harvard. I suppose this was a "choice" since I could have decided to stay at KU. However, it just seemed so obvious; there was no angst, no weighing of alternatives, no thinking about the pros and cons. Thus, going to Harvard in a way just happened.

Like many students who began university in the mid-1960s, my political ideas were rapidly radicalized as the Viet Nam War escalated and began to impinge on our lives. I was not a student leader in activist politics, but I did actively participate in demonstrations, rallies, fasts for peace, and endless political debate. At Harvard I majored in social studies, an intense interdisciplinary social science major centering on the classics of social theory, and in that program I was first exposed to the more abstract theoretical issues that bore on the political concerns of the day: the dynamics of capitalism, the nature of power and domination, the importance of elites in shaping American foreign policy, and the problem of

class and social change. I found all of this intellectually exciting, and wrote numerous term papers on these kinds of macrosociological issues, but these themes did not constitute for me an overriding intellectual preoccupation as an undergraduate. I wrote my senior thesis not on problems of political economy, classes, and the state but on a social psychological theme: the causes and effects of student leaves of absence from universities. I conducted a survey on this problem and analyzed the data using punchcards in order to understand the conditions under which leaves of absence would have a positive or negative impact on the students involved. The thesis was well received, but one would be hard put to find any hint of radical sensibilities in it.

As graduation approached in 1968 I faced a problem confronted by most healthy American males of the time: how to cope with the prospect of being drafted. It was impossible to get a conscientious objector deferment from my draft board in Kansas since I could not prove that I was a long-standing member of a pacifist religious group. I knew people who become expatriates, and others who were prepared to go to jail rather than be drafted. I was unwilling to make either of these sacrifices. Instead, I decided to enroll in a Unitarian seminary—the Starr King School for the Ministry in Berkeley—and thus get a ministerial deferment. I enrolled in the seminary not out of a deep and abiding commitment to the ministry as a possible vocation—that never occurred to me as something I would actually do—but because it was the only way I could think of at the time to keep out of the army in the context of the Viet Nam War. The enrollments at seminaries, especially in Unitarian seminaries, increased dramatically in the late sixties. When I received a scholarship to study history at Balliol College, Oxford, I therefore organized a way to be formally enrolled in the seminary while taking courses at Oxford. I made a point of specializing in the English Puritan Revolution under the tutorship of Christopher Hill so that if the draft board ever questioned this arrangement I could show that I was studying something connected to religion.

After two extraordinary years of wallowing in intellectual pleasures at Oxford, I returned in the fall of 1970 to the United States and entered the Unitarian Seminary in Berkeley. This is when the decisive choices through which my academic identity would be forged began.

Becoming a Marxist: Accountability and Eclecticism

When I entered the seminary I was already quite radicalized intellectually and politically. The general terms of political debate in England were more perme-

ated with Marxian-inspired ideas than was generally the case in the United States. At Oxford, under the stimulating guidance of Steven Lukes, I had read much more thoroughly a range of Marxist work than I had earlier and wrote a series of papers on various Marxist themes, including my first paper on the problem of class. Still, in 1970 I would not have said that the central focus of my scholarly work was the reconstruction of Marxist approaches to understanding social and political questions. That changed in the course of the next few years.

At the seminary I had two crucial formative experiences. First, I initiated and then led a seminar at the Berkeley Graduate Theological Union called Utopia and Revolution. Fifteen or so students from various seminaries participated in the seminar in which we read and energetically debated socialist, Marxist, anarchist, and various strands of utopian literature. This was the first extended academic context in which I was involved where the primary motivation was not simply the scholarly task of clarifying ideas and weighing the intellectual merits of arguments but, rather, sorting out our political vision and thinking about how to connect our concrete activities to a broad agenda of social change. The seminar was an exhilarating experience. Thirty years later I still teach a graduate seminar in the same spirit—Envisioning Real Utopias.

The second critical experience was a year-long internship as a student chaplain at San Quentin Prison. Every week I would drive from Berkeley to the prison north of San Francisco and spend the day in the Protestant chaplain's office talking to prisoners. This was the height of the militant period of the Black Panthers, and many black prisoners in San Quentin were highly politicized. When prisoners would come to me and ask me to pray with them, I would send them to the real chaplain saying that he was better at that. Very quickly it became known among prisoners that I was a sympathetic ear for political discussions, both about the conditions in the prison and about broader issues in American society. Through the prisoners I met, I became involved in an activist organization called the Prison Law Project, which linked radical, mainly black, prisoners with left-wing lawyers and was devoted to challenging prison conditions through litigation and other forms of activism. In the context of my work with the Prison Law Project and my role in the prison, I decided with my friends in the project to write a book about San Quentin, which eventually became published as the *Politics of Punishment* in 1973, about half of which was written by myself, and the rest by prisoners and others connected with the Prison Law Project.

The Politics of Punishment was by far the most ambitious piece of writing I had ever attempted. I remember when the book was finally done saying that my re-

spect for even very bad books had increased since I now knew how much work they entailed. Writing the book was also the first context in which I had to navigate the analytical imperatives of serious scholarly exposition with the political imperatives of popular accessibility and political relevance. I discovered that I could do academic work that was not just fun intellectually but that had moral and political aspirations as well.

In January 1971, the rules of military conscription changed and a lottery replaced the previous system. When the first lottery was conducted, I received a good number—somewhere above 250 as I recall—and since the expectation was that no one with a number above the low 100s would be drafted in 1971, I gave up my seminary student deferment and decided to enter graduate school in sociology.

Although I was formally enrolled as a graduate student in the Berkeley Sociology Department, the real core of my intellectual formation occurred in what might be called the Bay Area Student Run University of Radical Intellectual Thought. Almost from the start, I was heavily involved in a series of organizations and activities that brought radical students together across departments within the University of California and across universities within driving distance:

- I regularly attended a Bay Area-wide political economy seminar loosely linked to the Union for Radical Political Economics that usually met at Stanford in which problems in Marxist political economy were discussed. Over the years, I presented a number of papers in that seminar, including the earliest version of my work on rethinking the concept of class. At one seminar I laid out the problem of the “middle class” in which I described the class location of managers as ambiguous because of the way they combined relational attributes both of workers (they did not own the means of production) and of capitalists (they dominated other employees). Brigit O’Laughlin, an anthropologist at Stanford, suggested that these kinds of locations might better be thought of as contradictory rather than merely ambiguous, and thus the term for my contribution to the analysis of the middle class was born: contradictory locations within class relations.

- I was part of the founding editorial collective of *Kapitalistate*, a journal devoted to debates over Marxist theories of the state organized by the Marxist economist James O’Connor, then at San Jose State University. The collective involved students and unattached intellectuals from all over the San Francisco Bay Area and, through reading and commenting on papers, it linked us to students in Eu-

rope (especially Germany) and other places in the United States (especially Wisconsin). Through my involvement in the journal collective, I read a paper on state theory written by Roger Friedland and Gosta-Esping Anderson, at the time sociology graduate students at the University of Wisconsin, sent them detailed comments, and ended up coauthoring with them the final published version of the paper. Through them I became linked to students at Wisconsin and began to think of the Wisconsin Sociology Department as an exciting place.

- I was heavily involved in founding an organization of socialist-oriented academics called the Union of Marxist Social Scientists, which was organized to increase dialogue among activists and left-oriented academics. Its main activity was an annual conference held each spring at a summer camp called Camp Gold Hollow in the Sierra foothills, which was attended by several hundred people from up and down the West Coast. By the mid-1970s this conference became a politically charged venue in which students, a scattering of faculty, grassroots activists, and militants from various sectarian Marxist-Leninist quasi-parties gathered to debate theoretical and political matters. At the last camp I attended, in the spring of 1976, my work on social class was denounced in a large meeting by members of the League for Proletarian Socialism (a self-styled Maoist group) for reflecting “petty bourgeois socialism.” That annual conference is the direct ancestor of RadFest.

- In order to enable students to get formal academic credit for the kinds of study groups in which we were involved, I convinced a number of faculty members in the Berkeley Sociology Department to act as passive sponsors of a series of student-organized on-going graduate seminars exploring debates in radical theory. One of these—Current Controversies in Marxist Social Science—met continuously for four or five semesters and formed the basis for several courses I subsequently taught when I became a professor.

Through these activities I discovered that there existed an on-going, energetic intellectual tradition in which one could be a radical critic and engage in careful, rigorous, intellectually sophisticated academic work. The attraction was as much intellectual as political. The debates were exciting and demanding. When we read Louis Althusser, Nicos Poulantzas, Elmar Altvater, Perry Anderson, Claus Offe, Antonio Gramsci, Jürgen Habermas, James O’Connor, Barry Hindiss and Paul Hurst, Goran Therborn, and the other writers in the Marxist renaissance we felt we were at the cutting edge of ideas, really learning something important and gaining depth. These texts were usually hard, and it took work to

sort them out, but this also was part of the attraction: we were not doing something easy. There were many people joined together in the effort, and the dialogue created a sense of common purpose and community.

Some people in these circles were deeply involved in self-styled Marxist, Marxist-Leninist, Maoist, or Trotskyist parties, but most were not. Generally, most people in my intellectual circle saw party activists as disruptive, as infusing self-righteous dogmatic styles of argument into theoretical debates. Many of us were or had been activists in specific movements—the antiwar movement, the student movement, the prison rights movement—but above all, this intellectual community was academic: mostly graduate students and a few faculty engaged in the project of forging a new Marxist social science in the university.

That the intellectual anchor of debates in this community was Marxist, there can be no doubt. Still, not everyone who participated in these activities called themselves Marxist. Among radical intellectuals of the early 1970s many people saw their work as drawing from the Marxist tradition or being inspired in various ways by that tradition without defining their central goal as contributing to the reconstruction of Marxism. One can use Marxism without being a Marxist.

Most of what I have published, if you strip away the rhetorical parts that proclaim how the work tries to contribute to Marxism, could almost as well have been written in the softer spirit of having a Marxist inspiration. I could have framed my arguments by saying something like “the Marxist tradition is a rich and interesting source of ideas. We can learn a lot from it. Let’s see where we can go by taking these traditional notions of class and massaging them, changing them, combining them with Weberian and other elements in various ways.” I could have cast my class analysis this way without invoking any commitment to Marxism per se as a tradition worth reconstructing.

Many sociologists in the late 1960s and early 1970s, radical intellectuals of my generation, made that kind of choice. Consider Theda Skocpol’s early work, especially *States and Social Revolutions*. This book could have been written as a Marxist work with no real change in any substantive thesis. It could have been written as a book that was amending and reconstructing certain weaknesses in the Marxist tradition, particularly its inattention to the problem of state capacity and state breakdown, in order to rebuild and strengthen that tradition. Instead she chose, for reasons that she would have to explain in her own set of intellectual and personal coordinates, to treat the book as a dialogue with the Marxist tradition but firmly, rhetorically, outside it. I made the opposite choice. The question is, why did I do this, what was my thinking behind it?

Let me give you a vignette that I think helps to reveal what’s at issue here. In 1986 I gave a talk in Warsaw called “Rethinking Once Again, Alas, the Marxist Concept of Class.” In the talk, I discussed such things as contradictory class locations, exploitation in Soviet-type postcapitalistic society, the role of control over different kinds of assets for constructing new kinds of exploitation, and so on. Afterward, the first question was the following: “Professor Wright, I find your ideas very interesting and very compelling. I think there is a lot to be discussed about them, but why do you call this *Marxist*? Why deflect attention from what you are really talking about by saying that this has anything to do with Marxism?” What is at issue here is a dramatic difference in the contexts for pursuing radical intellectual work. In the Polish context of 1986, to declare that this was a reconstruction of Marxism meant something utterly different from what the same words mean when they are declared in the context of American sociology. In Poland, to reconstruct Marxism in the 1980s was to salvage an ideology of state repression. In the United States, to embed one’s work in a rhetoric of reconstructing Marxism means, in contrast, to declare one’s solidarity with struggles against capitalism, class inequality and oppression.

Thus, I think the first motivation behind the declaration of my work as contributing to Marxism centers on a point in the sociology of knowledge. What does it mean to define one’s work as integral to an oppositional current within an established set of institutions? This is very close to what sociologists talk about when they talk about “reference groups.” What really was at stake to me was the nature of the constituency or audience to whom I wanted to feel accountable. Whose criticisms did I want to worry about, and whose did I want to simply be able to dismiss?

These psychological issues are an important part of what is at stake in making the choice to see my work as embedded in the Marxist tradition, as contributing to the reconstruction of that tradition rather than simply drawing on it. Defining my work this way establishes to whom I am accountable, whose opinions are going to matter. The issue of reference group, however, is not just psychological, since reference groups are also social networks that dispose of real resources and impose real pressures of various kinds. Choosing a reference group, then, has the effect of creating a set of constraints that one faces in the future.

In the decision to describe my work as contributing to Marxism, then, there is a kind of Ulysses and the Siren story at work (to use a metaphor elaborated by Jon Elster). It is an attempt, however imperfect, at blocking certain pressures of cooptation that one experiences once one enters a profession. It is an attempt to

make life more difficult for oneself. The same holds true for feminist sociologists today. Some feminists say that their work is contributing to feminism as such. Rather than just contributing to sociology inspired by feminism, they see their work as contributing to building feminist theory. Such declarations make life more difficult, since you could say most of the same things without framing your agenda in this more provocative manner. Making one's life more difficult in this way, however, is not a sign of masochism; it is a strategy that makes it harder to slide inadvertently into a theoretical and intellectual practice that is overwhelmed by its professional acceptability. The pressures for mild, nonconfrontational, acceptable scholarship are enormous, and situating one's work firmly in a radical oppositional current is one way of partially neutralizing those pressures.

There is another side to the choice to contribute to building Marxism as an intellectual tradition rather than simply using it that entered my own decisions and that has become increasingly important in my subsequent on-going decision to stay in Marxism rather than to become, as is more fashionable these days, post-Marxist. This second aspect of the choice raises issues in philosophy of science rather than sociology of knowledge. What is the best way to contribute to the enhancement of our knowledge of social life? Is the most productive strategy to work within what one considers the best available paradigm, or is it better to take a more eclectic approach, avoiding any strong commitment to a single perspective but instead picking and choosing from different traditions as is appropriate for different particular questions one might ask? In a somewhat overstylized way we can contrast two stances toward these issues: a stance that places great value on ambitious programs for theoretical coherence and integration in the form of a sustained paradigm and a stance, sometimes referred to as a more empiricist approach, that argues that what we want to do is deeply and intensively describe the world while eclectically drawing from different sorts of ideas as we see fit for different problems.

My view on this contrast of intellectual practices is not the conventional one for someone who is committed to a paradigmatic view of knowledge in his own work. Most people who are committed to some kind of effort at building strong paradigms are antieclectic: eclecticism is viewed as the enemy of paradigm building. I believe, to the contrary, that there is a constructive symbiotic relationship between paradigm-mongers and carefree eclectics. The optimal intellectual terrain for radical theory—or for any sociological knowledge, for that matter—is a mixture of people who are committed eclectics and people who are committed

paradigmists. If I could snap my fingers and make every radical intellectual a committed Marxist, I wouldn't do it. I think it would be bad for Marxism and certainly bad for the Left. If I could snap my fingers and make everybody a committed eclectic, if that's not an oxymoron, I would also not do it. Eclecticism is in a certain sense parasitic on committed paradigms. To be an effective eclectic, there must be other scholars around who are worrying obsessively about how to rebuild paradigms and maintain the maximum coherence possible within them. But if that's what everyone did, it would be a constraint on the possibility of effectively reconstructing paradigms because the puzzles and worries and anomalies that a reconstructive project faces often come from the insights generated by the eclectics.

The environment of intellectual work that I see as optimal, and that I try to achieve to the extent possible in the intellectual settings within which I work, thus values an intellectual pluralism in which no one is holier-than-thou about metatheoretical principles. Dialogue between the doubts of the eclectics and the commitments of the paradigmists strengthen both. These issues hold for contemporary feminism as well as Marxism. In the feminist tradition radical feminism is crucial for healthy feminism, even though I think radical feminism is not the most plausible version of feminism. Still, it would be a shame for the feminist tradition if radical feminists were somehow persuaded to abandon the most radical and extreme forms of feminism. Similarly for the socialist tradition of intellectual work, it is important to have a body of scholarship and intellectual work that remains committed to rebuilding rather than simply drawing from the Marxist tradition.

Becoming a Sociologist: Fuzzy Disciplines and Intellectual Pluralism

The second choice in the early 1970s that helped forge my academic identity was the fateful decision to become a sociologist. When I entered sociology, I saw it more as a platform on which to do my work than as a discipline to which I felt any commitment as such (although I have to admit that over time my sense of loyalty to the field has grown considerably). As an undergraduate I majored in an interdisciplinary social science program (social studies), after which I studied history for two years at Oxford. I currently participate actively in an academic network sponsored by the MacArthur Foundation in which most participants are economists, and since 1975 I have been on the editorial board of the journal *Pol-*

itics and Society, which has stronger roots in political science than in sociology. I see myself as a social scientist and social theorist rather than a capital S Sociologist. Why, then, did I choose sociology as an academic home?

Of all the social sciences, sociology seemed to me to be the least disciplinary; it had the fuzziest boundaries. But even more significantly, sociology has valued its own marginal traditions in a way that other social sciences don't. Even anti-Marxist sociologists recognize the importance of Marx as one of the intellectual founders of what has become sociology. All graduate courses in theory contain at least some reading of Marx. There are economics departments in which the name Marx would never be mentioned. The only social science discipline that might have served as well as sociology was political science, and I suppose if I had been at some other university I might have become a political scientist. But at Berkeley I felt that sociology was a more congenial place in which to be a radical, and in general I now think political science tends to be somewhat less hospitable to radicalism because of the tight relationship between political science and the state. Political science is a breeding place for government advisers and policy analysts, and that aspect of political science as a discipline would be a constraint that I did not want to choose. So, I chose sociology.

Becoming a Multivariate Marxist: Legitimizing Marxism and Careerism

Very quickly in graduate school, even in a place like Berkeley, it becomes clear where the intellectual core of sociology as a discipline lies. Having decided to be a sociologist and having as a mission the reconstruction of Marxism as social science, I saw a crucial task of my work as trying to increase the credibility of Marxism within the academy, and I felt that quantitative research was a good way to accomplish this. As I wrote in 1987, reflecting on my early theoretical ambitions: "I originally had visions of glorious paradigm battles, with lances drawn and the valiant Marxist knight unseating the bourgeois rival in a dramatic quantitative joust. What is more, the fantasy saw the vanquished admitting defeat and changing horses as a result"

My decision to launch a series of projects involving large-scale data gathering and sophisticated statistical analysis was not driven by any epistemological conviction that these techniques generated deeper insights or more reliable knowledge. Indeed, on that score I have generally found that I learn more from good qualitative and historical research than from quantitative research. But I felt that, at that point in the history of Marxism in sociology (the mid-1970s), estab-

lishing the credibility of Marxism using a quantitative methodology had the greatest chance of making a difference in the intellectual space Marxists could occupy within the academy. I also just like playing with numbers and was pretty good at it.

This decision to pursue quantitative research was also bound up with particular personal relations in graduate school. My closest friend at Berkeley was an Italian student, Luca Perrone. Luca was a sophisticated European intellectual, at ease with the various theoretical currents of left-wing thought, but also enthusiastic about quantitative research. He was the perfect kindred spirit with whom to forge a quantitatively oriented Marxist research program. My first publication engaged with Marxism was written with Luca, a long theoretical essay published in Italian in 1973 comparing the conception of the state and politics in the work of Talcott Parsons and Nicos Poulantzas, and subsequently, my first quantitative publications in class analysis, including my first *American Sociological Review* (ASR) article in 1977, were also written jointly with him. As we approached the end of our time together in Berkeley we wanted to concoct a long-term project that would enable us to continue working together—a project that would bring me regularly to Europe and Luca to the United States. A large well-funded cross-national quantitative study on social class seemed a good way to do this. Tragically, Luca died in a skydiving accident in 1981 and, thus, did not live to see the results of our early collaboration.

To be honest, there was also, from the start, a darker side to the appeal of quantitative research. All academic disciplines, as institutions, contain a system of rewards and sanctions that channels work in particular directions, and there were clearly more resources to be had through quantitative research. I was very ambitious as a young scholar—ambitious in my search for what I considered to be the "truth," but also ambitious for status, recognition, influence, world travel. Embarking on a line of research anchored in conventional survey research thus offered tangible rewards.

I cannot reconstruct exactly what the balance of these motives were in the mid-1970s when I did my dissertation research, a quantitative study of class structure and income determination, or in the late 1970s when I began my twenty-year comparative project on class structure and class consciousness. But whatever the balance between grantsmanship and intellectual purpose, the choice to direct my research in this way was enormously consequential, and not always in ways to my liking. It resulted in a narrowing of askable questions and a divergence between much of my best theoretical work and my empirical research. Originally,

the idea in 1978 when I began the comparative class-analysis project was to do a survey of class structure and class consciousness in the United States, Italy, and Sweden. This was meant to be a brush-cleaning operation: settling and clarifying a range of empirical issues before returning to the problems I cared about the most—the state, politics, social change. But quickly the project expanded as scholars in various other countries asked to join the research, leading eventually to surveys more or less replicated in more than a dozen countries. This enlarged scale of the enterprise created a set of expectations and commitments that could not be easily (or responsibly) abandoned, and yet the work did not always yield intellectual insights in proportion to the time and resources the project absorbed.

Choosing a Department: Professional versus Intellectual Sociology

I initially went to the University of Wisconsin without a great deal of thought and deliberation. Through my involvement in *Kapitalistate*, I had made friends with a number of graduate students there, and through them various faculty in the department became aware of my work even before I was on the job market. In 1975, I was asked by the department to apply for an assistant professorship and was quickly offered a job even before I went for an interview, so I never really went on a national job search to explore all options. In 1987, however, I was offered a job at the University of California in Berkeley and spent a year there “trying it out.” By the spring of 1988, I was clearly faced with a genuine, unmistakable choice, a choice laden with “road not taken” potentials.

Here is how, at the time, I characterized the big difference between these two departments in the late 1980s. If you think of the famous people in the Berkeley department what comes to mind are titles of books: *TVA and the Grass Roots*, *Alienation and Freedom*, *Habits of the Heart*, *Mothering*. When you think of the famous people in the Wisconsin department what comes to mind is the journals in which they publish, the topics that they pursue, the datasets they have developed: the *ASR* and *American Journal of Sociology*, mobility and status attainment, the Wisconsin Longitudinal Study, log-linear analysis. Wisconsin was an article-writing department and Berkeley a book-writing department.

This contrast between the two departments is also reflected in the nature of their graduate programs: at Wisconsin a significant number of graduate students write dissertations that are spin-offs in one way or another from large, on-going research projects. The model of education is that of an apprenticeship, and while

students are expected to do original and innovative work, many do so within the context of some professor's research shop. At Berkeley, it is quite rare for students to play this apprenticeship role. Students are expected to be autonomous intellectuals; dissertations are supposed to be first drafts of books; it is rare that dissertations are in any direct way derivative from the data and projects of their advisers.

In agonizing about the choice of where to be, I stylized the contrast between these two settings by saying that Berkeley was one of the leading intellectual departments in which I would be on the discipline-oriented wing, whereas Wisconsin was one of the leading discipline-oriented departments in which I would be on the intellectual wing. Which of these settings, I thought, do I want to be in? Which would provide the most creative context for my future work? The irony was that although I actually found the intellectual climate of Berkeley more comfortable in many ways than that of Wisconsin, I felt that I would be more challenged and pushed in more interesting ways if I was more an intellectual maverick in a disciplinary department than a disciplinary maverick in an intellectualized one. I felt that at that point in history and at that point in my life, perhaps, the creative tension would be more constructive in Madison. At Berkeley I would be constantly contending with postmodernist currents that argued for the centrality of culture for everything and the impossibility of explaining anything. In Madison I would be arguing for the importance of an open and dialectical perspective on the relationship between social change and social action and the need for unconventional voices in sociology. So, I returned to Wisconsin, although I have retained close ties to Berkeley and frequently return to give talks.

In the years since that choice, the two departments have converged somewhat. I recently did a ministudy of dissertations done at Wisconsin and Berkeley since the 1960s in order to better characterize the two departments. Berkeley has been fairly consistent over the entire period: 75–90 percent of dissertations in each decade used qualitative methods. At Wisconsin there has been a sharp change: from the 1960s through the 1980s, roughly 70–80 percent of dissertations were quantitative. In the 1990s this dropped to just over 50 percent. This methodological shift in dissertation research reflects a change in the composition of faculty and, more broadly, in the intellectual culture of the Wisconsin department.

Staying a Marxist

When I became politically radicalized and first began my intellectual work in the late 1960s, Marxism really was the only game in town: if you were a serious

intellectual and really wanted to develop theoretical groundings for radical critique of the status quo, in some way or another you had to find a home in or make peace with the Marxist tradition, whether or not you then used the label as a self-designation. Marxist theorizing was at the cutting edge of sophisticated intellectual debate, and, while Marxism never became part of the academic mainstream, there was a certain intellectual cachet in calling oneself a Marxist within the academy. In sociology, Marxism was treated as a real rival to more mainstream traditions, so even though most sociologists disagreed with me, I felt that my ideas were taken seriously.

Beginning in the mid-1980s and accelerating in the 1990s, Marxism became increasingly marginal to academic life and intellectual debate. It is not that Marxist ideas have disappeared — many in fact have become absorbed into the mainstream — but rather that Marxism as an intellectual terrain is no longer the site of wide-ranging, energetic, innovative theoretical work. Particularly since the “fall of Communism,” to many people Marxism now seems an archaic discourse, and discussions of exploitation, class struggle, revolution, and socialism seem faintly ridiculous rather than hard-edged, nuanced challenges to the status quo. Many radical intellectuals who, in the early 1980s, firmly identified their own work with Marxism now no longer do so. They have not necessarily become self-described ex-Marxists and certainly not rabid anti-Marxists — as happened in the 1950s when the exit from Marxism was deeply bound up with anti-Communism — but they do longer see the reconstruction of Marxism as a pressing, or even relevant, task.

I have remained stubbornly working inside of Marxism and continue to work for the reconstruction rather than abandonment of this intellectual tradition. I do so, above all, because I continue to believe that many of the core ideas of this tradition are indispensable for any project of emancipatory social change. Specifically, the diagnosis of capitalism as a system of oppression built around class and exploitation, and the normative vision of a radically egalitarian democratic alternative to capitalism, are fundamental insights integral to Marxism. While I no longer see Marxism as a comprehensive theoretical paradigm capable of constituting a general theory of history and society, I still believe that the Marxist tradition contains a coherent framework of ideas that can provide a solid grounding for a socially engaged research program.

I have not, however, pursued this goal simply as an individual project of my own. To sustain these commitments and the hope to accomplish these goals requires embedding oneself in a particular set of social networks, a particular circle of people whose work one reads, with whom one discusses issues, and whose

judgments matter. A reference group is not just an impersonal audience defined by some social category; it is also a circle of people with names and addresses who constitute the active, ongoing basis for the intellectual interactions and support that spur one's own intellectual development.

In my case, there are two such concrete reference groups that have anchored my work since the 1970s. The first is a group of scholars that was at the core of an intellectual current known as analytical Marxism in the 1980s. The group has a less high-blown name that it gave to itself: the NBSMG — the No-Bullshit Marxism Group. The NBSMG is a group of a dozen or so philosophers, economists, sociologists, political scientists, and historians from five countries that has met every September in London, Oxford, or New York for a three-day conference from 1979 to 2000 (and, since then, once every two years). Many of the names associated with the NBSMG over the past two decades are relatively familiar — Pranhab Bardhan, Sam Bowles, Robert Brenner, G. A. Cohen, Josh Cohen, Jon Elster, Adam Przeworski, John Roemer, Hillel Steiner, Robert van der Veen, and Philippe van Parijs.

The term “analytical” in analytical Marxism reflects its central intellectual style: bringing the concern with conceptual precision, clarity, and rigor that is characteristic of analytical philosophy to bear on Marxian themes. Substantively, the central mission of the group was initially to explore systematically the theoretical and normative foundations of a series of pivotal Marxian ideas: exploitation, class, the theory of history, economic crisis. Subsequently, the preoccupations became less narrowly focused on Marxist concepts and more broadly directed toward the normative concerns with equality and social justice.

The group was initially formed around discussions of G. A. Cohen's extraordinary book *Karl Marx's Theory of History: A Defense* (Princeton, NJ: Princeton University Press, 1978). I read this book in the summer of 1979 (while in the process of adopting a baby in Costa Rica) and was completely blown away by it. This book is by far the most rigorous and profound book on Marx's work that I have ever read, and certainly the book that has most influenced the way I think about Marxism. I wrote a long review essay of the book with Andrew Levine that was published in *New Left Review* in 1980. Cohen read it, and invited me to attend the 1981 NBSMG meeting. I was invited back in 1982 and have been a member of the group since then.

For the first fifteen years or so, the group met in the same room every year and ate at the same restaurants. Mostly, we only saw each other during this three-day period. For me it was like a little chunk of the year snipped out, reserved for this

special world. I had the rest of the year, then the three-day no bullshit meeting in London.

Most years, of the ten or eleven people who attended a meeting about half presented papers. These got distributed five or six weeks in advance and were generally read quite carefully by participants. At the meeting itself, someone other than the author would introduce and comment on a given paper. Roughly an hour and a half or so would be spent demolishing/discussing the paper in a no-holds-barred manner. The intellectual style was intense and analytically exhausting. To an outsider, many of the discussions might seem destructive, but I think that impression would be mistaken. The interactions involved a particular form of intellectual aggressiveness that is not inherently invalidating; the very act of taking each other's work so seriously is itself an affirmation of respect and support. An outsider wouldn't really see this. Many people looking at this behavior would think this was a gladiatorial combat in which death was the only possible outcome. But from the inside it can be an enormously exciting setting for coming to terms with the subtle problems and gaps in one's ideas and gaining insights about the inner workings of other people's work.

The group is, as one might predict, all men. We have had discussions in the group from time to time about gender issues, both as a topic—I presented a paper on Marxism and feminism at one meeting—and as an issue in the group's composition. For better or worse, nobody in the group knew well any women scholars who both shared an interest in the substantive topics about which we were concerned and engaged those topics in the intellectual style that marked the group. It was probably also the case, I suppose, that many members of the group felt that the kind of intensity of the group would be harder to sustain if it was gender mixed. In any event, no women have been recruited as members of the “club,” although several have been invited to attend at various times. In these terms the NBSMG raises important, and troubling, issues in the sociology of gender. Networks of this sort are crucial sites where productive intellectual development occurs, where ideas are forged and refined. While the NBSMG does not control any financial resources—it gives no grants and everyone always pays for his own travel and expenses—nevertheless as a vigorous interpersonal network of intellectual exchange, it is influential and valuable. Undoubtedly the gender composition of the network both reflects the historically marginalized role of women intellectuals in the Marxist tradition and contributed in some way to sustaining such gender inequality.

From the early 1980s to the late 1990s, the NBSMG was the organized refer-

ence group that mattered most to me. When I wrote papers in that period, the ghosts who sat in the back of my room and periodically jumped up to tell me that what I had written was ridiculous, and made me worry about whether I got it right, were mainly from this group (and some kindred spirits to this group). The group has unquestionably given my work a particular direction and cast because I have to worry, by virtue of this reference group, about certain issues while others seem less pressing.

Gradually, in the course of the 1990s, the intellectual agendas and theoretical commitments of many of the members of the group changed. Two participants—Jon Elster and Adam Przeworski—decided to leave the group, feeling that in the context of busy schedules it no longer served their needs in a useful way. A number of others felt that while the normative issues at the core of group, especially a radical egalitarian stance toward issues of social justice, remained central to their work, the specific preoccupation with Marxism as a source of ideas and debates for advancing that normative agenda was no longer so important. By the year 2000, several people in the group expressed the sentiment that perhaps it was time to end the annual gathering, but we voted to continue, as much because of the value we all placed on the fellowship and durability of the group as on its intellectual pay-offs. The 2001 meeting was scheduled for New York in mid-September but had to be cancelled because of the 9/11 attacks. When we met the following year, September 2002, we decided to move to an every-other-year cycle. At the moment, it is uncertain whether this is simply a gentle way of incrementally ending the group or whether it will continue in a less energetic way. In any event, the drift in its intellectual priorities and the decline in its intensity have reduced its role as an anchor for my academic work.

My second reference “group” has, if anything, increased in salience over time. It consists of a single person, Michael Burawoy, a professor of sociology at Berkeley. Michael and I have read nearly every page that either of us has written in the past twenty-five years or so. He is constantly reminding me not to lose sight of the ultimate point of it all by becoming preoccupied with analytical rigor at the expense of political relevance; I am constantly telling him to be more precise in his formulations, to be clearer about the underlying logic of the conceptual distinctions he makes. Our intellectual styles are quite at odds with one another in many ways. He does ethnographic research of an extraordinary fine-grained character; my research has been quantitative, typically obliterating much of the nuance and texture of the subjects I study. He is generally skeptical of claims about “objective” truth; I have generally defended rather conventional philosophical

views of the scientific aspirations of Marxism and sociology. We have discussed these issues and their bearing on our respective work while walking my dog in the woods, biking the hills of Marin County, and looking for open restaurants in Moscow. In the late 1980s, this dialogue took the form of a series of published exchanges between the two of us in the 1987 and 1989 issues of the *Berkeley Journal of Sociology*. (The first of these exchanges is reprinted in my 1990 book, *The Debate on Classes*; the second appears as chap. 9 in *Interrogating Inequality*). Subsequently, we coauthored a number of papers, most recently “Sociological Marxism” in the *Handbook of Sociological Theory*. As of 2003, we began the process of trying to write a book together based on this paper. The idea is to reflect on the past twenty-five years or so of empirical research and theoretical development within Marxist-inspired social science and identify what we feel to be its enduring, robust core. Our hope is to elaborate a distinctive sociological Marxism around this core. The particular way in which personal loyalty and closeness is combined with intellectual difference in our relationship has been for me a vital source of intellectual challenge and encouragement. It is also, surely, at least part of the personal dimension of “staying” Marxist.

Envisioning Real Utopias

In my work with Burawoy, we have identified the robust core of the Marxist tradition as consisting of two theoretical clusters: first, a diagnosis of capitalism, both of the ways it imposes harms on people and of its logic of development and reproduction; and second, an account of the possibilities of a radically democratic, egalitarian alternative to capitalism. Class analysis pervades both of these: the analysis of class and exploitation is central to understanding how capitalism works, and the transformation of class relations is central to understanding a future beyond capitalism.

For two decades, from the mid-1970s until the mid-1990s, most of my scholarly work was dominated by the first of these theoretical clusters, above all, by the problem of strengthening the Marxist concept of class as a tool for studying capitalist societies. Except for occasional essays, I had given relatively little attention to the problem of emancipatory alternatives to capitalism. It now seems urgent to grapple with this issue. With the end of the cold war and the rise of capitalist triumphalism, this second theoretical cluster of the Marxist tradition has lost much of its credibility even among critics of capitalist society. For all of their oppressive flaws, the existence of the statist economies of the USSR and elsewhere

were a practical demonstration that alternatives to capitalism were possible. Marxist critics of those societies could then make a plausible argument that what these societies needed to become socialist was a radical democratic transformation. By the early 1990s those arguments no longer seemed credible to most people.

In this historical context, as my work in the Comparative Class Analysis Project was winding down in the middle-1990s and I faced the question of what research to pursue next, I decided to embark on what has since become the Real Utopias Project. The project directly grew out of my interactions with my closest colleague at Wisconsin, Joel Rogers. Joel is deeply engaged in both the theoretical and practical problems of progressive policy reform, ranging from issues of reinvigorating democratic institutions (he was the central founder of the New Party in the 1980s) to the problem of creating new labor market institutions that advance both economic equality and productive efficiency. He coined the expression “high road capitalism” to describe this endeavor and characterizes the strategy of reform as “paving the high road and closing off the low road.” I wanted a project that would be relevant to this kind of pragmatic concern with change within the limits of existing possibilities while also advancing the traditional Marxist concern with understanding alternatives outside of those limits. I initially called this endeavor “society by design” but felt a bit squeamish about the elitest social engineering tone of the expression. On a Sunday morning dog walk together (which we have done nearly every Sunday when both of us are in town since the late 1980s), Joel suggested that I call this enterprise “designing realistic utopias.” Soon this became the Real Utopias Project. As in many intellectual enterprises, getting the brand name right helped a lot in giving the project greater coherence and focus.

The idea of the project is to investigate systematic proposals that attempt both to embody emancipatory values and to take seriously the problem of institutional feasibility. The project is organized around a series of international conferences at which specific proposals are elaborated and debated. Each conference has resulted in the publication of a book containing the proposal and a range of the commentaries. The first of these books, published in 1995, revolved around work by Joel Rogers and Joshua Cohen on the problem of associative democracy. Subsequent books have dealt with market socialism (John Roemer, 1996), asset redistribution within capitalist markets (Sam Bowles and Herbert Gintis, 1999), empowered participatory governance (Archon Fung and Erik Olin Wright, 2003), and universal basic income and stakeholder grants (Bruce Ackerman, Ann Alstott, and Philippe van Parijs, 2004).

My academic career embodies a series of deep, probably unresolvable tensions: tensions between radical egalitarian values and elite academic professionalism; between the commitment to Marxism as a vibrant intellectual and political tradition and the fear of being trapped in indefensible, outmoded assumptions; between being relevant to real struggles and devoting my energies to refinements of abstract concepts. These tensions are impossible to escape, at least for me, but I hope in the end that they have been creative tensions that have pushed my ideas forward and kept me from sliding into comfortable complacency.

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