

of regulation was accompanied by a series of self-interpretations. Some of the mainstays of stable Fordism were broadcast constantly to the denizens of the Fordist metropolises as the American Way of Life. These included economic stabilization through demand management, the homogenization of consumer tastes, labor peace, and a steady increase in standards of living, with wages pegged to the cost of living—a system introduced in the General Motors–United Auto Workers contract of 1950 that was termed the Treaty of Detroit by sociologist Daniel Bell in *Fortune* and in the Ford contract of 1955 that gave the UAW a modified version of Walter Reuther's demand for an annual wage (Bell 1950; Lichtenstein 1995, 280–85). When positivists pointed out the connections between existing social patterns and their preferred manner of studying society, reality seemed to ratify their approach.

One result of this conjuncture was the solid implantation of methodological positivism as doxa in the sociological discipline. U.S. sociology's own view of itself followed a narrative of steady progress from social meliorist beginnings toward scientific maturity. In the main historical treatment of the field from the 1950s, Hinkle and Hinkle (1954, 22) described a field becoming ever more focused on “scientific method,” which they defined as the quest to discover laws of behavior and a “preference for concrete, empirical work.” Despite differences of taste or viewpoint, most of the players in the field recognized common stakes and definitions of field-specific cultural capital. Reputational, social, and economic capital in sociology tended to accrue to more positivist, empiricist, and scientific positions. Fluency in these idioms began to function as a form of scientific prestige. Even those who disagreed with positivism tended to collude in its dominance. If they refused, they were channeled into less rigidly positivist fields or into poorly regarded sociology departments. Sociology at last became a well-structured field. By returning to this period we may begin to understand some of the ways in which contemporary sociology is still haunted by the specters of this somewhat remote past.

Acknowledgments

Thanks to Craig Calhoun, Christian Topalov, Immanuel Wallerstein, and Jonathan VanAntwerpen for their comments on drafts of this chapter.

[TEN] Orthodoxy, Heterodoxy, and Hierarchy: “Mainstream” Sociology and Its Challengers

Craig Calhoun & Jonathan VanAntwerpen

What is needed is a new and heightened self-awareness among sociologists, which would lead them to ask the same kinds of questions about themselves as they do about taxicab drivers or doctors, and to answer them in the same ways. Above all, this means that we must acquire the ingrained habit of viewing our own beliefs as we would those held by others.

ALVIN GOULDNER, *The Coming Crisis of Western Sociology*

The social sciences deal with pre-named, pre-classified realities which bear proper nouns and common nouns, titles, signs and acronyms. At the risk of unwittingly assuming responsibility for the acts of constitution of whose logic and necessity they are unaware, the social sciences must take as their object of study the social operations of *naming* and the rites of institution through which they are accomplished.

PIERRE BOURDIEU, *Language and Symbolic Power*

Sometime around 1970, sociologists began to refer to an illusive phenomenon called mainstream sociology. Most of those who used the term at first saw this mainstream as a hegemonic force that oppressed them personally and blocked desirable paths for sociology's development. The mainstream was typified by the “ASA leadership,” by the *American Sociological Review*, and by a few elite departments like those at Harvard and Columbia. It was more East Coast than West Coast, and indeed some major and influential West Coast sociology departments—like Berkeley's—derived a certain glamour from appearing more insurgent than mainstream. Yet the battle between the alleged mainstream and its critics was also played out within departments, and as much within the Berkeley department as any other. It was a battle of graduate students against faculty, junior against senior, sometimes women and people of color against the evidently white, male mainstream. It was also a battle of theoretical and methodological perspectives, though one has to recover a good bit of context and trajectory to grasp why the alignments took the shape they did.

On the insurgent side, Marxists lined up with ethnomethodologists, symbolic interactionists joined heterodox Weberians, and feminists found common cause with fieldworkers. In the alleged mainstream, survey researchers swam with mathematical modelers, modernization theorists treaded water with organizational theorists, policy researchers probed the same shallow waters as demographers, and functionalists dominated all and claimed to knit everything together in a common theoretical framework. Or so, at least, the mainstream looked to those outside it. But this was only a bit less of an oversimplification than saying that those in the mainstream had grey hair and wore suits.

The sides were drawn in a particular moment, one charged with the energy of large-scale social movements—civil rights, antiwar, women's, environmental—and fueled by the demographic momentum of the baby boom and the expansion of the university system. The sides had a certain elective affinity with long-running distinctions, such as quantitative versus qualitative research, but didn't map neatly onto them. Indeed, such crude distinctions mask internal fissures, such as the extent to which new developments in quantitative methods during the 1960s—path analysis and LISREL, for example—could make scions of the previously dominant Lazarsfeldian version of quantitative sociology feel suddenly outside a new mainstream, where the fastest-flowing channels were carved by analytic statistics rather than data-gathering techniques, and even quantifiers were assigned to the backwaters if they weren't causal modelers or minimally users of multiple regression techniques.¹

The odd lines of opposition had perhaps their greatest influence not as they were staked out in the late 1960s and early 1970s, but in the decade or two after that. For if "mainstream" had been an epithet tendentiously hurled by radicals in the earlier period, it was a positive virtue claimed no less tendentiously by those who consolidated their influence in the wake of the upheavals. During the difficult decades from the late 1970s through the early 1990s, when jobs were scarce and tenure cases especially likely to be both contentious and terminal for the candidates, being "mainstream" became a mark of merit. It was in fact not under the Old Regime but after the failed Revolution that it mattered most who was judged to be part of the mainstream.² This was so, not least, because there were stakes—jobs, and

1. On how dramatically quantitative methods changed in the 1960s, see Raftery (2005).

2. It was in the late 1970s and early 1980s, for example, that tenure was famously denied to a number of left-leaning sociologists who had won major prizes for their work—like Jeffrey Paige at Berkeley and Theda Skocpol and Paul Starr at Harvard. The attempt to assert the authority of the mainstream after a hundred flowers bloomed required more repressive measures

NSF grants, and acceptances from the *ASR*—and because those stakes were much more inequitably distributed. In the 1960s, the expansion of the university system ensured an ever-wider availability of jobs. From the mid-1970s, not only were there fewer new jobs but there were more graduate students finishing degrees and there was a growing "backlog" of those who were un- or underemployed. Demands for pre-employment publications shot up, and it became common for those considering tenure cases to insist on achievements far different from those they had earlier been obliged to demonstrate themselves. It was when sociology contracted that it most clearly revealed itself to be a field that differentially distributed rewards and resources in accord with a dominant ideological self-understanding or set of values.

It is not that the discipline did not absorb any of the old radicals; on the contrary, many were tenured, though few in the most elite departments. And the discipline also made room for Marx in the canon of classical theory and for new emphases on race, class, and gender in the curriculum. The new pattern incorporated heterodoxy but further normalized hierarchy. No sociological theory could claim to knit together the concerns of the field as a whole or even to provide a common vocabulary. Subfields proliferated, many founding their own journals and ASA sections. But this is just the point. When sociology contracted, a long-standing differentiation between the undergraduate—and even to a considerable extent the graduate—curriculum and the typical contents of the *ASR* became much more acute. Sociology's majority—employed mostly in teaching positions, sometimes in "applied" research or practice but mainly outside the elite research universities—began to look more and more different from its elite.³ And the resources and rewards that flowed differentially to the elite began to diverge more sharply from those available to the rest—a trend that continues into the present. Harvard faculty always had higher salaries, more internal research support, more graduate students, and lighter teaching loads than

than were in place before—though as sociologists have long observed, resort to actual force often reveals a weakness in authority. It is worth noting that at the time that Paige and Skocpol were denied tenure, they were disciplinary academics—publishing in "mainstream" sociological journals and winning prizes from the ASA—although they challenged dominant approaches. Starr, by contrast, had won the Pulitzer Prize and considerable recognition outside the discipline but was less active within it. There was a different sense in which he deviated from orthodoxy. Obviously other factors were at work in each case, including Skocpol's gender.

3. We refer here to conditions of work and intellectual orientations, but it is also true that as women and people of color entered sociology in greater numbers, they found positions disproportionately outside the elite research departments. See Ferree, Khan, and Morimoto, chap. 13, this volume.

professors at small colleges and nonflagship branch campuses of state universities, not to mention those with less job security than most state university faculty members. The gap simply grew—a lot.

Under these conditions, hierarchy was renewed and intensified, ironically making the concept of the mainstream even more significant but also making clear that it could have at least two meanings (whose distinction was implicit in the original invocations). On the one hand, “mainstream” was the “core,” the direction of the future, the heart of the discipline, to which “stars” contributed “cutting-edge” research (to mix a number of metaphors frequently mixed in departmental personnel committees). On the other hand, “mainstream” might have meant the direction in which the majority moved, the source of the waves that occasionally rocked the boat of the *ASR* when insurgents thought it should represent the whole field more. It usually did not. Both the 1960s critics and the 1980s advocates used “mainstream” to describe a particular elite and to give the illusion of its nearly uncontested dominance in the field. A more accurate picture would stress the differentiation between “mass market” sociology, produced especially for and through teaching, though also through a great deal of research and analysis, and a brand-name sociology staking primary claim to the mantle of disciplinary science.

This self-reinforcing stratification system is the sort that Pierre Bourdieu described as typical of developed fields, academic and otherwise. Sociology is a weaker field than some, less able to defend its boundaries, but it is a field well enough organized to distribute its own form of capital, recognizing and rewarding some sorts of performances more than others. In the United States, the postwar period was crucial to the consolidation of sociology as a field, as Bourdieu argued:

Indeed, it is no exaggeration to say that the universe of sociology, which had begun to function like a *field* in the interwar period—that is, as a space of competition, of struggles, and of genuine debate (there were also the Chicago school, Marxists, and many of the currents that were to emerge later in the so-called period of crisis)—this universe was soon organized into a veritable hierarchical corporate body, a *corporatio* unified around a common vision of science founded upon a few common principles and on a great many exclusions. (Bourdieu 1991a, 378)

Although our analysis is informed by Bourdieu’s perspective on fields, he greatly overestimated the extent to which there was a single unified structure organizing the American sociological field in the postwar period. Bourdieu claimed that the elite leaders of American sociology “succeeded

in imposing a true intellectual *orthodoxy* by imposing a common corpus of issues, stakes of discussion, and criteria of evaluation.” The terms in which Bourdieu evoked the orthodoxy were familiar from C. Wright Mills’s famous challenge to it, centered on his telling, but tendentious, suggestion that American sociology was caught between two sources of irrelevance: “abstracted empiricism” and “grand theory” (Mills 1959). In Bourdieu’s words, a blend of “functionalist theory” and “positivistic methodology” had come to dominate sociology in the United States. “Establishment sociologists” had put forth a “professional ideology” with global influence. This notion is not altogether false, but it both exaggerates the cohesion and dominance of sociology’s postwar elites and neglects the extent to which the “mainstream” only became visible in the 1960s and 1970s clashes over it, and in attempts to impose authority that became more effective in the 1970s and 1980s.

In what follows, we focus first on the retrospective construction and invocation of the category of “mainstream sociology,” with special attention to the heroes of the insurgents, Mills and Gouldner. We then turn to the era of postwar expansion most frequently associated with the rise and dominance of the so-called mainstream, discussing the figures Bourdieu referred to as the Capitoline triad—Parsons, Merton, and Lazarsfeld—and continuing with a broader view of the postwar elite, including especially developments at the University of Chicago. We return in conclusion to a critical consideration of the discourse of mainstream sociology. In this chapter we are engaged throughout with the interplay of hierarchy, diversity, orthodoxy, and heterodoxy within postwar American sociology.

A Folk Theory

The mythic image of a postwar “sociological establishment” was embedded in a historical narrative of American sociology that is emblematic of a common set of understandings regarding the trajectory of the discipline in the postwar period. Indeed, we take it to be exemplary of a particularly widespread narrative of the history of postwar sociology, and one that demands further attention and interrogation. In brief, that narrative goes something like this. During the interwar period, American sociology was a pluralistic field full of competing visions and “schools” of sociology. After World War II, the discipline witnessed the installation and imposition of a new orthodoxy, the rise of a “new sociological Establishment,” and the formulation of the “professional ideology” of establishment sociologists—what came to be known as “mainstream sociology.” “Mainstream sociology” and the estab-

lishment sociologists who were its beneficiaries and spokespeople were then subjected to vigorous critique and challenged by disciplinary insurrection. This revolt, carried forward by a “disobedient generation” that came of age in the late 1960s and early 1970s, ultimately led to, and was further propelled by, intellectual fragmentation, increasing diversification, and disciplinary “crisis.”⁴

This account of the sociological establishment and its demise typically focuses on “the three great figures” of postwar American sociology: Talcott Parsons, Robert Merton, and Paul Lazarsfeld. As Bourdieu claimed: “Based on their preeminent university positions, the three great figures of the Capitoline triad of the American sociological Pantheon were able to dominate, both in the United States and in other major Western countries, not only teaching institutions but also official publication outlets, professional associations, and even—more or less directly—access to the resources necessary for empirical research” (Bourdieu 1991a, 378). It is a surprisingly individual-centered analysis for a usually relational thinker, but one solidly linked to standard accounts of postwar American sociology.⁵ Not entirely without insight, these all-too-familiar accounts can also obscure and mislead. In particular, standard accounts of postwar sociology overestimate the pre-1968 consensus, neglect important older lines of struggle, and exaggerate the extent to which one among many contending claims to the disciplinary mainstream clearly dominated. Yet an emphasis on a few powerful individuals has pervaded numerous critiques of mainstream sociology, both in books and articles and, even more frequently, in ordinary conversations among sociologists. Although the concept of “mainstream sociology” is ambiguous enough to mean many different things, it has been repeatedly tied to a small number of key figures in sociology’s dominant postwar elite, especially among those American sociologists who came of age in the 1960s and 1970s,

4. For personal accounts of American sociology’s “disobedient generation,” see Sica and Turner (2005).

5. Of course, neither Harvard nor Columbia was “preeminent” in American sociology before the arrival of Parsons, Merton, and Lazarsfeld; if anything, Chicago was. Harvard and Columbia were extremely valuable institutional bases, but not in themselves determinative. Bourdieu had some experience of U.S. sociology, though not enough to prevent him from thinking Robert Merton an old-line patrician and George Homans a man of the people. (Merton came, in fact, from a family of impoverished Jewish immigrants, while Homans was a Boston Brahmin.) Bourdieu had his longest stay in the United States in 1972–73—when criticism of mainstream sociology was in full flow. He was probably closest to Erving Goffman and more generally derived his understanding of postwar orthodoxy in American sociology—and his image of a sociological establishment that was preceded by pluralism and followed by fragmentation—from rebel sociologists within the U.S. field. Mills and Gouldner were also prominent among these.

during the period when the discourse of mainstream sociology first began to circulate widely.

While it would be difficult to argue that there were no hegemonic projects in postwar sociology, we emphasize the diversity of stances articulated by the actors within the supposedly unified, consensual, and singular projects suggested by the concepts of “mainstream sociology” and the “sociological establishment.” Standard accounts of mainstream sociology and the postwar establishment, we think, lack adequate historical reflexivity. There were both attempts to impose orthodoxy and energetic and effective resistance to them.

Common accounts of mainstream sociology not only exaggerate the cohesion and dominance of the postwar establishment but also underestimate the regional and institutional differentiation of the field. Adequately assessing the breadth and depth of success enjoyed by attempts to “impose” a “common corpus of issues, stakes of discussion, and criteria of evaluation,” we would argue, requires substantial historical research, including an examination of the field that extends beyond the major PhD-granting departments and the most influential individuals.⁶ The present chapter offers only a first step, emphasizing diversity within the disciplinary elite but not analyzing the field as a whole. By interrogating familiar, received understandings regarding these key disciplinary actors, however, we seek to promote a renewed debate about the shape, substance, and stakes of postwar sociology, a debate that might lay the basis for the more detailed research we envision.

Laid down in the struggles of the 1960s and 1970s, and in their aftermath, the conception of a dominant sociological mainstream unrestrained by intellectual opposition has had a powerful effect on the way that American sociologists understand their discipline’s history. Once a discursive strategy, a “classificatory epithet,” and a weapon of critical sociologists, the notion of the “mainstream” has become an analytic tool for periodizing postwar sociology, even as it also lives on as a label with which to carve up the contemporary field.⁷ Yet such accounts of postwar American sociology, drawing at least indirect inspiration from the earlier invocations of the mainstream, frequently beg a significant set of historical questions about one of the most important and dramatic periods in sociology’s short history as a discipline. They do so, we suggest, because they rely too heavily and uncritically on the “folk theory” of disciplinary development embedded in the discourse of mainstream sociology.

6. For a similar suggestion, see Bulmer (1994).

7. For the notion of a “classificatory epithet,” see Bourdieu (1988).

Take, for example, a *New York Times* op-ed piece, published in May 2002, in which Harvard sociologist Orlando Patterson lamented the passing of David Riesman, author of *The Lonely Crowd*, one of the best-selling books ever written by an American sociologist (Gans 1997). Riesman's work, Patterson claimed, "inevitably raises questions about the claims and limitations of academic sociology today." By Patterson's lights, the discipline of sociology was in decline, hampered by intellectual limitations that were rooted in "the rise in professional sociology of a style of scholarship that mimics the methodology and language of the natural sciences"—a style that ignored or pushed aside many significant social, cultural, and political issues. The problem, Patterson wrote, was "mainstream sociology," a dominant approach to the discipline that had abandoned its "important mission" to engage the American public in an analysis of "how they live." "Mainstream sociology," Patterson charged, "eschews any exploration of human values, meanings and beliefs because ambiguities and judgment are rarely welcomed in the discipline now." By contrast, Riesman, and a group of other publicly oriented sociologists—from Erving Goffman and C. Wright Mills, to Daniel Bell, Nathan Glazer, Peter Berger, and William F. Whyte—had both engaged and influenced public culture in the United States, practicing a form of sociology "different in both style and substance from that of today" (Patterson 2002).

So, what is—or was—mainstream sociology? While at pains to elaborate its particular pathologies, Patterson nonetheless took for granted that his audience would understand what he meant when he used the phrase. To be in the mainstream is to be at the center of a prevailing trend, to belong to an established tradition or field of activity, to be conventional rather than avant-garde, a denizen of the middle and a defender of the golden mean—one who floats comfortably in the wide river, rather than exploring one of its many tributaries. Patterson's readers would likely have understood at least this much of his meaning. In an age when politicians and journalists, musicians and religious leaders are regularly judged to be in or out of the mainstream, why not sociologists as well?

Riesman, Patterson wanted it known, had been anything but a mainstreamer. Rather, he had embraced an independence of mind and possessed "the nerve of failure," defined—and it was with this image that Patterson closed his piece (a sort of obituary both for David Riesman and for a discipline he saw as in deep decline)—by "the courage to face aloneness and the possibility of defeat in one's personal life or one's work without being morally destroyed." Those in the mainstream had given in to the temptation to simply go with the flow. Not so David Riesman. Not so C. Wright Mills.

If we—either as contemporary sociologists or readers of the *New York Times*—could have been expected to understand what Patterson meant by "mainstream sociology," it is curious to note that the same might not have been said of Riesman or Mills, had they been reading this rendering of the discipline in the late 1950s or early 1960s.⁸ The notion of "mainstream sociology" is of relatively recent vintage. Indeed, although critics of "mainstream sociology"—and here Patterson is only the most recent in a long line of sociologists who have taken aim at this particular bogeyman—have often invoked figures such as Riesman and Mills, it is not clear that either of these writers actually ever used the phrase.

Where, then, did the concept of "mainstream sociology" come from? Its precise origins are difficult to pinpoint, in part because the phrase has appeared as much, if not more, in ordinary conversations among sociologists as it has in books and articles. Nonetheless, reference to "mainstream sociology" seems to have begun to spread through the field in the late 1960s and early 1970s. These were years of unrest and fragmentation. They were also the years during which opposition to mainstream sociology became much more prominent and pointed—some would say "critical" and "incisive," others "uncivil" and "overwrought."

Some indication of the commonality and significations of the discourse of "mainstream sociology" in the early 1970s can be gathered from a book review written by Lewis Coser (1974) and published in the journal *Social Forces*. Reviewing William J. Goode's *Explorations in Social Theory*, Coser welcomed the collection of essays as a sign of health in sociology, and in particular as a sign of the strength of a unifying "theoretical approach" associated with "functionalist analysis." From Coser's perspective, Goode's functionalism was of the "flexible and pliable" sort, open to grappling with conflict in ways that Coser (1956) considered fundamental to understanding human affairs. The book was also an indication that critics of the sociological "mainstream"—in this case functionalism—had rashly and perhaps ignorantly announced its impotence and demise. Calling Goode's

8. Both Riesman and Mills had significant "establishment" sides. Riesman came to sociology from law and sought to engage a broader public than professional sociologists, but he had strong roots in what would later be seen as the disciplinary "mainstream." He taught first at Chicago, then Harvard and was a visiting part of the Lazarsfeld-Merton group at the Bureau of Applied Social Research (see fn. 34 below). Though Mills came from Texas by way of Wisconsin—not an East Coast establishment background—his intellectual roots lay more in pragmatist philosophy and Weber than in Marx. As Becker (1994) notes, he was always determined to maintain his base at Columbia and in New York as a publishing center.

work an “impressive achievement,” Coser (1974, 564) wrote in closing: “If those who have recently come to proclaim the ‘death of mainstream sociology’ from the rooftops were to bring themselves to read it, they might conceivably be led to recognize that their obituaries are decidedly premature.”⁹

Who were these naysayers? Why were they prophesying (or perhaps just calling for) the “death of mainstream sociology”? And why did Lewis Coser see the need to chastise them? The critics of mainstream sociology were themselves a motley bunch, including a variety of radical sociologists, Marxists, feminists, phenomenologists, symbolic interactionists, and ethnomethodologists. In the late 1960s and 1970s, these loosely knit “critical” sociologists participated in various forms of disciplinary revolt and intellectual insurrection.¹⁰ In the process, they retrospectively reconstructed the postwar sociological “establishment” to which Bourdieu would later refer, critiquing an elite group of sociologists who purveyed what came to be called “mainstream sociology” and at times making the case for their preferred alternative.

Inventing the “Mainstream”

For many, the watershed year was 1968.¹¹ As one sociologist told us, it was at the 1968 meetings of the American Sociological Association, held in Boston, that the term *sunshine boys*, a reference to the liberal center of the field in the United States, began to circulate more widely. In an article for *The Activist* written in 1964, Dusky Lee Smith had used the term to describe Seymour Martin Lipset, Nathan Glazer, and Amitai Etzioni (Smith 1964). But already in 1959 Mills had identified “sunshine moralists” as a central tendency in sociology and linked to a broader “liberal practicality” (Mills 1959, 78, 88).

It was also at the 1968 ASA convention that a graduate student from Brandeis University named Martin Nicolaus gave an address titled “Fat-Cat Sociology” in which he repeatedly lambasted the discipline’s established elite.¹² “The ruling elite within your profession,” he told an audience full of

9. The proclamations of the critics notwithstanding, this discourse of death does not seem to have gotten quite as frenzied in the United States as it did in France, where Daniel Cohn-Bendit and his student comrades produced an article titled “Tuer les sociologues” (Kill the sociologists). See Steinmetz and Chae (2002, 112).

10. For more detail, see Fuller (1996), Laslett and Thorne (1997), Levine (2004), McAdam (chap. 11, this volume), Sica and Turner (2005), and VanAntwerpen (2006).

11. See, for instance, Wallerstein, chap. 12, this volume.

12. Nicolaus (1968). Gouldner (1970) famously took Nicolaus’s “remarks” as a jumping-off point for discussion on the “coming crisis” of Western sociology.

sociologists, “is in charge of what is called Health, Education, and Welfare.” Nicolaus shared the platform with Wilbur Cohen, then secretary of health, education, and welfare. “The department of which the man is head,” Nicolaus said, referring to Cohen, “is more accurately described as the agency which watches over the inequitable distribution of preventable disease, over the funding of domestic propaganda and indoctrination, and over the preservation of a cheap and docile reserve labor force to keep everybody else’s wages down. He is Secretary of disease, propaganda, and scabbing.”

Richard Flacks has called this speech “one of the more electrifying moments in the history of the ASA” and one that “provided an opening for ‘radical sociology’” (Flacks 1991, 19). Nicolaus’s criticisms of Cohen were undone only by his criticisms of the sociologists and the “ruling class” they served: “Sociology has risen to its present prosperity and eminence on the blood and bones of the poor and oppressed; it owes its prestige in this society to its putative ability to give information and advice to the ruling class of this society about ways and means to keep the people down. . . . The professional eyes of the sociologist are on the down people, and the professional palm of the sociologist is stretched toward the up people.”¹³

While Nicolaus took aim at the entire profession and worried that calls to reform sociology were, in 1968, too little and too late, he reserved particular scorn for the elite leaders of the discipline, those “fat cats” who gave his short talk its title. “The honored sociologist, the big-status sociologist, the jet-set sociologist, the fat-contract sociologist, the book-a-year sociologist, the sociologist who always wears the livery—the suit and tie—of his masters,” he told his audience, “this is the type of sociologist who sets the tone and the ethic of the profession, and it is this type of sociologist who is nothing more or less than a house-servant in the corporate establishment.” The members of the American sociological “establishment,” in short, had become—indeed, had long been—what Bourdieu, more than twenty years later, would call the “organic intellectuals of the dominant class” (1991a, 378).

Two year later, Sociologists for Women in Society (SWS) was formed, yet another sign of discontent and transformations afoot within the discipline.¹⁴ Indeed, many feminist sociologists who entered the discipline in the late 1960s and early 1970s were also Marxists or radicals and thus considered themselves part of a broader “critical sociology movement” that called for attention to issues of race, class, and gender. As both women and people

13. Or even more forcefully: “Sociologists stand guard in the garrison and report to its masters on the movements of the occupied populace” (Nicolaus 1968).

14. See Ferree, Khan, and Morimoto, chap. 13, this volume, for an account a SWS’s collective advocacy on behalf of women, and its active protest against the marginality of women within the ASA.

of color entered the discipline in greater numbers, they began to challenge dominant sociological paradigms regarding race and gender, often thereby challenging the “mainstream.”¹⁵

Critiques of “mainstream sociology” were not always at the same time prophesies of its immanent death. They might be, instead, demands for its immediate or future transformation, a transformation that would involve moving underrepresented sociologists or sociological perspectives “from the margin to the mainstream” (see Metz 1994). Thus, Ruth Wallace’s edited volume, *Feminism and Sociological Theory* (1989), made the greater inclusion of feminist sociological theory within the “mainstream” one of its explicit aims. Although Wallace’s volume came at the end of the 1980s, as feminist theory was consolidating its gains and assessing its continuing challenges, the call for inclusion and the quest for disciplinary transformation were already intertwined in an earlier period, both among feminists and other critical sociologists. As Doris Wilkinson wrote in her review of James E. Blackwell and Morris Janowitz’s *Black Sociologists: Historical and Contemporary Perspectives*, “perhaps what is required in the postfunctionalist period, as American sociologists reflect on their heritage, is an ideological metamorphosis and exorcism of racialist elements permeating the sociological consciousness.” Wilkinson evinced hope that the volume she was reviewing would push forward the “future incorporation of black sociologists’ conceptual paradigms into mainstream sociology.”¹⁶

By the early 1980s, the extent to which the notion of a “mainstream sociology” was familiar in feminist circles was evidenced by the appearance of a telling variant. After Mary O’Brien coined the notion of the “male-stream” in her 1981 *The Politics of Reproduction*, feminist sociologists quickly

15. See Laslett and Thorne (1997), Brown (1991), and Collins (chap. 17, this volume). It remains an open question how closely linked the pursuit of more equitable inclusion within sociology’s mainstream was to its intellectual transformation and where the balance fell in different projects.

16. Wilkinson (1975, 462). In a similar vein, although from the vantage point of methodology rather than conceptual paradigms, R. Stephen Warner (1976, 68) cited ethnomethodology’s “potential contribution to mainstream sociology.” See also the introduction to *Radical Sociologists and the Movement*, in which the authors suggest that radical sociology amounts to “a vibrant intellectual countertradition” that “emerged *within* mainstream sociology” (Oppenheimer, Murray, and Levine 1991, 5; our emphasis). Indeed, many radical scholars were attracted to sociology precisely because they saw it as a discipline at least somewhat hospitable to their critical perspectives, concerns about the dominance of the “mainstream” notwithstanding. As Erik Olin Wright puts it, in his contribution to *A Disobedient Generation*, “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” (2005, 342). It remains an open question how closely linked the pursuit of more equitable inclusion within sociology’s mainstream was to its intellectual transformation and where the balance fell in different projects.

took up the term. “What women need to do,” O’Brien wrote, “is to be able to demonstrate that male dominant culture and the male-stream thought which buttresses and justifies it are both, in some sense, prejudiced by the very fact that they are masculine.”¹⁷ Although *The Politics of Reproduction* was mainly concerned with political theory, the notion of a “male-stream” resonated in sociology as well. If opposition to “mainstream sociology” had connected feminists to others on the discursive and disciplinary margins, opposition to “male-stream” sociology set them apart once again, sharpening a gendered critique of disciplinary hierarchies and inequalities. While feminists were part of a broader “critical sociology movement” then, “mainstream sociology” was not feminism’s only target. Feminist sociologists were just as willing to critique Marxists and radicals for their “male-stream” assumptions, just as women of color would critique white feminists for unquestioned assumptions and privilege of their own.

The concept of “mainstream sociology” was invoked as well by Marxists and other critical sociologists, who employed it at times as an alternative to the Marxian notion of “bourgeois sociology,” or alongside references to “Establishment Sociologists.” And in the late 1970s and early 1980s, just as the feminists had, Marxists and radicals relied on the term to story their own recent past within (and against) the discipline.

In an article published in 1982 in the *American Journal of Sociology*, for instance, Michael Burawoy used the notion of “mainstream sociology” as seeming shorthand for the structural functionalism he took to be dominant in the immediate postwar period. “The two decades after World War Two,” Burawoy wrote, “were dominated by Talcott Parsons’s grand synthesis of Weber, Durkheim, Pareto, Marshall, and subsequently, Freud. *The Structure of Social Action* (1937) set new parameters and directions in the heyday of an expanding field. It was during this period that Parsons, together with a number of eminent colleagues and students, developed and consolidated the basis of structural functionalism, lending American sociology at least the appearance of an overarching coherence” (Burawoy 1982, 51).

Since “there were few Marxists able to sustain a creative dialogue and critique to counter the euphoria of 1950s sociology,” Burawoy suggested, Parsonian structural functionalism had “pursued its totalizing mission unhindered by an intellectual opposition that might have brought its premises into line with the emerging political realities and social movements of the 1960s” (1982, 53). Indeed, in 1965 Parsons had called Marxian theory “ob-

17. O’Brien (1981, 5). O’Brien’s criticisms of male-stream thought had a clear affinity with prominent criticisms of both male-dominated and “mainstream” sociology. In each case, the emphasis was on the specific social sources from which “mainstream” or “male-stream” sociological theory sprung and on the resulting ideological effects of those sources.

solete." Yet Parsons, Burawoy suggested, was not only oblivious to the Marxist literature after Marx; his theory had also proven to be oblivious to much of the world around it and to "the new historical forces being unleashed on its own doorstep." "Isolated" and "abstract" in character, Parsonian theory was out of step and ill-equipped to deal with the "burgeoning collective disaffection of the 1960s."¹⁸ As a result, "the 1960s involved the rejection of mainstream sociology," a rejection epitomized most forcefully by the work of two frequently cited sociological rebels, C. Wright Mills and Alvin Gouldner. As Burawoy wrote: "Vilified by C. Wright Mills, and later given a more nuanced critique by Alvin Gouldner (who sought to recover the emancipatory potential, the voluntaristic moment of structural functionalism), mainstream sociology came under relentless assault. 'Conflict theory' replaced 'consensus theory'; contradiction replaced equilibrium; critiques of capitalism replaced its celebration" (1982, S4).

Burawoy's account of these changes within the sociological field was intended to fit a specific purpose, introducing an *AJS* issue devoted to "Marxist Inquiries." Thus, in his brief rendering of postwar American sociology, he attempted to situate and contextualize a "resurgence of Marxism in American Sociology," a revival whose impetus had come from "the protests and disillusionments of the 1960s and early 1970s" (Burawoy 1982, S7). Burawoy himself was an important part of that resurgence. When in the mid-1970s, Erik Olin Wright and others succeeded in getting the Department of Sociology at the University of California, Berkeley, to consider Burawoy for a position, letters were solicited from Burawoy's professors at the University of Chicago, where he had received his PhD. In the midst of a long and contradictory letter evaluating Burawoy's sociological work and potential, Edward Shils wrote to Robert Bellah: "It is my impression that Mr. Burawoy is hampered intellectually by excessive and unrealistic preoccupation with what he regards as conflicts between himself and the prevailing trends of sociological analysis in the United States. He seems to think that he must struggle to prevent himself from being overpowered or seduced by 'mainstream sociology.'"¹⁹

By the mid-1970s, then, the notion of "mainstream sociology" was in

18. Burawoy's rendering appears to have been representative of a widely held view among critical sociologists. To cite another example, more than ten years earlier, Steven Deutsch made a similar point in his review of Alvin Gouldner's *The Coming Crisis of Western Sociology*: "While American cities burn and American military might destroys a people and culture in Southeast Asia, Talcott Parsons and his fellow Establishment Sociologists launch their celebration of American society" (Deutsch 1971, 322).

19. A portion of this letter is reproduced in Burawoy's chapter for *A Disobedient Generation* (Burawoy 2005a).

the disciplinary air, and by the early 1980s, Burawoy could confidently use it to refer to structural functionalist orthodoxies. Yet, as the letter from Shils indicates, at this point the term was being used largely as a means of intervening critically in debates about the discipline. Indeed, Shils's image of Burawoy's "excessive and unrealistic preoccupation" with "mainstream sociology" was no doubt indicative of the self-image of the so-called mainstream. Shils's use of scare quotes is instructive. From Shils's perspective, "mainstream sociology" may as well have amounted to a figment of Burawoy's imagination. It was a fantasy that paid no intellectual dividend, a counterproductive and misguided obsession.

From the perspective of Burawoy and his peers, however, it was precisely the "struggle" (not only individual but collective) between mainstream sociology and its others—whether Marxism, feminism, or some other form of "critical" sociology—that would generate genuine intellectual progress. The invocation of a sociological "mainstream" sharpened the conflict between the previously dominant structural functionalism and its ascendant opponents. Like its feminist variant, "male-stream," the notion of "mainstream sociology" employed a binary logic, differentiating between those individuals or forms of sociology that were in the mainstream and those that were willing to stand—consciously and conspicuously—outside it. This binary gave the notion of "mainstream sociology" some of its power and helps to explain in part why it captured the imagination of a generation of critical sociologists who were opposed to what they saw as dominant trends within the discipline.

Two key touchstones for sociologists of this generation, many of whom mobilized the discourse of mainstream sociology in order to critique the discipline, were Alvin Gouldner and C. Wright Mills. Indeed, E. Digby Baltzell once referred to Mills as "the great disestablishmentarian guru." "In my day at Columbia," Baltzell wrote, "when ambitious graduate students still looked to Brooks Brothers rather than the local Army and Navy Store for their sartorial standards, Professor Mills was a prophet in lifestyles, as well as in sociology, as he roared up to Fayerweather Hall on a motorcycle, clothed more often than not in the style now cultivated as a badge of baptism by the followers of Professor Gouldner."²⁰

As Gouldner would later suggest, the sentiments of the New Left and the

20. Baltzell (1972, 215–16). As Howard G. Schneiderman (1991, xv) notes in his introduction to Baltzell's *The Protestant Establishment Revisited*, Baltzell's cool memories of Mills were at least in part related to his claim that Mills had included one of Baltzell's long papers in his book *White Collar*, without attribution. See also Wrong 1999. By the 1990s, Baltzell's own sartorial standards had returned to fashion and were celebrated in a J. Peterman catalogue that hawked an "E. Digby Baltzell Memorial Tweed Suit" in tribute to the past glory of the WASP.

"Psychedelic Culture" of the 1960s were "deeply dissonant with the sentiments and assumptions embedded in the Parsonian synthesis"—so much so, he suggested, that "the mind boggles at the thought of a Parsonian hippie" (Gouldner 1970, 160). Mills, on the other hand, represented a clear alternative. "My first image of sociology," Harvey Molotch (1994, 231) has written, "was through the writing of C. Wright Mills, whom I also imagined as an album cover. He merged with Jack Kerouac, Lenny Bruce, and Henry Miller in my mind; they were all heroes who knew the world through its edges—deviant, strident, and/or dirty-mouthed."

While he has often been celebrated as a sociological rebel, C. Wright Mills was much more than just a maverick on a motorcycle. In fact, this image of Mills-the-rebel, as Daniel Geary has suggested, may be one of the reasons that Mills-the-sociologist has frequently been both mythologized and misunderstood. An unstinting critic of American sociology, Mills was alienated from the professional and academic center of his discipline, including some of his most powerful colleagues in the field. Yet his intellectual approach and sociological interests were importantly connected to powerful currents within the discipline, in no small part due to his place at Columbia University. As Geary puts it, "Mills' approach to social science was distinctive, yet it shared important elements with mainstream work."²¹

A critic of what would come to be called mainstream sociology, Mills seems never to have used the term himself. But is it the sort of term he might have used? Is the understanding of sociology as embedded in the discourse of "mainstream sociology" an understanding Mills would have embraced? In at least one sense, the answer to this question is clearly yes. Those who employed the notion of mainstream sociology often relied on an either-or logic, differentiating those individuals who were in the mainstream from those who were willing to stand outside it. Its use therefore often involved an important element of intellectual self-identification, if only of the negative sort. Mobilizing the discourse was a way to position oneself within the discipline and discourse of sociology, if only by saying what one was not. This is the sort of critical disciplinary position and intellectual identity Mills cultivated, especially as his career developed.

In another sense, however, it is less clear that the discourse of "mainstream sociology"—pro or con—fits with Mills's position and perspective all that well. First, although an intellectual outsider in a variety of ways, Mills spent the greater part of his career in the sociology department at Columbia University, one of the two universities most widely associated with

21. Geary (2004, xi). We draw substantially on Geary's reading of Mills and his milieu.

the postwar hegemony of mainstream sociology, and home to both Merton and Lazarsfeld.²² From an institutional perspective, at least, Mills was a member of what would later be called the "sociological establishment," even if he would not be counted as an establishment sociologist. Second, and perhaps more importantly, there is a sense in which the notion of mainstream sociology implies the existence of an alternative approach to sociology, and perhaps an alternative sociological tradition. Yet Mills never really elaborated such a tradition.

In *The Sociological Imagination*, for instance, while he referred to the possible continuation of a "grand tradition" of "classical sociology," Mills largely ignored existing alternative trends within the discipline. The much-vaunted notion of the "sociological imagination" did not draw on examples of contemporary works of sociology—those occupying what Mills had earlier called a "third camp"—and thus did not seem to represent a currently viable alternative to the two leading styles of sociological research that were the book's primary targets. It was rather presented as an outsider's vision of what social science might be or might become, if not for the dominance of Parsonsian "grand theory" and Lazarsfeldian "abstracted empiricism."

To a certain extent, Mills's hope that such dominance would give way to a greater flowering of the sociological imagination can be gathered from the title he initially gave to what was to become his most famous book. Prefiguring the discourse that announced the "death of mainstream sociology," *The Sociological Imagination* was originally to be called *Autopsy of Social Science*. In the end, Mills decided that what sociology needed was not an autopsy of its already dead body but a "diagnosis" of its present sickness, and one that might presage a "coming health" (Mills 1959, 132; see Geary 2004, 230–33). Mills's rhetorical style nonetheless left the impression that he saw himself as one of the very few sociologists capable of administering the appropriate shot in the arm. "I must ask that you *not* mention any of this to our friends, especially on Morningside Heights," he wrote to a friend with whom he had discussed the first draft of his book. "I want it to be just one big, dandy surprise: as from a prophet who comes in from the desert" (see

22. Mills's contempt for Columbia's proprieties and authorities is celebrated. It is sometimes suggested that it was either occasioned by or the reason for a refusal to appoint him to the graduate faculty. Mills spent his entire Columbia career affiliated with the undergraduate college and not the graduate school. Irving Louis Horowitz (1983) presents this as evidence of repression by Lazarsfeld and Merton (who recruited him), and there may be truth to this. But it seems as likely to be Mills's way of preserving autonomy from the Merton/Lazarsfeld team—though of course at considerable cost in chance to influence graduate students.

Geary 2004, 232). If Mills was to be remembered as a maverick on a motor-cycle, then, this was at least in part a myth of his own deliberate making. And he would not be critical sociology's last self-proclaimed prophet.

Straddling the boundaries among disciplinary professionals, left critics, and more publicly oriented writers was another self-styled prophet and critic of the postwar mainstream, Alvin Gouldner. Gouldner spent most of his career as an academic sociologist, however pugnacious and "ill-adjusted" he was to that role. He was among Robert Merton's most brilliant students and certainly the one most likely to leave a trail of blood and controversy. Earlier in his career he produced sociology in the Columbia mode of problem-oriented, modestly critical, broadly functionalist (but just a little bit Marx-inspired) inquiry. Later he founded *Theory and Society* and pushed for an alternative to "mainstream sociology," among other things seeking to connect Western sociology better to Marxism and all sociology better to classical social theory.

Until the late 1960s, wrote Richard Flacks and Gerald Turkel, radical voices in sociology had been "cries in the wilderness" (1978, 198). Yet little more than a decade after the publication of Mills's *The Sociological Imagination*—and in just the period during which, according to Flacks and Turkel, "the radical polemic began to have a real impact on sociology's character"—came the publication of another book that captured the imagination of a new generation of sociologists critical of their discipline's "mainstream." With *The Coming Crisis of Western Sociology*, Gouldner had an improbable bestseller, yet it was perhaps a sign of the times, and of the centrality of sociology to those times, that the *New York Times Book Review* named it one of the Twelve Books of the Year for 1970. *The Coming Crisis*, wrote Steven Deutsch in an *ASR* symposium on the book, was a "monumental work" that went "beyond the writings of C. Wright Mills, whose writings have had great impact on anti-establishment thinking, and whose concerns were with the ideological factors in social theory." Just as Mills's work had challenged the social theorists who dominated the field, and thus drawn their ire, so *The Coming Crisis*, Deutsch predicted, would sit uncomfortably with "Establishment Sociologists in the functionalist tradition."²³ As one sociologist told us, Gouldner's critique of these elite sociologists, and especially Talcott Parsons, may have been "overdone," but it was also "damning," and the book became "a kind of rallying text" for a new generation of politically engaged critical sociologists. As Flacks and Turkel put it (1978, 197): "What

23. Deutsch (1971, 322). In the course of his review, Deutsch used "mainstream sociology" as a marker for the work of these "Establishment Sociologists."

had been in the 1950s a rhetorical underground within the discipline, became by the late 1960s a critique in action; for the new-left activists challenged not only national policymakers and the university managers, but also their own professors."

One key feature of Gouldner's work, not infrequently adopted and adapted by his New Left followers and fellow travelers, was a distinction—one of the most memorable of Gouldner's "ubiquitous dichotomies"—between the "Academic Sociology" produced by the "Establishment Sociologists" and the form of sociology Gouldner championed, which he referred to as "Reflexive Sociology."²⁴ Like Mills's *The Sociological Imagination*, the bulk of Gouldner's *The Coming Crisis* was concerned with a critical consideration of the "sociological establishment," embodied by the figure of Parsons. Yet "Reflexive Sociology," Gouldner's explicitly "radical" alternative, was intended to be not only a "nay-saying or a 'critical sociology,'" but also the basis for articulating a positive vision of sociology and society (Gouldner 1970, 500).²⁵ While Academic Sociology misrecognized the social world in which it was situated, Reflexive Sociology would be positioned to grapple with that world, and thus to achieve a "distinctive awareness of the ideological implications and political resonance of sociological work" (499). If Parsonian theory was facing an "impending entropy," in good part because it was out of step with the political and social spirit of the times, Reflexive Sociology provided a recipe for a self-critical radical sociology. Since Parsonian sociology was "no longer instrumentally or expressively appropriate to the time," Gouldner wrote, "it withers as an intellectual paradigm."²⁶ In the face of an emerging "polycentrism" in American sociology, in which Parsons and his followers were being de-centered, an opening appeared for the development of a more reflexive approach.²⁷

24. On Gouldner's "ubiquitous dichotomies," see Lemert and Piccone (1982). For more on Gouldner, see Chriss (1999).

25. As other sociologists appropriated and adapted Gouldner's approach, several different and not entirely commensurable dichotomies were put into play: "academic," for instance, was clearly a broader category than "mainstream."

26. On the "impending entropy" of Parsonianism, see Gouldner (1970, 159–62). In contrast with Burawoy's later characterization of Parsonian theory's inability to deal adequately with the "burgeoning collective disaffection of the 1960s," which emphasized the civil rights movement, the antiwar movement, and the student movement (Burawoy 1982, 54), Gouldner (1970, 162) emphasized that Parsonianism was "out of phase" with both a "mature Welfare State" and with an "emerging Psychedelic Culture."

27. On the emerging polycentrism, see Gouldner (1970, 22, 157). At the same time, it should be noted that Gouldner's own book reproduced the overwhelming emphasis on theory—in more or less the Parsonian sense—as constitutive of sociology.

Behind the Myth of the Mainstream: Harvard and Columbia

Functionalism became a favorite target of the insurgents partly because they were interested in storming the citadel of high theory. They needed to challenge Parsons. But more generally, functionalism had become a general vocabulary for sociology, assumed except where a specific effort was made to supplant it.²⁸ As Immanuel Wallerstein notes in his contribution to this volume, structural functionalism was the “dominant theoretical label” of this period, and Parsons’s 1937 *The Structure of Social Action* was the defining text. Beginning his project of intellectual synthesis and general theory building in the 1930s, Parsons had some very important collaborators, like Edward Shils, and a very advantageous institutional position at Harvard. He quickly moved into the forefront in the postwar period, famously arguing that Weber, Durkheim, Marshall, and Pareto had all converged on a common theory of action (Parsons 1937).

There was, however, another convergence. At the heart of Parsons’s “functionalism” was an argument about the essential interconnection of innumerable disparate aspects of social life, issues in social reform, possibilities for social policy. These were all part of a “system.” What this meant, to some extent, was that piecemeal projects of empirical research, social reform, and policy intervention were doomed to underperformance if not failure unless they took account of the deep interdependence of parts within the system as a whole. Thus, Parsons reproduced the concern for “interdependence,” which Thomas Haskell (1977) has identified as basic to the move away from the old nineteenth-century approach that embedded social science deeply in specific reformist ventures and idealized individual commitments to them. At the end of the nineteenth century, increasing engagement with interdependence doomed the American Social Science Association and helped spur the development of academic social science disciplines, including especially sociology. It was a central theme of the *AJS* in the 1890s. The “general theories” of the day were much looser than those of Parsons (whether Sumner’s Spencerian social Darwinism or the integrative approaches of Ross, MacIver, Giddings, and similar earlier generalists). Although Parsons’s approach to sociology would thoroughly displace these earlier theories, he was to a large extent taking up an analogous intellectual agenda. And if early on he would famously ask “who now reads Spencer?”

28. One crucial condition of this was that Merton and Lazarsfeld chose to play on the functionalist team. The differences between Columbia and Harvard were kept muted rather than elevated into a clash of paradigms, and the rapprochement helped to sustain the notion that functionalism was a singular “mainstream.”

and later reveal that he seemed to read Spencer more than most, it was partly because he was taking over the intellectual space claimed by the American Spencerians, but in a far more professional way.²⁹

It hardly needs repeating that Parsons’s theoretical writings were influential, though many have pointed out that the actual substantive influence was perhaps less than frequency of citation or borrowing of terminology might suggest. Parsons’s earliest students included several of the key figures of postwar sociology.³⁰ The Department of Social Relations he helped to found played an important role in anthropology and to a lesser extent psychology. Hardly simply conservative, despite its later reputation, it was an expression of the optimism felt by many social scientists that they were finally going to achieve a systematic, integrated, and cumulative approach to knowledge. This was, moreover, a liberal optimism, for the social scientists expected their work to inform government policy. It was in large part as the country as a whole became more conservative, not least in the McCarthy era, that Parsons and many colleagues turned their hopes away from state-led reform, at least within the United States. This is one reason why so many turned their attention to modernization, which was, as Nils Gilman (2003) has suggested, not only a reflection of America’s cold war competition with the USSR but an externalization of liberalism from domestic affairs into the “development” of the non-Western world. Probably the most widespread and influential engagement with Parsons’s general social theory came through modernization theory and related projects of comparative research (though it should be recalled that Edward Shils also played a leading theoretical role here). Modernization theory was taken up more by political scientists, like Gabriel Almond and others who took up the comparative study of political behavior (partly under the influence of the committee Almond long led at the SSRC), or by anthropologists like Clifford Geertz, who, along with Shils, took up the study of “new nations.”³¹ But in

29. As Alan Sica (1997) notes, this much-cited question about the reading of Spencer was not Parsons’s own invention, but was rather borrowed from his Harvard colleague Crane Brinton. It is also worth noting that sociology has tended to swing back and forth between periods in which theoretical synthesis was dominant and periods in which analysis in various empirical subfields grew with only loose connections to one another, if any. The Spencerians were the most important synthesizers arguing for interdependence before Parsons.

30. Examples included Robert Merton (and some others shared with Sorokin), Kingsley Davis, Wilbert Moore, Herbert Garfinkel, Renee Fox, Robert Bellah, Neil Smelser, Robin Williams, and Marion Levy.

31. Geertz’s calls for “local knowledge” would later appeal to a variety of rebels against functionalism; his roots as a student of Parsons and a functionalist analyst are not always recognized.

sociology as well, it was perhaps at the very “macro” level of what societies were supposed to be like and how they could be compared that the Parsonsian system revealed most purchase.³² And it is therefore no accident that when functionalism went into decline, many of the most ferocious challenges focused on modernization theory (most prominently bringing various versions of Marxist theory into play).

Domestically, Parsons himself wrote on the American university, and others of his students developed aspects of his theory in different empirical domains, from religion to medicine. But as suggested previously, Parsons’s theory was most important where concerns were the most general—and accordingly, in the Parsonsian sense, theoretical. Kingsley Davis and Wilbert Moore, for example, famously extended Parsonsian theory directly into empirically informed analyses of domestic social stratification. The Davis-Moore argument as to the functional value of inequality became a shibboleth for insurgents in the 1960s, who sought to show how functionalism legitimated the existing order and neglected the role of power.

In many active lines of empirical research there was only a nod toward Parsonsian functionalism. In quite a few of these lines of research, the project of “functionalism” was more substantially influenced by the work of Robert K. Merton. Merton had studied with Parsons toward the end of his graduate school years at Harvard, though his primary mentors were Pitirim Sorokin and the historian of science George Sarton. He gave Parsons generous credit for showing him a more analytically integrated view of the enterprise of sociological theory, but already in the 1930s he had argued with the budding general theorist, suggesting that his “formulations were remote from providing a problematics and a direction for theory-oriented empirical inquiry into the observable worlds of culture and society.” Merton would go on to state his “case for ‘theories of the middle range’ as mediating between gross empiricism and grand speculative doctrines.”³³ In other words, Merton claimed the middle ground between “abstracted empiricism” and “grand theory” even before Mills used the terms as so effective an indictment. But Merton’s “middle-range theory” was different from the kinds of analyses Mills produced in *White Collar* or *The Power Elite*. The latter were works aimed not only at professional sociologists but at a broader

32. Parsonsian comparative sociologists like Marion Levy have been nearly completely forgotten but were very influential for decades. Some others like Robert Bellah remained prominent as they shifted topics from comparative research to communitarian inquiry into American society. And indeed, as Gilman has noted, modernization theorists not drawn to neoconservatism were apt to become communitarians.

33. This is Merton’s recollection in “A Life of Learning” (Merton 1994).

public. Though their analyses and (more or less implicit) theoretical frameworks were different, Mills’s writings were in crucial ways more akin to David Riesman’s *The Lonely Crowd* than to the works of either Merton or Parsons.³⁴

Merton’s idea of middle-range theory focused on transposable explanatory constructs—what in contemporary literature are often evoked by the “mechanisms” at their core. These had both a manifest and a latent function. The former was to focus sociologists’ attention on explanatory problems that could be tackled effectively and be the basis for what Lakatos would later dub a progressive research program (Lakatos and Musgrave 1970). The latent function was to secure some autonomy from Parsons without direct confrontation. Together, both manifest and latent functions contributed to the success Merton enjoyed in creating an approach to sociological explanation that offered sociologists not merely a vocabulary, a theoretical framework, or a belief system but a craft skill. Merton had been hired at Columbia as part of a compromise—MacIver would get a theorist and Lynd an empiricist.³⁵ In fact, both MacIver and Lynd were supplanted by the new guys, who quickly formed a strong team. The empiricist was Paul F. Lazarsfeld, though it needs to be emphasized again that although styled the “theorist” in this relationship, Merton was in fact consistently engaged in empirical research (and even innovated technique, as, for example, by developing the “focused group interview” or “focus group”). The idea that theory and empirical research should be combined was already prominent at Columbia; Giddings had stressed as much in the early twentieth century. But Merton and Lazarsfeld gave this new substance and form.

34. Riesman’s differentiation from the Columbia sociologists should not be exaggerated. Merton’s account of “social structure and anomie” shaped the concepts of anomie, adjustment, and autonomy pivotal to *The Lonely Crowd*. With Nathan Glazer, Riesman tried out the conceptual framework by reanalyzing the interviews Mills’s students had conducted for *White Collar*. And he contributed a warm memoir to the festschrift for Paul Lazarsfeld, crediting the latter with influencing his decision to leave law for sociology. As he wrote, “It is hard to give a sociology student today a sense of the excitement that surrounded the Bureau of Applied Social Research in the decade 1948–58—an atmosphere from which I learned and drew collegiality and friendship. It was a shop where all kinds of studies were underway and in which substantive explorations of then relatively new areas . . . were proceeding simultaneously with methodological inquiries . . . ideas were freely shared, hunches tried out, languages of social research elaborated” (Riesman 1979, 212).

35. In fact, the story of their hiring and eventual partnership is more complicated and interesting. At first, indeed, they barely spoke and showed little interest in each other—until Lazarsfeld decided he simply ought to be gracious and acknowledge his opposite number by going around to say hello. The rest is history, though not adequately written history. See Hunt (1961), Lazarsfeld (1975), and Merton (1994).

They pioneered what became a widespread and normatively approved approach to formulating research projects and journal articles.³⁶ If Parsons's early students were central to modernization theory and the most general arguments about social structure and inequality, Merton's and Lazarsfeld's students became leaders in a wide range of specific research programs.³⁷

Parsons looms larger in histories of sociology than his actual influence warrants, not only because he has been a regular target of critics (a fact that has tended to retrospectively inflate his disciplinary centrality), but also because histories by sociologists tend to focus disproportionately on theory. Parsons's theory is treated as an icon representing the whole postwar era. But this is partly because it can be deployed as an exemplar of a general theory and set alongside other putatively analogous theories in a classical canon. This is the approach of textbooks, but equally of much serious theoretical reading and writing (for example, Jürgen Habermas's *Theory of Communicative Action*). It was exemplified in Parsons's own *The Structure of Social Action*. And the form or genre itself is in important ways one of Parsons's influences. Drawing on his German education and European reading, he helped to establish the sociological canon.³⁸ There had long been histories of social thought—by Howard P. Becker and Harry Elmer Barnes, for example, and many of the other great figures of early twentieth-century sociology (see Sica, chap. 21, this volume). But most of these ranged over all manner of social thought from ancient Greece to medieval scholastics, early modern moral philosophers, and political economists. Parsons cre-

36. Before he ever got to Columbia, Merton had published articles in three of the five first volumes of the *ASR*, as well as the *AJS* and *Social Forces*. He was among the first sociologists whose reputation would rest more on articles than books. Merton and Lazarsfeld published relatively little jointly, considering how much each influenced the other. Only a single co-authored article (Merton and Lazarsfeld 1943) marks the extent to which their separately published wartime inquiries into communications and mass persuasion were in fact mutually informing. Their important edited collection, *Continuities in Social Research* (Merton and Lazarsfeld 1950), served as something of an advertisement for their shared perspective. In it they both paid tribute to Stouffer's *American Soldier* studies and showcased their own different styles of analysis—notably in Merton and Alice Rossi's chapter on reference groups.

37. The students included Peter Blau, James Coleman, Lewis Coser, Rose Coser, Alvin Gouldner, Seymour Martin Lipset, Alice Rossi, Peter Rossi, and Philip Selznick.

38. What Parsons thought belonged in this canon is suggested by the anthology he edited with Edward Shils, Kaspar D. Naegle, and Jesse R. Pitts, *Theories of Society* (Parsons et al. 1961). Parsons was also active in translation and in advising the Free Press on its selection of canonical works to publish in English. The Free Press publications of the 1950s and 1960s were themselves a considerable influence on American sociology and especially the understanding of sociological theory within it.

ated a new genre of specifically sociological theory—as distinct from social thought more generally (albeit with a tenuous argument as to the convergences that defined the canon). This was part of what Robert Merton (1994) recalled of Parsons's teaching at Harvard—"the corpus of social thought which Sorokin summarized," Merton claimed, "Parsons anatomized and synthesized"—and it amounted to a crucial act of field formation, in Bourdieu's sense, as much because of its exclusions as its inclusions. Parsons's critics have emphasized his failure to give Marx much prominence in his identification of the crucial resources he presented to the sociological "mainstream." Certainly his canon, like any, was skewed. And later readings of Parsons suggest he learned rather more from Marx than he was inclined to emphasize in the 1950s.³⁹

Parsons's act of canon formation has, however, been both influential and misleading in quite another way as well. It established each of those authors included as a producer of general theory in the same sense as Parsons himself sought to produce it. But neither Weber nor Durkheim—to take the two most central examples—was so purely an abstract systematizer as was Parsons. To render each a "theorist" required placing emphasis on one dimension of their intellectual production and specifically underemphasizing the extent to which each was—and more or less inseparably—a pioneering empirical researcher. Merton did not produce much abstract general theory in this sense—what Mills labeled "grand theory." He did seek generality through middle-range theories, each of which would transcend particular contexts of explanation. One might thus use the theory of deviance or reference groups or role sets in explaining patterns of crime or science or educational attainment. Merton was in the business of giving working sociologists tools with which to produce publishable research and to evaluate publications and see what should be retained as part of cumulative scientific knowledge.⁴⁰

The same pragmatic orientation that helps explain why Merton figures

39. See, for example, Jeffrey Alexander's own effort to read theory in the Parsonian mode, *Theoretical Logic in Sociology* (1982). Also on Parsons, see Robertson and Turner (1991), Camic (1987, 1989), and Howard Brick's useful efforts to trace Parsons's movement from a state-centered vision of social engineering in the 1930s to a more conservative and impressively apolitical social theory in the 1950s (Brick 1993, 2000).

40. Merton's somewhat unsatisfactory distinction of the history of theory from its systematics attempted both to express this approach to scientific knowledge and to claim something of the ground of general theory that Parsons dominated. It was a distinction at once informed by and in tension with much of his own empirical research on science. See Merton (1968b).

less than he should in disciplinary histories helps also to explain why Lazarsfeld is much more widely and centrally remembered than Samuel Stouffer—in a sense Lazarsfeld's Harvard counterpart. Lazarsfeld himself regarded Stouffer as a titan and a major influence on his own work. But Stouffer has faded from memory—and indeed in his lifetime his influence faded, despite his major work in sociological statistics and the prominence he received from the *American Soldier* studies (see Abbott and Sparrow, chap. 8, this volume). Lazarsfeld's greater fame no doubt reflects in part his influence on the substantive field of communications research. It is also the product of his institutional activism. He founded not only Columbia's highly productive Bureau of Applied Social Research but the Center for Advanced Study in the Behavioral Sciences as well. If Merton was the key leader in the sociology department, Lazarsfeld was in each of these other institutions.⁴¹ Surely Lazarsfeld's influence stemmed importantly from his partnership with Merton (and vice versa). And the lack of corresponding teamwork between Parsons—with his more self-sufficient version of theory—and Stouffer helps to explain why Stouffer is less well known today.⁴² But equally important is the specific approach that Merton and Lazarsfeld took up together. This approach focused on identifying problems—often practical as well as intellectual, which was a crucial reason for Lazarsfeld's capacity to keep the Bureau and their larger project funded—and seeking to resolve these in a way that would contribute to cumulative scientific knowledge without depending on prior theoretical synthesis.

Whatever its implications for their retrospective fame, the “craft” orientation Merton and Lazarsfeld developed equipped their students (and many others) with a set of skills oriented toward pragmatic problem solving and intellectual production. Their influence came through the students and the approach as much as their own specific publications. Even many of the concepts Merton originated may be thought of in this way. “Unintended consequences” was an idea running through the whole of functionalist analysis. What Merton did in his 1936 article on the topic was to frame it in a way that made it a workable tool for sociologists. Likewise, “role model,” “Matthew effect,” “self-fulfilling prophecy,” and other Mertonian coinages passed not only into the general vocabulary but into the sociologist's tool

41. The founding of the center was the occasion for a rare falling out between Merton and Lazarsfeld, when the former broke ranks with the latter's plan for a more hierarchically structured organization centered on the training of junior members by senior.

42. At the Lazarsfeld centenary conference, September 29, 2001, Merton suggested that at the time Stouffer was recruited to Harvard there was a conscious intention to create such a partnership, but that this never took off.

kit. They were not so much theories to be tested as equipment for constructing explanations.⁴³

The point is of more general relevance, however. Merton and Lazarsfeld contributed at least as much to the mainstream of American sociology as Parsons did—if by this we mean the process of continually producing more sociology. Mills skewered Lazarsfeld—a bit unfairly—with his invocation of “abstracted empiricism” as one of the sources of sociological irrelevance. But as we have discussed, Merton was not in the grand theory business. Both Merton and Lazarsfeld—and their numerous students—were already engaged in a project that escaped the dualism. But their project did not escape the authority or normalization structures of American sociology. Indeed Merton and Lazarsfeld were extremely influential gatekeepers. Most centrally, though, if Parsonsian theory gave postwar sociology its preeminent ideology of unity and generality, Merton and Lazarsfeld provided the most influential exemplification of how theory and research fit together, and with their students they produced the canonical model for expressing this combination in journal articles and monographs.

Merton himself reflected on their relationship as having given sociology its “original odd couple”: “Paul was and remained the matter-of-fact but methodologically demanding positivist; I was something of a doubting Thomas about positivism who, in my very first published paper, had dared to satirize, rather than adopt, the ‘enlightened Boojum of Positivism’” (Merton 1994, 170). It is crucial to grasp that the Boojum is a snark, from Louis Carroll's poem “The Hunting of the Snark,” and although it is the most dangerous of snarks, it is nonetheless an imaginary creature. This characterization did not stop positivism from becoming an important target for the critics of mainstream sociology in the 1960s and 1970s. “Positivism” encompassed at once Mills's “abstracted empiricism,” philosophical positions more directly indebted to either Comte or the Vienna positivists, and more generally the production of false certainties about social life. Merton's early satire did not immunize the Columbia team against the criticism. Neither did the fact that he introduced some Marxian influences into the functionalist corpus (a point Alvin Gouldner acknowledged in *The Coming Crisis*).

Merton was more liberal than Parsons—or perhaps better said, more to the left, since in many ways Parsons was also a liberal (if a rather conserva-

43. Stephen Cole (2004) has pointed to Merton's disinterest in testing the theories that stood behind concepts like “Matthew effect.” Cole may be right that Merton simply thought this a lesser task better delegated to lesser mortals. But it is also important to see Merton's engagement in providing usable tools—*bonnes à penser*, in Lévi-Strauss's sense, concepts good to think with—rather than only tested propositions for more or less positivist accumulation.

tive one). And while Lazarsfeld was in important senses an empiricist-positivist, he confounded the stereotype that this was an antipolitical position. He remained oriented by his youthful socialist convictions and more general notions of the practical usefulness of social science even as he insisted that researchers must avoid a short-circuiting of the connection between science and politics. More importantly, though, neither man addressed questions of fundamental social change. Even when taking up concerns for social structure, they did not address its largest parameters: capitalism, nationalism, global power structures. As meliorist liberals, they sought improvements in society within a perspective not dissimilar to Merton's notion of middle-range theory—making progress on one problem at a time.

Lazarsfeld was more drawn to the idea of social scientists as “experts” shaping policy—an ideal he shared with his student James Coleman. Coleman would go on to establish a remarkably productive team at Johns Hopkins in the 1960s. With Peter Rossi and Arthur Stinchcombe as other leading lights, the Hopkins group pursued something close to the Columbia agenda—with perhaps a stronger emphasis on policy relevance and methodological sophistication.⁴⁴ Merton was both less confident about this and more drawn to the purely intellectual dimensions of social science. Nonetheless, he was proud that his study of an integrated community informed Kenneth Clark's testimony in the landmark desegregation case of *Brown v. the Board of Education*. But with the signal exception of Merton's early work on science, neither Merton nor Lazarsfeld engaged the question of whether society could in basic ways be different. “Macro” questions about society as a whole were left largely to Parsons, within an intrafunctionalist division of labor. Merton and Lazarsfeld showed little interest in modernization theory, though some of their Columbia students and colleagues did—notably Seymour Martin Lipset and Immanuel Wallerstein.

The suggestion that the empirically measurable conditions of actually

44. Coleman's mobilization of his research to inform court decisions about school integration produced perhaps the single most famous impact of high quality sociological research on policy (made all the more interesting by Coleman's later research on white flight and private schools, and his concern that the policy his earlier research and testimony had informed was counterproductive). In addition, he pioneered mathematical modeling as a sociological research technique. Rossi pioneered risk assessment, the study of disasters, and scientific approaches to program evaluation (as well as conducting a “community study” with an accent on the question of policy and choice—focused on the politics of race and class in a nearby Maryland planned community). Stinchcombe made pivotal contributions to organizational research and the development of economic sociology, and also wrote an influential manual for the construction of middle range (or even slightly more specific) sociological theories.

existing social life did not exhaust the possibilities for social organization was central to critical theory, and more generally to critical alternatives to positivism and sociology's mainstream. Indeed, positivism became an omnibus term of accusation (and sometimes embrace) partly because of the influence of Lazarsfeld's sometime colleague Theodore Adorno and other Frankfurt school critical theorists. These figures had spent the war years in the United States, the early part of them in New York, with hospitality from the Bureau of Applied Social Research. Lazarsfeld involved Adorno in some of his research on radio communications and propaganda. The relationship was one of respect, although they eventually fell out. The issue was not so much theoretical differences as Lazarsfeld's conclusion that Adorno simply could not be practical in relation to empirical research. Lazarsfeld saw the researcher as an expert informing policy, while Adorno maintained a central European conception of the researcher as philosopher-intellectual. Adorno got huffy about Lazarsfeld's déclassé turn to “administrative research.” He found Lazarsfeld's integration into American culture and styles of work troubling, a threat to his very self-conception as intellectual. Lazarsfeld got tired of Adorno's unwillingness to get on with new work in the United States (rather than returning to recycled German work). He later wondered, though, whether he had failed as a manager, since Adorno played a more productive role in the studies that would become *The Authoritarian Personality*, with its famous f-scale and deployments of survey research. Adorno's brief transit through Columbia was not without influence. When he returned to Germany after the war, he actually tried to teach survey techniques (more cynically, he found that claiming to be an expert on empirical research methods was a valuable source of distinction). Indeed, the project of joining empirical social science to theory was basic to the Frankfurt Institute of Social Research, both before and after the war, even if perhaps more fully achieved in the Lazarsfeld-Merton years at Columbia.⁴⁵

Back in Germany, Adorno helped launch a “positivist dispute” in German sociology. This was a curious debate, because no important figure argued the case for positivism—certainly not Karl Popper, who was invited to speak to the German Sociological Association in 1961, explicitly refusing the label “positivist” and attacking inductivist and naturalist conceptions of science. Popper's position somewhat disarmed Adorno, who was to speak next, but he and his colleagues still challenged the logical positivists of early twentieth-century Vienna. They saw these as intellectually serious but mis-

45. On the Frankfurt school, see Wiggershaus (1994).

taken on two key issues. One was their faith in the unity of science—the Comtean project by which distinctions between the human and natural sciences would vanish as humans came to be understood entirely objectively. Building on Dilthey and Weber, the Frankfurt theorists insisted that such understanding of human actors could never be complete. Moreover, they argued that if pursued without a critical complement that gave greater respect to the distinctiveness of human beings and the importance of action, such positivism would inevitably do violence to humanity. Second, they objected to the positivists' notion of science as outside of history and free from social influence. This allowed the illusion of perfect scientific certainty, but that could only be ideological and potentially condone disastrous overconfidence (generally on the lines of being certain enough of ends to claim justification for troubling means, as in various twentieth-century projects of social engineering).

It was largely in the terms of this debate—though refracted through many circuits of reception—that “positivism” became the omnibus label for reductionist empiricism and a scientism that denied both social influences and social responsibilities.⁴⁶ Although both Lazarsfeld's students and his opponents were happy to see him exemplifying the positivist pole of this formative dualism, it partially distorts his work—mainly because the argument implied in the positivist/critical split is so poorly joined (witness Popper's resistance to it). It does so also because the later deployment of the opposition mapped it onto one between “apolitical” research and political engagement, obscuring the more precise distinction between the public role that Lazarsfeld sought for sociologists—as experts with objective knowledge—and the more directly political roles advocated by his critics, who would have had sociologists more often addressing the general public, and not only policymakers.

*Statistical Innovation, Symbolic Interactionism,
and Fieldwork at Chicago*

Lazarsfeld was a major influence on quantitative methods in sociology through the 1950s, but by the 1960s, other individuals and groups had taken the lead. The most prominent of these was probably Otis Dudley

46. This—and more generally engagement with the critical theory of the Frankfurt school—is also the source of reliance on the term *critical* to describe a range of perspectives challenging positivism and “establishment sociology” in the 1960s and 1970s. Much critical sociology was not specifically critical theory in the narrower Frankfurt sense. Marxism itself became important in a variety of different forms, and more “homegrown” radicalism and the

Duncan, though Hubert Blalock, Leo Goodman, Leo Srole, and many others were also important. Although Lazarsfeld had trained in mathematics as well as psychology and made important innovations in statistical analysis, his actual practice centered far more on data gathering. His approach was to find the best facts he could, innovating in ways of collecting them where necessary, and presenting them in a systematic fashion. Eventually the actual conduct of surveys would move out of sociology departments into the hands of specialists. And despite persistent worries over the quality of data and the influence of technical features of survey design, analytic statistics would become the primary focus of graduate methods courses.⁴⁷

For Lazarsfeld and others in the mid-twentieth century, analytic statistics meant starting with cross-tabulations and then developing ways of studying patterns of variation in distributions. Lazarsfeld's latent structure analysis (now more familiar as latent class models) improved, for example, on the sort of work earlier sociologists like Giddings (which tended mainly to consist of deriving average values for characteristics of whole social categories). The next generation of leaders in sociological statistics showed still more interest in analytical statistics—and benefited both from the spread of computer technology and the availability of new and better data sets. Some took up econometric techniques. More sophisticated use of probability estimates transformed thinking about causal attribution in sociology.⁴⁸ By the 1970s, hazard models would extend this work dramatically, making possible new kinds of dynamic analyses. Blalock, Duncan, Goodman, and others pioneered linear regression and its extension into path models, structural equation models, and event history models. While the general ideas of regression and path analysis had older histories and were used in other fields, Duncan both brought them into sociology in connection with well-recognized analytic problems—like measuring social mobility—and brought them to bear on newly available unit-level survey data.

social problems movement also informed both the critical turn in sociology and the U.S. New Left generally.

47. On the persistent questions about survey design, see Schuman and Presser (1981).

48. In many ways, the statistical innovations of the 1960s and 1970s centered on causal analysis; Blalock's *Causal Inferences in Nonexperimental Research* (1961) helped to pave the way. Blalock spent most of his career at the universities of Michigan, North Carolina (where he had earlier done his PhD), and Washington. Though he spent three years at Yale, the series of major state universities at which he taught is indicative of where much of the action was in the development of quantitative sociology. Odum had helped to pioneer this topic at North Carolina (as Ogburn had at Chicago). Wisconsin was not intensively quantitative until the 1960s, when William Sewell Sr. led the remaking of a department that previously had been headed by Howard Becker and Hans Gerth.

This made possible the extraordinarily influential study *The American Occupational Structure*, which both launched status attainment research and called attention to the power of the new statistical techniques.⁴⁹ Its appearance was followed quickly by the founding of *Sociological Methodology* in 1969 and *Sociological Methods and Research* in 1972 (with Edgar Borgatta of the University of Washington playing the leading role in establishing each).

It is instructive to recall how recently these major innovations in statistical analysis came to sociology and that they were more or less simultaneous with the “crisis” of mainstream sociology in the 1960s. Positivism (if that is the right word) was gathering steam even while it was under attack, and it arguably survived the attack better than the attackers did.

Duncan’s primary methodological innovations came while he was on the University of Chicago faculty. Just how many Chicago schools there have been depends on the interests and perspective of the enumerator—urbanists may count differently from other sociologists.⁵⁰ But in general terms, the first Chicago school was that associated with the discipline’s founding and the leadership of Albion Small, and continued into the heyday of Park, Burgess, and W. I. Thomas. The second was centered on the mid-twentieth-century years when Everett Hughes and Herbert Blumer taught at Chicago and W. Lloyd Warner held a joint appointment in anthropology and sociology. This appointment solidified a common engagement with ethnography for both sociologists and anthropologists at Chicago, at a time when the disciplines were pulling farther apart in the rest of the United States. Generations of field researchers were trained in the department, from Howard S. Becker and others who studied with Hughes and Blumer to those trained later by Morris Janowitz and Gerald Suttles (both Chicago products themselves). The fieldwork approach was also integrated—at least loosely—with the symbolic interactionist theoretical framework Blumer and others synthesized, drawing on the work of George Herbert Mead. At the same time, while “Chicago school” usually referred to the fieldwork tradition, Chicago was home to William Fielding Ogburn, Philip Hauser, and Leo Goodman (as well as Duncan) and a center for innovation in quantitative sociology, including especially demography and human ecology. Park’s human ecology tradition was systematized and renewed at Chicago by Duncan (and at Michigan by Amos Hawley). The post-

49. Blau and Duncan (1967). This built on earlier work in which Duncan developed the multiple indicator socioeconomic index (as a replacement for mere prestige ratings) as well as on his elaboration of path analysis. See Duncan (1961, 1966).

50. On Chicago, see Bulmer (1984a), Fine (1995), Abbott (1999), and the introduction to this volume.

war Chicago department was more heterogeneous than the common link of “Chicago” to urban ethnography would suggest, as important as that was.

Here, in short, was a distinctive competitor for the mantle of “mainstream.” Of course, Chicago was a collaborator as well as a competitor. Among its most distinguished faculty members was Edward Shils—who had helped invent both the Committee on Social Thought and the Committee on the Study of New Nations. Shils was central to the birth of modernization theory and had a hand in many of the other distinctive intellectual projects of the postwar era. And he was Parsons’s collaborator, not least on the influential *Working Papers in the Theory of Action*.⁵¹ Still, Chicago represented the old guard from which the founders of the *ASR* had sought to break away. It remained among the most prestigious and influential of departments. And as a major producer of PhD’s, Chicago was peopling other programs: notably Northwestern and Berkeley, and more generally a range of universities in the Midwest and West.

On the one hand, the Chicago fieldwork tradition carried forward a tradition of inquiry focused especially on the marginalized, disadvantaged, and rebellious. Even when studying elites, the Chicago fieldworkers were irreverent—especially compared to the more affirmative studies from the Columbia and Harvard functionalists. The contrast is evident in nearly simultaneous studies: *The Student-Physician* (Merton, Reader, and Kendall 1957) and *Boys in White* (Becker et al. 1961). This difference in style helped foster a greater tendency to write for the broader public and for undergraduate students, compared to the East Coast functionalists. The issue is not just prose clarity, for while it is easy to mock Parsons on that score (and masterful mockery helped Mills’s critique greatly), Merton’s prose was as limpid as any sociologist’s. The issue has more to do with descriptive detail, perhaps, and with interest in individuals as well as social patterns, but most of all with the intention to inform a general understanding of social life rather than either policy decisions or the accumulation of tested propositions in a positivist model. Many of the fieldworkers resisted professionalization or identified themselves as teachers and writers as well as sociologists. They

51. Parsons, Bales, and Shils (1953). Shils did do empirical work, perhaps most notably *The Intellectual between Tradition and Modernity: The Indian Situation* (1961), but he is best known as a theorist. In a sense, this makes him anomalous for a Chicago department that viewed “pure theory” with suspicion (and to be sure, Shils was a man of Chicago’s interdisciplinary committees, not primarily the Department of Sociology). Donald Levine, for example, was told as a student in the 1950s that one couldn’t make it at Chicago as a theorist and so had better do empirical work. Whatever the other merits of this advice (which he seems to have resented), it led to major studies of Ethiopia—including the classic *Wax and Gold* (Levine 1965).

sought to be not the government's "experts" but the voice of those they studied. They were seldom recipients of NSF support.

The fieldwork tradition was commonly linked to symbolic interactionism, which provided a loose interpretative framework. Few of the studies were theory driven, though, and to a considerable extent the fieldwork tradition was hostile to theory—especially grand theory of the Parsonsian variety, but even middle-range theory as well. There were efforts to develop symbolic interactionism as a theory, notably by Blumer. And interactionism came to be presented in textbooks as a theoretical alternative to functionalism—alongside "conflict theory," which was itself a loose euphemism for Marxist, Weberian, and other perspectives that challenged the consensus orientation of functionalism but did not necessarily have much else in common. It is possible that neither symbolic interactionism nor conflict theory would have figured as "theories" were it not for the establishment by Parsons and other functionalists of a specific idea of what a theory was and how it worked in securing the scientific status of sociology. What many fieldworkers drew from symbolic interactionism was less a theory than an injunction to pay attention to actual patterns of interaction among real people. "Respect the nature of the empirical world," Herbert Blumer repeatedly implored his readers. The way to respect that world, Blumer (1969, 32) maintained, was to go out into it and, through a "direct examination" and a "diligent effort," to put one's ideas to the test of its complicated reality. In this sense, interactionist research had an inevitably anthropological dimension. Doing sociology meant going out into public.

Blumer's "theorizing" notwithstanding, symbolic interactionism often came to mean "micro-sociology" as much as it meant a specific theoretical orientation. Indeed, there were arguments against theory and in favor of approaching fieldwork with as few preconceptions as possible. In this tradition, learning was often organized as much through the reading of previous case studies as through mastery of a theoretical apparatus. Likewise, it was not until later that there was a significant movement to formalize qualitative methods, including fieldwork, and this came under pressure to match the formalization of quantitative methods (see DeVault, chap. 5, this volume).

On the other hand, Chicago's newer quantitative wing helped to bring demography firmly into sociology. Demography developed in close relationship to quantitative methods, including not only survey research methods but also new analytic statistics. When the Rockefeller Foundation made funds available in the 1960s to establish population research programs, almost all the universities with major PhD programs in sociology were recipients. At Harvard and Columbia, however, sociology stayed mostly aloof

from research programs that developed more in public health. Demography did not develop into a major part of graduate studies or research in sociology, as it did at Chicago, Michigan, Wisconsin, North Carolina, and Berkeley, where there were closer ties between population research centers and sociology departments. Yet clearly, both demography and quantitative methods were emerging into the sociological "mainstream" in important ways.

The expansion was not primarily theory driven but rather supported by methodological advances, new large-scale data sources, and of course external funding. Internally, the field of population studies was rent by some of the same division between problem-oriented, often applied research and more abstract "pure science" orientations that divided sociologists generally. Funds from Rockefeller and Ford were made available mainly on the basis of the sense that "population problems" were of immediate importance—both because of a broad worry about overpopulation and, in more focused ways, because of the perception that population expansion stood in the way of economic development, poverty reduction, and environmental protection.⁵² There was, however, a conceptual framework that offered a notion of sociology in which population was central. This was human ecology, which was developed in this period especially by Otis Dudley Duncan (Hauser and Duncan 1959) and Amos Hawley (1950). Duncan provided the acronym POET to articulate the basic premise that human social life was based on the intersection of population, organization, ecology, and technology. Although this was not presented mainly as a challenge to functionalism (in the way that would be typical of the revival of Marxism in sociology a generation later), it did suggest an alternative approach to what Parsons was calling the "functional requisites" of society (Parsons, Bales, and Shils 1953). It also gave more emphasis to material conditions (though not in anything like the Marxist approach). And if Parsons's account of the social system had the advantage of articulating a specifically sociological level of analysis within which individual researchers could feel their particular studies gained general significance, the POET paradigm had the advantage of suggesting both closer links to other fields of science and greater practical relevance.

The fieldwork and human ecology dimensions of Chicago sociology converged in urban and community studies. These had been central since the earliest days of Chicago sociology. Community studies research was also

52. The Rockefeller Foundation came to its postwar concern with population issues in a trajectory shaped by its earlier engagements with eugenics as well as by neo-Malthusian fears and optimism about technological responses like the "green revolution."

important to sociology more generally, and there were other important traditions. Hans Gerth nurtured a Wisconsin school of community studies, the most prominent exemplar of which is probably *Small Town in Mass Society* by Arthur Vidich and Joseph Benschman. Robert and Helen Lynd's *Middletown* remains one of the most influential of all community studies. Community had also been a primary interest of Robert Lynd's Columbia colleague MacIver; Herbert Gans continued this tradition there. "Community" was an intermediate construct between individuals and small groups and society at large. So too were various specific sorts of "acting groups": for example, unions, newspaper writers, musicians, and gangs.⁵³ And so of course were more abstract constructs like formal organizations of bureaucracy, which grew in importance in the 1940s and 1950s.

While the precise place of Chicago sociology within the discipline's postwar elite would be a matter of continued debate among historians of sociology, its disciplinary importance clearly endured in various ways. The department remained highly ranked. It continued to produce larger numbers of PhD's than any other American sociology department, and only in the late 1970s and early 1980s was it equaled or surpassed by Michigan, Wisconsin, and Berkeley.

"Mainstream Sociology" Revisited

In 1978, Richard Flacks, who had his own contentious personal history with the Chicago department, coauthored an article with Gerald Turkel on the emergence of neo-Marxian perspectives in American sociology. In that

53. See Lipset, Trow, and Coleman (1956), Janowitz (1952), and Becker (1963). All of these were conceptualized more or less concretely, as specific kinds of groups. This contrasted with a growing emphasis in the postwar years on more abstractly conceptualized kinds of social phenomena—e.g., bureaucracy—rather than concrete groups. This was part of an effort to create a more scientific sociology, pursuing generalization by abstracting from particulars of cases. As dramatically different was the replacement of all specific attention to intermediate associations of various sorts—a core theme since De Tocqueville and throughout sociology's history—with the dichotomy "individual/collective." Parsons's approach to the social system certainly allowed for the existence of a variety of groups, and Parsons even implied this was a good thing. But his theory saw systematicity—and thus science—at the levels of personality, culture, and social system. These were alternatives to economic conceptions of abstract market systems (always the "other" against which Parsons constructed his approach). The various specific groups were only particulars to be understood by the general theory: community was a social system and thus an instance of the collective. The general process needed to be understood, and the shift from community to association reconceptualized in terms of pattern variables and modernization. But specific communities were mere particulars if they were of sociological interest at all.

article, Flacks and Turkel claimed that it was "no longer possible to speak of a mainstream in sociology or to argue that a monolithic consensus surrounds any perspective or paradigm." There had been, they suggested, an "unraveling of the postwar consensus in the discipline," an unraveling to which radical sociologists and other critics of the mainstream had contributed. The "crisis" that academic sociology faced in the 1960s had brought about a "climate of shattered confidence and consensus," and by the end of the 1960s those who dominated sociology in the immediate postwar period had been put "on the defensive" (Flacks and Turkel 1978, 198). By implication, of course, it had once been possible to speak of a coherent "mainstream" in sociology and to argue that there was a "monolithic consensus." To speak of something shattered is to imply that it was once whole. Yet in an important sense the "mainstream" was an invention of the very New Left that Flacks and Turkel sought to story, as was the wider use of the notion of "Establishment Sociology." Sociologists would continue to refer to the "mainstream," critically and otherwise—and not just retrospectively. At the same time, "Establishment Sociologists" would maintain their "confidence," although the shape of the sociological establishment, as Flacks and Turkel noted, was changing by the late 1960s and would continue to change in the decades that followed.

By the late 1970s, the field of American sociology had grown enormously. Throughout the postwar era, public universities played an increasingly important role within the discipline, with major departments at Michigan, Wisconsin, Berkeley, and Chapel Hill producing hundreds of PhD's throughout the 1970s alone. Both Michigan and Wisconsin had become exemplars of yet another version of the sociological mainstream, albeit one with close affinities with and connections to the quantitative wing at Chicago (see Steinmetz, chap. 9, this volume). In self-conscious contrast to—but certainly not in disconnection from—the professionalizing projects pursued here and elsewhere, the rising department at Berkeley sought a different strategy. As Berkeley sociologist Philip Selznick recalled, the "implicit mission" at Berkeley during the postwar years was "to turn marginal fields into mainstream fields" (Burawoy and VanAntwerpen n.d.). Under the leadership of Blumer and others, Berkeley engaged in what Seymour Martin Lipset would later dub a "catholic sociology," cultivating a plurality of sociological approaches and intellectual agendas. And by Lipset's lights, this emphasis on diversity had "created a time bomb that finally exploded," as the Berkeley department was beset in the late 1960s by internal struggles and political controversies, caught up in the crisis of a "self-destructive discipline" (Lipset 2001, 266).

Such narratives of disciplinary crisis and self-destruction, at times accompanied by an explicit yearning for a supposedly lost age of unity and consensus, bear striking resemblance to critical accounts of the postwar mainstream and its demise. Yet, as we have argued, critical discourses regarding “mainstream sociology” have tended to overestimate the degree of unification among the postwar “establishment” and thus have risked exaggerating the degree of disciplinary rupture wrought by the 1960s and 1970s. Was a “monolithic” postwar “consensus” really “shattered” by the sixties, or is the story of postwar sociology somewhat more complicated than that? Although we have not attended in detail here to the disciplinary effects wrought by the struggles of 1960s and 1970s, we have suggested that the postwar sociological elite was not the narrowly defined monolith it was sometimes imagined to be. Likewise, failure to establish deeper roots in earlier dissident sociological traditions may have undermined the rebels of the 1960s and 1970s, obscuring the more complicated realities of their disciplinary situation.⁵⁴

Writing in 1991, Richard Flacks reconsidered the assumptions the rebels had once made about their discipline:

We were, it turns out, wrong to believe that there was a self-conscious and powerful “establishment” in sociology that could or would mobilize real power against us. We were, in fact, wrong to think that sociology had become a crucial vehicle for maintaining social control. Indeed, we shared with analysts like Daniel Bell and other theorists of the post-industrial society an exaggerated belief in the strategic centrality of the university for shaping the society’s future—a belief that led us to think that our challenge to the discipline and to the university was more weighty than it turned out to be.⁵⁵

54. See Flacks and Turkel (1978, 195): “The polemic against Establishment Sociology in the US has a long history. It became a significant underground force after World War II as sociology became a consolidated academic discipline, and as sociological research became an institutionalized adjunct of policy and management.” Like others, Flacks and Turkel seemed to use the terms “Establishment Sociology” and “mainstream sociology” almost interchangeably. Mills, as we have suggested, chose to present himself as bringing a completely new critical vision to sociology, making less of his previous engagements with pragmatism than he might have (perhaps maintaining distance from the Chicago school) and also linking his project to Weber and Marx (thus appealing, like Parsons, for European prestige). He also chose to work at Columbia and in New York in order to be at what he took to be the “center” of American intellectual and public life. For more on Mills, see Becker (1994).

55. Flacks (1991, 25). A page later, however, Flacks writes: “If there was an establishment sociology twenty years ago, we helped do it in; and so, for good or ill, and despite rearguard resistance in some departments, the field is to some extent ours.”

By the early 1990s, Flacks realized that it was somewhat anachronistic to speak of a sociological “establishment.” But what of the sociological “mainstream”? While the discourse of “mainstream sociology” still resonates widely today, it remains difficult to call up the most appropriate label for its opposites or “others.” Are they tributaries? Eddies? Cataracts? Narrows? However suggestive, none of these metaphors seems likely to capture the imaginations of graduate students or critical sociologists wondering about how to figure their place within the discipline. References to “Establishment Sociology,” especially common in the wake of Gouldner’s *The Coming Crisis*, dwindled in the 1980s and have now more or less disappeared. Yet in the same period the discourse of “mainstream sociology” has proliferated and continues to be common throughout the discipline, even as its users and referents have become more diverse.

The discourse of the “mainstream” has shifted along with the discipline itself. As the theoretical, methodological, ideological, and even regional and institutional shape of the sociological “establishment” has, in many ways, been transformed, so has the ever elusive (if not illusive) “mainstream.” What is mainstream sociology today? Is it in public or private universities, mass or elite sociology? It is often difficult to say. Yet the word is still bandied about throughout the discipline, both as both a positive and negative classification. As one sociologist told us: “When you are trying to make a new senior hire and a dean needs to see a grant record, you use the word ‘mainstream’ to indicate admirable strength of character and likely productivity; when you want to hire somebody who is creative and unorthodox, you say that they ‘challenge the mainstream in a responsible manner’ or some such nonsense. It’s mostly ideological claptrap.”

No doubt the tendency of “mainstream sociology” to function as a floating signifier served to promote its diffusion, especially in the 1960s and 1970s. If there was ever a time when “mainstream sociology” had a fairly stable primary meaning, it was between Mill’s 1959 critique of the twin evils of “grand theory” and “abstracted empiricism” and Gouldner’s 1970 declaration that the rebels were winning. The key targets of critics of the “mainstream” were frequently the same “three great figures of the Capitoline triad” that later figured prominently in Bourdieu’s retelling of the rise of the new sociological establishment. As one veteran of the earlier struggles put it: “I doubt there is a mainstream anymore, ever since Parsons died. Surely Merton, Lazarsfeld, Parsons, Shils, and their followers in the ’50s and ’60s embodied the mainstream, even if they themselves argued against such a notion. That kind of hegemony isn’t available anymore, so far as I can see.”

Yet the desire to claim and to contest the alleged mainstream remains strong. Indeed, something like the notion of “mainstream sociology” is perhaps built into the very disciplinary project that establishes a struggle over authoritative knowledge, legitimate intellectual perspectives, and the conditions and rewards of sociological work.

Although the recurrent preoccupation with “mainstream sociology” may thus be seen as a product of endemic struggle over the state and structure of American sociology, it is a preoccupation that remains liable to obscure as much as it reveals, thereby contributing to oversimplified understandings of both the discipline’s postwar history and its present condition. The application of the “mainstream” label, we have argued, was rooted in a specific discursive formation within the field of American sociology. It functioned as a sort of classificatory epithet, lumping together a range of otherwise disparate dominant sociologists and sociologies under one undifferentiated, and usually derisive, category.⁵⁶ “Mainstream sociology” was simultaneously a category of historical analysis and a discursive strategy mobilized largely by a new generation of opponents and critics of the discipline’s postwar establishment.⁵⁷ But it was also readily appropriable after the rebel ascendancy by those who sought to resume a more “professional” disciplinary project. In the midst of such disciplinary developments and transformations, discourses of mainstream sociology became a pervasive and widely circulating piece of disciplinary “common sense,” a key component of a “folk theory” of American sociology’s postwar development, subsequent crisis, and ensuing attempts at disciplinary reconstruction.

In seeking to historically situate and critically assess the emergence and circulation of such discourses, our aim has not been to suggest that they are

56. As Murray Hausknecht (1972) pointed out in his review of J. David Colfax and Jack L. Roach’s *Radical Sociology*, applying the label “mainstream sociology” had at times the effect of suggesting that a supposedly radical work of sociology was in fact not radical enough, particularly if it did not break sufficiently with mainstream methodologies. It was thus a classificatory epithet aimed not simply at the key figures of the sociological establishment but sometimes at critical sociologists who would otherwise have in various ways represented alternatives to that establishment.

57. Thus, at least at the beginning, to refer to the “mainstream” or “establishment” sociologist was generally not to engage in a discourse of self-identification, although it may be argued that the use of such terms often involved the staking of a specific sociological identity. Just as Gouldner’s mind boggled at the thought of a Parsonian hippie, so too would it be baffling to imagine Smelser or Lipset identifying with a project called “Establishment Sociology.” Indeed, from the perspective of many so-called mainstream sociologists, the sociology they practiced required no modifier. As Kingsley Davis (1959, 771) put it in his ASA presidential address, “structural-functional analysis is sociological analysis.”

entirely or essentially without merit. While some invocations of the sociological “mainstream” may indeed amount to little more than “ideological claptrap,” it is also clear that discourses of mainstream sociology can sometimes serve an effective and productive purpose, not least in the hands of those seeking further disciplinary transformation, efforts that frequently revolve around the critical appraisal of sociology’s recent past and present possibilities. From the point of view of sociologists writing and rewriting the history of their discipline, however, the relevant issue is what sort of critical purchase or epistemic gain might be afforded by an analysis that employed a suitably reflexive conception of the disciplinary mainstream. The challenge for historians of sociology engaged in such analysis would be to craft a conception of mainstream sociology that succeeded in breaking with the potentially misleading commonsense understandings associated with the term, while not being so discontinuous with these understandings as to be unrecognizable. With this challenge in view, we close with a brief consideration of one significant effort to make critical use of the concept of mainstream sociology in the writing of American sociology’s history.

In one of the few book-length treatments of the history of American sociology, Stephen Turner and Jonathan Turner have offered an account of the “postwar synthesis” not altogether different from the one we have proposed here. In the closing chapter of *The Impossible Science*, Turner and Turner argued that while Parsons, Merton, and others made an ambitious attempt to unify the discipline of sociology in the years after World War II, “there was never a single, clear, consensual model of the new sociology.” Indeed, they suggested, what consensus there was in this period was “both extremely limited and extremely tenuous.” The Turners nonetheless viewed such synthesizing attempts as forwarding a “highly persuasive general image of the scientific future of the discipline,” an image that they associated with a new “mainstream” of American sociology, a mainstream linked to developments in both “theory” and “quantitative methods” (Turner and Turner 1990, 188–92).

In a subsequent article, which was in part a response to critics of the book, Stephen Turner elaborated substantially on this conception of the “mainstream,” writing that “the basic *historical* story line of *The Impossible Science*, however concealed,” was a narrative of “the rise and triumph of mainstream sociology” (Turner 1994, 46). Emphasizing the methodological side of the story and referring to “mainstream sociology” as a “methodological tradition” whose “hidden source” was Franklin H. Giddings, “a figure neglected, abused, or ignored by virtually the whole historical literature produced by sociologists on American sociology,” Turner referred to other

major and much-discussed developments in postwar American sociology—including both “Parsonianism” and “the Lazarsfeld-Merton BASR survey model”—as “sideshow” that had been of “little long term significance” (48, 43, 46). By contrast, “the long quest to create a quantitative science” of sociology, while largely an intellectual failure, had been a huge political success for a privileged and dominant network of elite sociologists—though this success would be repeatedly and hotly contested by the discipline’s recalcitrant “underclass” (55). Thus, if there was a “core” to mainstream sociology, it was to be found in a quantitative tradition that was linked together historically by tacit knowledge passed down from well-positioned and influential teachers to doctoral students, through “the Apostolic succession of apprenticeship” (50).

Although the details of Turner’s provocative and fiercely argued thesis cannot be neatly summarized or adequately reproduced here, his narrative regarding the “origins” and “success” of mainstream sociology both intersects with and disconnects from the folk theory we have described, in interesting and potentially instructive ways. First, on Turner’s account, mainstream sociology has been from its very beginnings the project of a disciplinary elite. Like the critical sociologists of the 1960s and 1970s, Turner represented the “mainstream” of sociology not as a majoritarian enterprise but rather as a minority movement that attempted to impose its scientific vision of sociology from the top down. Aided by its access to substantial resources and its establishment in major journals and powerful graduate departments, this movement nonetheless faced stiff resistance from those in sociology whom it sought either to reform or to exclude. “The rest of the discipline,” Turner writes, “did not go along gently,” providing one explanation for “the distinctive awfulness of sociology’s internecine squabbling” (1994, 46, 55).

While one might expect the presuppositions and sociological practices of the “mainstream” to be defined as those that were most widely accepted throughout the discipline, in fact the term has been used repeatedly to refer to the prerogatives and attempted dominance of a narrowly defined disciplinary elite. This was true even of those critics of mainstream sociology who self-conceived as critically avant-garde or cutting edge, situating themselves outside the mass of the sociological mainstream, and thereby conflating an opposition to the top of the disciplinary hierarchy with an opposition to the middle or center of the disciplinary majority.

Although Turner made no such claims to cutting-edge status, his association of mainstream sociology with a disciplinary elite did not stray far from the commonsense understandings embedded in the discourse of

mainstream sociology. A second element of his argument represented a more distinct departure, however. Clearly delimiting what he meant by “mainstream sociology,” Turner implicitly set his conception of the mainstream apart from the numerous understandings that had placed Bourdieu’s “Capitoline triad”—and particularly Parsons—at the center (although his article dealt in detail with the case of Parsons, as did *The Impossible Science*). This displacement of the supposedly hegemonic postwar theorists had the virtue of giving Turner’s rendering of mainstream sociology greater analytic clarity. But it also risked disconnecting that rendering from some of the most prevalent understandings of what—and who—the “mainstream” was. In and of itself, this need not be seen as problematic. Indeed, much of Turner’s argument might be read as a sustained interrogation of the received understandings of mainstream sociology we have critically examined here. Yet because he never explicitly acknowledged it as such—because, in other words, he did not seek to account not simply for the rise of something he called mainstream sociology but also for the historical emergence and circulation of the “mainstream” label itself—the effects of his interrogation were muted. When, a decade after his article was published, renewed discussion of “mainstream sociology” surfaced in the midst of debates over the promotion of “public sociology,” the terms of those debates were still largely defined by conceptions of mainstream sociology inherited from the critical discourses of the 1960s and 1970s. This was in part the result, to be sure, of the fact that many contemporary sociologists in the United States do not read widely in the history of American sociology. But it might also be seen as a product of a general failure on the part of historians of sociology to adequately historicize the master concepts around which they have built their historical narratives and explanations. Historicizing such concepts—especially when those concepts have wide purchase throughout the discipline—would be one way to engage less historically inclined sociologists in direct dialogue. This project of historicizing sociology should also be a central element of any attempt to critically examine the history of the discipline.

American sociologists in recent years have shown an increasing interest in the history of their own discipline. As American sociology’s past, present, and future—public, professional, and otherwise—continue to be debated, historians of sociology can and should seek to historicize and critically inform the stakes of those debates. A central claim of this chapter has been that debates regarding the postwar sociological establishment and its challengers would benefit from a more historicized and more reflexive conception of mainstream sociology. Engaged in struggles over the future

of their discipline which were at least in part “struggles over classification,” the insurgent sociologists of the 1960s and 1970s projected the politicized categories of their present onto the past, retelling the history of postwar sociology in the process.⁵⁸ For them, the notion of “mainstream sociology” was as much a discursive strategy as it was an objective historical category. It was a resonant rhetorical tool, a critical classification, constituted and mobilized by the critics, and employed at various points in battles over the present meaning and future shape of the discipline. The pasts it projected in the midst of such struggles should be seen in this light.

Acknowledgments

Thanks to a variety of interlocutors for sharing their reflections on “mainstream sociology,” including Gabi Abend, Howard S. Becker, Michael Burawoy, Randall Collins, Arlene Kaplan Daniels, Kai Erikson, Daniel Geary, Todd Gitlin, Charles Lemert, Harvey Molotch, David Nasatir, Tom Scheff, Alan Sica, Dorothy Smith, Judith Stacey, Barrie Thorne, Stephen Turner, Immanuel Wallerstein, Erik Olin Wright, Mayer Zald, Harriet Zuckerman, and participants in the NYU workshop on the history of sociology.

58. For commentary on the danger of this approach in the history of sociology, see Camic (1994). For “struggles over classification,” see Bourdieu (1991b).

[ELEVEN] From Relevance to Irrelevance: The Curious Impact of the Sixties on Public Sociology

Doug McAdam

During his term as ASA president, Michael Burawoy (2005b) called for much more attention to, and debate about, the role of “public sociology” in the discipline. While *any* attention to this issue is welcome in my view, there is no shortage of ironies associated with the contemporary conversation about the matter. As we internally debate the merits of different versions of public sociology, the external—or public—disregard for sociology is palpable. One fears that even if we were to embrace a credible version of public sociology, we would struggle mightily to get a host of important real-world constituencies to care very much.

It was not always this way. Without overstating the case, I think it fair to say that in the quarter century following World War II, sociology enjoyed a public presence and policy resonance that far exceeds its influence today. And yet, or perhaps because of this, there was little self-conscious conversation within the discipline about the nature of, or call to, public sociology. Already broadly committed to various forms of public sociology, sociologists apparently felt little need to discuss or debate the matter.

The question is, how did we get from the engaged, successfully public sociology of the immediate postwar period to the current desultory state of affairs? A full answer to this question is beyond the scope of this chapter. Instead, I confine myself to just one strand of the story—the ironic impact that the baby boom cohorts had on the tradition of engaged, public sociology that so thrived in the post-World War II period.

The chapter, then, takes up one aspect of a very complicated legacy, that is, the impact of the 1960s on contemporary sociology. I begin with two clarifying comments and a caution. The first clarifying comment concerns the decidedly nonchronological definition of the sixties that I adopt in the chapter. The sixties that I refer to here is not the ten-year period, 1960–69, but that distinctive bundle of political, social, and cultural trends that we popularly—if erroneously—associate with the decade. In this more amorphous cultural sense, the sixties is very much a moving target, an evolving sensibility that took hold at very different times in very different places and