[NINE] American Sociology before and after World War II: The (Temporary) Settling of a Disciplinary Field

George Steinmetz

The central problem I address in this chapter on American sociology between the early 1930s through the mid-1960s is the shift from an epistemologically splintered discipline before World War II to a more hegemonized situation afterward.[1] I analyze sociology *internally* in terms of its fieldlike qualities (or lack thereof), in Bourdieu's (1985b) sense, asking about the emergence of agreed-upon definitions of unequally distributed social scientific capital. The drawback to a strictly field-level analysis, however, is that it cannot explain why certain definitions of scientific capital—in this case, broadly positivist positions—are ascendant in particular historical periods. Specifically, the Bourdieuian approach cannot explain why methodological positivism was only able to triumph after the war. The actor-network approach pioneered by Bruno Latour is enlightening about the sorts of strategies scientists use in seeking to expand their influence or scientific capital, but it cannot explain why the same techniques succeed in one historical context and fail in another. Both of these compelling approaches to the sociology of science need to be supplemented by attention to the ways in which epochal changes in the organization of society influence the ways scientists think about their objects of inquiry and the nature of scientific activity.[2] The extra-field determinant that is emphasized in most of the existing literature on the sociology of the social sciences is money. The story of social science scientism rightly emphasizes the massive influx of federal funding during and after World War II, which bolstered the impact on sociologists' epistemological orientations of the already significant levels of private funding (Turner and Turner 1990; Kleinman 1995;

1. See Steinmetz (2005a, 2005g) and Steinmetz and Chae (2002) for a more detailed elaboration of this use of the term *epistemological.*

2. This is not to say that Bourdieu entirely ignored the effects of the environing "field of power" on production internal to scientific fields (see, for example, Bourdieu 1991d). My contention is rather than Bourdieu did not adequately theorize the relations between the specific field (e.g., philosophy, sociology) and everything that lay outside the field; see Steinmetz (forthcoming b) for discussion of this point.

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Price 2003). But while funding was certainly *part* of the conjuncture that catapulted methodological positivism to its leading position in postwar sociology (and in political science, psychology, and some other social science disciplines), material resources alone do not provide a sufficient explanation. After all, significant research money for "scientific" forms of sociology had been available before World War II, both from private sources like the Laura Spelman Rockefeller Foundation and from the federal government in the New Deal work relief programs (Ross 1991; Camic, chap. 7, this volume). Moreover, the styles of sociology geared toward serving the state and industry and the flood of resources that supported those activities were *both* part of the broader Fordist social formation that emerged after the war.[3] In other words, the causal arrows running between funding and social epistemology are not unidirectional, and these relations were themselves mediated through the larger societal complex of Fordism.

Fordist forms of societal regulation resonated powerfully with social science positivism. The spontaneous social epistemologies that were encouraged by Fordism contributed to the epistemic realignment in sociology by making positivism seem more plausible. American sociologists participated individually in Fordist forms of societalization in their everyday lives, and their personal assumptions about the social came to be more closely aligned with positivism than had been the case before the war. Fordism as a mode of social regulation insisted on the ontological reality of "the social" as an object, and this was the very object over which sociology as a discipline claimed jurisdiction. As a result, sociologists were especially fixated on the predictable, repeated regularities of social existence inside the U.S. metropole. By contrast, disciplines like anthropology and area studies that claimed jurisdiction over the global South were confronting the turbulence of decolonization and revolution rather than a world that resonated with epistemological notions of general laws and "constant conjunctions of events." Analyzing the relations between Fordism and the social sciences undermines the internalist versus externalist division in the study of science insofar as the internal workings of sociology were intrinsically linked to more encompassing patterns of social life.

The burden of my argument in this chapter is to track the postwar narrowing of sociology's intellectual diversity or, more precisely, the shift from a relative equality between nonpositivist and positivist orientations in terms of scientific prestige to a condition in which positivism as defined

3. On Fordism in the United States see the foundational text by Aglietta (1987); for a brief overview see Steinmetz (forthcoming a).

here was clearly dominant. Because I have explored the causal linkages between Fordism and postwar positivism elsewhere (Steinmetz 2005a, 2005g, 2005d, forthcoming b) this chapter focuses on establishing in more descriptive detail the epistemological dimensions of the midcentury shift. Specifically, I examine the epistemological characteristics of some of the leading sociology departments and disciplinary publications of the middle third of the century.[4] In the conclusion I summarize my arguments and findings from previous studies concerning the specific ways that postwar Fordism seemed to provide immediate confirming evidence for social science positivism.

Methodological Positivism in American Sociology before World War II

Methodological positivism was already well represented in U.S. sociology before 1945. By *positivism* I am referring neither to pure logical positivism (Ayer 1959) nor to Comte's (1975) classical doctrines but rather to a historically specific set of practices, conventions, and assumptions about social science that emerged between the late nineteenth and mid-twentieth centuries and that continues to evolve and flourish in the social sciences today.[5]

There are three main dimensions of methodological positivism. The first element is an *epistemological* commitment to covering laws, that is, to the identification of Humean "constant conjunctions" of events, or to the

4. Given the ongoing discussions about whether this postwar formation has in fact disappeared, I will not try to establish a specific date for its dissolution. Calhoun (1996) points implicitly to the continuing domination of the discipline by the position I call methodological positivism, and Somers (2005) suggests that a neopositivist formation is currently being consolidated within U.S. sociology. I elaborate two distinct futures for sociological positivism in Steinmetz (2005a, 2005g) without forecasting either of them.

5. This definition is based primarily on discussions in the critical realist philosophy of science, but it is also adjusted to the peculiarities of actual sociological practice. Alternative terms that attempt to capture a similar cluster of scientific tendencies include "instrumental positivism" (Bryant 1985), "objectivism" (Bannister 1987), and "standard" (Mullins and Mullins 1973) or "mainstream" (Calhoun and VanAntwerpen, chap. 10, this volume) sociology. Because these alternative terms neglect to differentiate between empiricist *ontology* and positivist *epistemology*, however, they cannot register more recent changes in sociological positivism, especially the increasingly widespread combination of depth-realist concepts (that is, *nonempiricist* ontology) with a commitment to general laws (*positivist* epistemology); for one defense of this version of positivism see Kiser and Hechter (1991). A comparative overview of the actually existing forms of positivism in the various U.S. social science disciplines during the twentieth century is provided by the essays in Steinmetz (2005e); Abbagnano (1967) compares philosophical definitions of positivism.

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probabilistic variants of covering laws that were accepted as legitimate by logical positivist philosophers in the mid-twentieth century. This is the central, defining element.

The second component is an empiricist *ontology,* according to which scientific statements link empirically observable events. The terms *logical positivism* and *logical empiricism* were almost interchangeable during the period discussed in this chapter. By the same token, epistemically positivist sociology was usually empiricist. In more recent decades we have seen the emergence of nonpositivist empiricism and nonempiricist positivism. Postpositivists like Foucault reject "depth hermeneutics" in favor of a kind of neoempiricism, whereas new versions of social science positivism define general laws as linkages between nonempirical depth-realist mechanisms and surface-level events.

A third component or set of assumptions stems from *scientism,* that is, the belief that the social and natural sciences should approach their objects of study in an identical fashion. For the social sciences, this premise means that its objects of study can be treated as brute material facts whose identity is independent of what people think about them. It also means that social facts, like natural ones, are subject to "invariable natural Laws" (Comte) independent of time and place.[6] Scientism's specifically methodological implication has been that the social sciences should strive to become quantitative and experimental like the natural sciences and should eschew normative evaluations.

Several historians have maintained that this broadly "methodological" syndrome was prevalent in U.S. social science during the first half of the twentieth century (Bannister 1987; Bryant 1975, 1985; Ross 1991).[7] Stephen Turner (1994) presents a thesis of strong epistemic continuity that dates from Columbia sociology department founder Franklin Giddings through to the present. This persistence, according to Turner, is maintained by a set of tacit and explicit commitments, passed on from generation to generation, privileging quantitative data (especially surveys) and statistical techniques.

6. Bryant (1975) identified two distinct traditions of positivism: Comte's version and the strand running from Locke and Hume through to Mach and Pearson, twentieth-century logical positivism, and finally to Ernest Nagel (1961/1979). The depth-realist version of positivism was already present in Carnap's later writing (Steinmetz 2005e). With respect to the doctrine of invariable laws (whether social or natural), Comte's position is identical to this more recent one. Note that my use of the term *scientism* here differs from Camic's in chapter 7 of this volume; he connects it to the turn toward more empirical research in 1920s sociology.

7. I am using the adjective *methodological* here in the sense of *méthodos,* which refers in philosophical contexts to the "pursuit of knowledge, investigation," and by extension "a plan or strategy for carrying out an investigation" (Baxter 2002, 42–43).

Other writers emphasize the adherence of many other founders of U.S. sociology to a version of positivism inspired by Karl Pearson, whose own epistemological views were indebted to Ernst Mach.[8] Franklin Giddings trained a large number of sociologists who went on to shape the leading sociology departments in the United States.[9] During the interwar period, numerous American sociologists endorsed what at the time was called the "natural science" perspective, that is, the "naturalist" view that sociology should pattern itself on the biological and/or physical sciences.[10] In his presidential address to the American Sociological Society in 1926, John L. Gillin (PhD, Columbia, 1906) revealed the positivists' disciplinary ambitions, stating bluntly that "the application of scientific method and the increased emphasis upon objective data have been acting as *selective* agents in consigning these *enemies* of sociology"—theorists and social reformers—"to a deserved desuetude."[11] Philosopher Otto Neurath (1931) was already defending a version of social science based on logical positivism before the war. The positivist philosophers' most influential statements came afterward, culminating in Ernst Nagel's widely read *Structure of Science* (1961). Sociologist George Lundberg, who taught at the University of Washington from 1945 until his retirement in 1961, promoted philosophical foundations for the discipline based on the Vienna school of logical positivism (Lundberg 1939a, 1939b, 1964; Platt and Hoch 1996). Lundberg had earned his doctorate with Giddings's student Stuart Chapin (PhD, Columbia, 1911) at Minnesota in 1925. In his 1943 presidential address to the sociological association, Lundberg referred to the distinction between the "vague processes by which [scientists] arrive at hypotheses" and the rigorous "context of verification." This was an allusion to the distinction between the "context of discovery" and the "context of justification" proposed by philosophers Hans Reichenbach (1938) and Karl Popper (1934). Lundberg's Washington colleague Stuart Dodd (1942) was perhaps the most scientistic adherent of methodological positivism in twentieth-century U.S. sociology.[12] And lest we dismiss positivism as restricted in this period to the Columbia depart-

8. See Platt (1996, 71–72, 76); Giddings (1896); Mayo-Smith (1895). The exalted status of Pearson among these founders and their students (and students' students) is suggested by Stouffer's remarks (1958); see also Bulmer (1984a, 176, 179).

9. These included W. F. Ogburn, Frank A. Ross, F. Stuart Chapin, and Stuart A. Rice.

10. For exemplary naturalist statements by sociologists in this period see Bain (1927, 414; 1935, 486), Cobb (1934), and Chapin (1935c).

11. Gillin (1927a), quoted in Oberschall (1972, 242); my emphasis. The adjective "selective" indexes the prestige of social Darwinism in early American sociology.

12. Dodd's influence and reputation within sociology should not be overstated, of course (Platt 1996).

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Epistemological Stalemate in American Sociology

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ment and to the sociological provinces, as some have suggested, we should recall that even the ostensibly antipositivist Talcott Parsons insisted in the 1930s that sociology's goal should be the discovery of "analytical laws" that formalize "a uniform mode of relationship between the values of two of more analytical elements."[13] In 1943 C. Wright Mills (1943) assailed American sociology textbooks for their atomism, empiricism, and doctrines of value neutrality. An analysis of sociology textbooks from this period finds widespread agreement on the need to emulate the natural sciences (McCarthy and Das 1985, 27–30).

A closer look at the pre–World War II period reveals, however, that scientistic positivism was far from hegemonic in the discipline or, more importantly, in the leading departments, during the 1930s and the first half of the 1940s.

Epistemological Stalemate in an Unsettled Field: American Sociology, 1930s–45

American sociology was particularly riven during the ideologically turbulent 1930s.[14] There had already been a decline in the use of the Chicago-style case study, leading to a methodological and theoretical interregnum (Lengermann 1979; Kuklick 1973; Wiley 1979). The Chicago approach was not simply replaced by structural functionalism, as has sometimes been suggested; instead, "grand theory" now coexisted with the "ideographic" case study, both of which were coming under attack by the "natural science" approach. Explicit, philosophically sophisticated resistance to scientism emerged in the discipline during this period (Evans 1986–87, 119). The result was an epistemological stalemate or, better, a sort of pluralism. No single epistemological/methodological position was dominant in the leading departments during the 1930s and early 1940s, with the exceptions of Minnesota and UNC–Chapel Hill.

13. Parsons (1937/1949, 622). Despite Parsons's emphasis on values or norms, he fell into an *objectivist* analysis of the subjective, as Camic (1989, 64–69) argues, reducing it to the single category of the means-ends schema (see below).

14. Many writers on this period acknowledge U.S. sociology's epistemic disunity, although often with disapproval; see, for example, Kuklick (1973), who refers to an "identity crisis" in the profession between 1930 and 1945, based partly on "intellectual obsessions." It seems to me sociological, however, to treat disunity per se as identical to crisis, since all social fields (and all semiotic systems) are constructed around differences. What is distinctive about the 1930s is that no single position was dominant in sociology, lending the field a degree of openness that is unsettling to the same people who have been decrying sociology's "crisis" since the 1970s (see Steinmetz and Chae 2002).

Any interpretation of this pre-1945 lack of consensus would certainly need to attend to both external and internal developments. Outside the academy, wrenching social and economic disruptions provided tactile evidence against any project of subsuming social life under repeatable, general laws (Arendt 1994). The decline of external sources of research support undercut the momentum of many big science projects, although New Deal work relief programs partly compensated for the drop-off (Camic, chap. 7, this volume). The influx of intellectuals from fascist Europe brought a greater level of philosophical sophistication to American sociology, including interest in logical positivism as well as its well-informed critics. For example, Herbert Marcuse's first book in English, *Reason and Revolution* (1941), included an extended critique of positivism. The personnel of American sociology departments became more diverse and less rural although still overwhelmingly white and male.[15] The sociological association was riven into different camps fighting against Chicago's domination, leading to the creation of the *American Sociological Review* and various factions, specialty sections, and regional associations (Camic, chap. 7, this volume; Turner and Turner 1990). Critiques of capitalism, although not inherently linked to antipositivism (witness the scientistic formulations of much Marxist analysis in that period), may have introduced some skepticism about foundation funding, which at the time tended to reward positivist approaches (Ross 1991).

My aim in revisiting this period is not to explain its fractured condition, which would require a different and much longer analysis, but simply to establish it in order to set up a contrast with the postwar era. Sociology between the 1930s and 1945, unlike the discipline during the two postwar decades, was not a well-ordered, hegemonized field. After 1945, by contrast, the methodological positivist definition of field-specific capital came to be almost universally *recognized* by all members of the field, regardless of whether they approved of it or adopted it themselves. The difference between the pre- and postwar periods does not have to do with the *availability* of relevant ideas and procedures, all of which were present even in the nineteenth century. What differed was the effectiveness with which adherents of methodological positivism could defend their position as a general measure of scientific capital.

Before trying to establish the distinctiveness of pre-1945 U.S. sociology

15. Whereas a quarter of the authors of *AJS* articles between 1895 and 1900 were members of the National Conference of Charities and Corrections or the American Prison Congress, this percentage decreased to 4 percent in the 1935–40 volumes (Oberschall 1972, 204, citing Sutherland 1945).

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it is important to recall that the multivocality of discourse, perception, and practice is a precondition for the functioning of any field in a sociological sense. A discipline can be divided and heterogeneous and still be "structured in dominance." To use a more current sociological language, a discipline that is internally fragmented can still be structured like a field, with widely acknowledged definitions of distinction. Any social field combines *heterogeneous* practices and perceptions with *homogeneous* principles of domination. Indeed, without some internal diversity (for example, disagreements about epistemology within a social science) there would be no raw material that groups seeking domination could wield as weapons of distinction. Scientific domination does not work by means of intellectual *Gleichschaltung* or an all-encompassing paradigm, along the lines proposed by Thomas Kuhn (1962), by Althusser in his theory of ideology (1971), or by Horkheimer and Adorno in their work on mass culture (1944). On the contrary, for a field to be configured in Bourdieu's sense, new differences will be invented or discovered even where they had not been previously perceived. The opposite of a well-structured field is an unsettled field, that is, an assemblage of practices in which no single definition of cultural capital holds sway.

The methodological upshot is that a historical sociology of sociology cannot establish the existence of a dominant intellectual structure simply by examining undergraduate textbooks (e.g., Inkeles 1964, 8–9), the topics of master's and doctoral theses (e.g., Bain 1927), the role of influential leaders like Giddings and his students and students' students (e.g., Turner 1994), or citation lists of the most famous sociologists (e.g., Oromaner 1969), although all of these provide important bits of evidence. Conversely, a historical sociology of sociology cannot establish an open, pluralistic, or fragmented condition simply by pointing to the existence of divisions, conflicts, debates, or dissidents. The mere presence of a C. Wright Mills in postwar U.S. sociology, centrally located at Columbia, does not tell us much about scientific power or scientific pluralism. Nor does the fact that there were as many opponents as there were supporters of the "natural science" approach among ASA presidents during the 1945–65 period reveal a lot about the field's power structure, since all members of the ASA could vote in presidential elections, including those located in less influential departments.[16] Book awards are also an ambiguous criterion of status in an article-

16. None of these "opponents" among ASA presidents rejected the methodological positivist formation vigorously, except for Sorokin late in his career (1956/1976; see below). Partial opponents of the natural science perspective among ASA presidents included Louis Wirth, E. Franklin Frazier, Robert C. Angell, Florian Znaniecki, Herbert Blumer, Howard P. Becker, Everett C. Hughes, Pitirim Sorokin, Talcott Parsons, and Robert Merton.

driven field such as that of (postwar) American sociology.[17] The ASA's main book award was named after Robert M. MacIver from 1956 to 1968 and after Pitirim Sorokin between 1968 and 1979. Both writers were associated with elite universities and leading departments, and both rejected the positivist trends in the discipline. MacIver was a "decisively antiscientistic" critic of "neopositivist methodology" all along, and Sorokin had turned harshly against the scientistic mainstream in the 1950s. Some responded by calling Sorokin shrill and eccentric, and indeed, his position in *Fads and Foibles in Modern Sociology* (1956) was eccentric in that decade.[18]

It makes more sense to look at the epistemological positions and scientific politics that were associated with the top-ranked departments and leading sociological publications.[19] Both of the leading journals, *American Journal of Sociology* (*AJS*) and *American Sociological Review* (*ASR*), were edited before 1945 by sociologists who were critical of methodological positivism. Opponents of the "natural science" approach in sociology were not only employed by the leading departments but also headed some of them. It is impossible to measure the epistemic disunity of the prewar period or the dominance of the positivist position after the war in quantitative terms, since many texts are internally heterogeneous. Nor can methodological positivism be identified by the use of quantitative methods. Patterns of citations in the *ASR* or *AJS* cannot be used to track dominant epistemologies, since only a handful of books or articles were cited more than once during any given year.[20] It makes sense to examine these journals in more substantive terms, however, and to look more closely at five of the leading departments in this period: Columbia, Chicago, Michigan, Wisconsin, and Harvard. These departments produced the largest number of PhD's among ASA members during the 1950–59 period, which is linked to one of the

17. Thus most research on the diffusion and status rewards of "core publications" in sociology deals exclusively with articles; see, for example, Oromaner (1986).

18. First quote from Hałas (2001, 28); second quote from Coser (1971, 508); see also Sorokin (1956).

19. As identified by peer rankings by all heads of sociology departments and by statistics on the suppliers of PhD's to those departments. See also Camic, chap. 7, this volume, who names the same departments as top ranked in this era.

20. A preliminary analysis of the *ASR* for 1950 found that only a few books or articles were cited twice, and only three were cited more than twice. Each book or article was assigned one point regardless of the number of times it was cited in a given ASR article, two points if it was cited in two different *ASR* articles, and so forth. Self-citations were eliminated. This suggests that epistemological homogeneity is revealed not through the citation of common texts but through similar assumptions. Thanks to Prasanna Baragi for help with this citation analysis.

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21. Figures on departmental rankings from Rile
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22. Bourdieu (1984); see Breslau (2005) for an ex

23. Of course it would still be useful to compare
ranked ones, along the lines of Bourdieu's comparati
groups in fields like art and music.

main correlates of departmental prestige, the exchange of PhD's among departments (Burris 2004). The fact that the top-ranked departments in a given discipline are not necessarily located at the top-ranked universities means that we have more diversity even within this small sample than meets the eye. Two departments that were highly ranked in the interwar period but that are excluded from this analysis are UNC–Chapel Hill and Minnesota.[21] The first of these was described by Turner and Turner (1990, 52) as a "bastion of correlational methods," and Minnesota was given over to "a positivistic orientation" (Martindale 1976b, 77).

The argument for focusing on leading departments rather than, say, a random sample is also directed against the notion (Shils 1972) that peripheries typically share the same values with the center. Although all members of a settled field agree on what counts as symbolic capital, the dominated may still hold proudly to a dissonant set of "values" and even develop a taste for necessity, a taste for their own cultural domination.[22] We should not exaggerate the importance of dissidence or difference, which are as likely to reproduce power hierarchies as they are to disrupt the operations of a field.[23]

The other aspects of this small sample that are worth mentioning are the geographic location of the departments and the distinction between private and public universities. Sociological positivism was not concentrated in public as opposed to private universities, or in the Midwest as opposed to the East Coast during the interwar period (nor is it distributed that way today). Columbia played a central role in the positivist disciplinary formation before the 1930s and after 1945, while Chicago and Michigan were the *least* positivist of the leading departments before 1930. Nor should the impact of postwar Fordism on sociologists—an argument I take up in the conclusion—be understood as being mediated by their physical proximity to the sites of Fordist industrial production, since Fordism at that time was as much about consumption and federal-level policies as it was about the point of production. Fordism is named after a social experiment that started in Highland Park and Dearborn at a time when the Michigan sociology department, under the leadership of Charles Cooley, firmly rejected scientism. For that matter, even the Ford Motor Company's own "sociological depart-

21. Figures on departmental rankings from Riley (1960, 918). Minnesota, which ranked near the top of the prestige rankings in 1934 (Burris 2004, 241), is analyzed by Martindale (1976b).

22. Bourdieu (1984); see Breslau (2005) for an example of this from the economics field.

23. Of course it would still be useful to compare the elite departments with less highly ranked ones, along the lines of Bourdieu's comparative studies of prestigious and dominated groups in fields like art and music.

ment" pursued some remarkably unscientific strategies, like the famous "melting pot" ceremony for Americanizing immigrant workers.[24] Postwar Fordism as a societywide phenomenon was unevenly distributed across geographic space. The U.S. South and rural America in general had less direct contact with Fordism. But as a "cultural dominant" it shaped everyday life in Cambridge and New York, Ann Arbor and Chicago, and all of the other sites of the leading departments.

EPISTEMOLOGY AND THE LEADING U.S. SOCIOLOGY DEPARTMENTS, 1930s–1945

COLUMBIA. As one of the first departments, Columbia occupies a central place in narratives of American sociology. From the beginning Columbia was a center of the "natural science" approach. This is usually attributed to Giddings, Columbia's first sociology professor and a leading figure there for almost forty years. Giddings insisted on empiricism, scientism, and statistical methods (for others, if not always for himself). He called for "men" who were "not afraid to work; who will get busy with the adding machine and the logarithms, and give us *exact studies,* such as we get from the psychological laboratories, not to speak of the biological and physical laboratories. Sociology can be made an exact, quantitative science, if we can get industrious men interested in it" (Bernard 1909, 196, quoting from Gidding's response to a questionnaire). According to Seymour Martin Lipset (1955, 286), another Columbia PhD, "philosophically [Giddings] always remained a positivist" (see also Manicas 1991, 65; and Hinkle 1994, 34–46).

At the same time there was, in contrast to Chicago, "a strong theoretical element in the sociological milieu at Columbia" during the 1920s and 1930s, one that was also promoted by Giddings. Doctoral dissertations were written on "the works of the great European sociologists" by students like Theodore Abel (1929), who was praised by Giddings and appointed at Columbia in 1929 as lecturer and two years later as assistant professor.[25]

24. In 1916, "Ford rented the largest public meeting hall in the city. On the stage stood a replica immigrant ship and in front of it a giant kettle, a 'melting pot' . . . the ceremony literally stripped the worker of his past identity and gave him a new one: 'Down the gangplank came the members of the class dressed in their national garbs . . . [then they descended] into the Ford melting pot and disappeared.' Teachers used long paddles to 'stir' the pot. Before long, 'the pot began to boil over and out came the men dressed in their best American clothes and waving American flags'" (Zieger and Gall 2002).

25. Quote from Hałas (2001, 31); see also Lipset (1955, 294). Platt reports that Abel "went so far as to take the opportunity to criticize his colleagues" for their "undue empiricism" in comments to a member of a congressional committee (1996, 202n).

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Robert MacIver, a theorist, ethnographer, and explicit antipositivist, was recruited in 1929. In a paper given to the American Sociological Society meetings in 1931 MacIver attacked the "would-be imitators of the natural sciences," especially the "extreme behaviourists," who "would even jettison their proper subject in order to claim the name of science for a beggarly residue" by imitating "at all costs the mathematicians and the physicists." For MacIver, "imitation, though always bearing the signs of the inferiority complex, may nevertheless succeed when, in following its original, it is applying like tools to like materials. But it is most apt to fail when it applies like tools to unlike materials, and this is just what the social scientist is in danger of doing" (1931, 27–28). MacIver opposed empiricism, arguing that "science is never . . . merely empirical" but is "concerned with phenomena as they reveal an order, a system of relationships." Social relations, unlike natural ones, were inherently both subjective or meaningful and objective. Hence the impossibility of a purely statistical sociology: "we do not comprehend legal codes by measuring them" (MacIver 1931, 33, 28). Three years later MacIver suggested "that social relationships are in such flux that no laws can be formulated" at all. By 1937, as the global social crisis deepened, he held "that the task of sociology is essentially that of interpretation" (Abel 2001, 158, 252).

Other Columbia faculty working in theoretical and qualitative ways in the 1930s included Bernhard Stern and Robert Lynd (C. Wright Mills did not arrive until 1945). Stern was a Marxist in the 1930s who wrote for the journal *New Masses* and founded *Science and Society,* in addition to writing numerous books in the sociology of medicine and other fields. Before moving to Columbia, Stern had been fired in 1930 from the University of Washington for being "too liberal" and was subsequently "harassed by the Catholic Church and academic administrators during his years at the University of Michigan" (Peace 1998, 85). His interest in sociology had been awakened by Charles Cooley at Michigan in 1920. As a student at Columbia, where he was influenced by Franz Boas, who by that time was quite skeptical about "the possibility of establishing significant 'laws' in the cultural realm" (Stocking 2001, 40). Stern rejected the approach of Giddings and the social Darwinists and aligned himself instead with founding sociologists like E. A. Ross and Albion Small.[26] Summarizing his own views in 1949, seven years before his death, Stern wrote that "if the social sciences are denuded

26. See Stern (1959a), vii–x; Merton (1957a). Stern had written his dissertation with Ogburn at Columbia, who strongly supported him; see Bloom (1990), who also discusses Stern's appearance before HUAC and the support offered to him by his colleagues at Columbia.

of value judgments they are really naked of value" and condemned to waver "between the discourse of shallow empiricism, which seeks refuge in the assemblage of particulars, and abstract philosophizing" (1959b, 33). His comment on "history and sociology" is startlingly contemporary:

> Sociologists once talked of imbuing historians with correct perspectives. But now the situation is frequently reversed and it is the historian who can serve as an example to sociologists. . . . The frailty of sociologists lies in their tendency to abstract from historical reality 'ideal types' that are applicable everywhere and nowhere, beyond time and space, and hence in a netherworld of unreality. . . . Sociologists do not stress the great importance of the dimension of time. . . . It renders much of attitude testing fatuous. . . . Sociology will remain one-dimensional and hence shallow, and its concepts empty shells . . . unless the examination of historical concepts becomes a meaningful and disciplined task of sociologists. (1959b, 34)

A much more influential figure than Stern at Columbia and in national sociology was Robert Lynd, author with Helen Lynd of *Middletown: A Study in Contemporary American Culture* (1929). Although the Lynds had not felt it necessary to defend their noncomparative, nonquantitative approach in *Middletown,* their follow-up study in 1937, *Middletown Revisited,* insisted that the "big story lay beyond 'economics statistics' in the 'drama of competing values'" (Camic, chap. 7, this volume). Despite his empiricist stance (Abel 2001, 267–68), Robert Lynd's 1939 *Knowledge for What?* rejected the "natural science" paradigm, ontological atomism, and doctrines of value neutrality. Here he described history as "the most venerable of the social sciences" and speculated (like Stern) that sociologists would begin to do their own historical writing. Lynd understood sociology as inherently concerned with cultural meaning; indeed, sociology itself was for him just another "culture-crystallization." Most strikingly, in light of the clamoring of the "natural science" crowd during the 1920s, Lynd suggested that the social sciences should emulate the humanities and seek a closer rapprochement with "novelists, artists, and poets," who provide "insights that go beyond the cautious generalizations of social science" (Lynd 1939, 116, 129, 138, 153–54, 178).

Along with MacIver, Lynd helped Max Horkheimer make the connections that allowed the Frankfurt Institut für Sozialforschung to move to Columbia (Jay 1973, 39; Wiggershaus 1994). Although the Institute jealously guarded its autonomy, it was not isolated from the Columbia sociology department. The Institute's *Zeitschrift für Sozialforschung* (renamed *Studies in Philosophy and Social Science* in 1940) carried articles by members of the Co-

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lumbia faculty, and Institute members lectured and taught at Columbia after 1936 (Jay 1973, 40, 114–16, 188, 192). One émigré who received support from the Institute in the 1930s was Paul Lazarsfeld, who was recruited by the Columbia sociology department in 1941. Lazarsfeld worked on some Institute projects, including the study of German workers' mentalities that was headed by Erich Fromm, and he collaborated briefly with Adorno in the context of his Princeton Office of Radio Research (forerunner of the Bureau of Applied Social Research at Columbia). Lazarsfeld's Rockefeller Foundation grant allowed Adorno to write the first of his famous essays on jazz music and the "regression of listening" (Adorno 1938; Béthune 2003).[27] This relationship is even more remarkable when we consider that the *Zeitschrift* was publishing critiques of logical positivism by Marcuse. An enthusiastic review of *Knowledge for What?* by Franz Neumann (1939) summarized Lynd's book as "fundamentally a renunciation of positivism and empiricism" and "even more important because it appears at a time when logical empiricism, which in the United States is closely linked to sociology, makes ever growing claims." Lazarsfeld published an article (1941) in the volume of the *Zeitschrift* that carried a review of Marcuse's *Reason and Revolution.* Such close proximity between critical and affirmative sociology (to use Horkheimer's [1937] terms) would no longer be possible even several years later.

CHICAGO. Chicago was the first American sociology department (founded in 1892) and was considered to be the leading department throughout the first three decades of the twentieth century. The founder of the department and the *AJS,* Albion Small, earned his PhD at Johns Hopkins in 1889 with a thesis on the birth of American nationalism and the Continental Congress (Small 1890). Small had been exposed to the German historicist school of political economy during his studies in Berlin and Leipzig (1879–81), and he wrote a detailed studied of the central European cameralist tradition (Small 1909a). Although Small eventually moved toward a version of scientific naturalism, his early predilections for abstract theory and concrete historical studies set the tone in the department and the *AJS* during its early years.[28] The central figure in the department's rise to prominence was

27. After Adorno's falling out with Lazarsfeld, his music project was cut from the renewed grant in the fall of 1939 (Jay 1973, 223).

28. See Vidich (1985, chap. 8); Dibble (1975); and Fuhrman (1978, 98). American sociologists long recognized the relevance of the nineteenth-century German methods debate to their own conflicts (Mills 1943, 168; Hinkle 1994, 48–57). On Small and Giddings see also O'Connor (1942).

Robert E. Park, who taught at Chicago from 1914 until 1933. Park had studied in Germany with Georg Simmel—his only formal sociology training—and he wrote his PhD thesis in Heidelberg under Wilhelm Windelband (Coser 1977, 368), according to whom the social sciences belonged to the *Geisteswissenschaften,* which investigate unique and subjectively meaningful phenomena. Park's antiscientistic tendencies were even stronger than Small's, at least initially. Park was openly disdainful of statistical social science (Bulmer 1984a, 153), although he was not averse to importing natural science models, for instance, in coining the term *human ecology* and in instructing his students to avoid subjective value-judgments (Camic, chap. 7, this volume). The "Chicago style" case study, associated with Park and Ernest Burgess, eschewed grand theory and conceptual categories and remained strictly empirical. At the same time its focus on detailed studies of unique places avoided the positivist covering-law format.[29] Some of the early Chicago case studies were presented in narrative form, lending themselves to a more historical understanding of the task of sociology. For a variety of reasons (some of them detailed by Abbott 1999), the Chicago sociology department did not move solidly into the scientistic camp until the second half of the 1950s (Fine 1995). Other faculty who did not fit the positivist mold at Chicago included W. I. Thomas (at Chicago 1895–1918), Florian Znaniecki (1914–20), Louis Wirth (at Chicago through 1951), Everett Hughes (through 1960), and Herbert Blumer (who was at Chicago until 1952 before moving to Berkeley). Statistical approaches were represented in the department after the recruitment of William F. Ogburn in 1927, but Ogburn later recalled that he found at Chicago a "much more hostile attitude to statistics than had existed at Columbia" (Bulmer 1984a, 181). Louis Wirth, who taught at Chicago starting in 1926, supported Ogburn's appointment and was seen as part of the empirical wing of the department. But Wirth expressed dismay in 1947 about sociology's "aura of pseudo-scientific glamour." As editor of the *AJS,* Wirth criticized the growing enthusiasm for "complicated scientific gadgets" and "super-refined techniques for ordering and summarizing the . . . accumulation of mountains of authentic but meaningless facts" (1947, 274).

MICHIGAN. One of the leading proponents of what is nowadays sometimes called "humanistic" sociology was Charles Horton Cooley, a founder of the American Sociological Association and one of its early presidents

29. The positivist antipathy to the case study was already well established in the 1930s and continues to this day (Steinmetz 2004a).

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Cooley papers, Bentley Historical Library, box 2, fold

(1918). Cooley taught the first sociology course at Michigan in 1894 and was a professor there until his death in 1929. Cooley's own master thinkers included William James and Ralph Waldo Emerson, and he had taken courses with John Dewey in the Michigan philosophy department. He was also acquainted with George Herbert Mead, who taught at Michigan while Cooley was in graduate school there (Cooley 1930b, 6; Coser 1977, 343). Cooley's "ideal sociologist" was Goethe, and in his personal journals and "several of [his] books there are more references to Goethe than to any social scientist" (Coser 1971, 319; Cooley 1918, 402).

Cooley's concept of the "looking glass self" (1927, 194) and his recognition of the inherently ideational-meaningful character of social practice and the need to study social life in its total context are often hailed as examples of the alternatives to positivism that were available in early twentieth-century U.S. sociology. One study of early American sociologists concludes that Cooley was the only one who "did not express a belief in the discovery of social laws" (Fuhrman 1978, 100, 96). Hinkle (1994, 61) describes Cooley as being firmly antipositivist. He is perhaps the only American sociologist cited approvingly by Adorno (1991, 121). For U.S. sociologists in the first third of the twentieth century he provided a bridge to the earlier "revolt against positivism" (Hughes 1977, 33) among the generation of the 1890s.

Cooley is best known for his argument that social research had to be grounded in "sympathetic participation" because "the Social Order can be understood only as a complex of ideas." Cooley gave the example of "the Virgin" in 1200 AD, who, though ectoplasmic, "was a most important member of the social order" (Cooley journals, March 16, 1927, vol. 23, 56). As his nephew noted, Cooley "was preaching *verstehende Soziologie* in *Human Nature and the Social Order*" (published 1902) before Weber began publishing on the topic, and many years before Weber became well known in U.S. sociology (Angell n.d., 10a; Platt 1985).[30] For Cooley, the "materials themselves" of social research were "living wholes which can only be apprehended by a trained sympathy in contact with them" (1927, 156). Sociologists who try to "dodge the mental and emotional processes in which society consists," Cooley argued, were engaging in "pseudo-science" (1927, 154). This view distinguishes Cooley from Ward and the other first-generation theorists in Amer-

30. Parsons (1968, 55) believed that the "intellectual traditions which set the stage for Weber were . . . somewhat unfamiliar to Cooley, ignoring the fact that Cooley had ordered Weber's (1905/1958) *Protestant Ethic* for the Michigan library and had taken courses in "foundational problems of ethnics" and "general natural history" in Munich in 1884 (Angell n.d., 9; Cooley papers, Bentley Historical Library, box 2, folder of student notes from 1884).

ican sociology who emphasized "the virtues of working at a remove from" the social object of study (Breslau 1990b, 427). Cooley found statistics deeply misleading in sociology, even though he had received training in statistics as an engineer (Wood 1930, 710; Angell n.d., 10). The "exclusive devotion of one class of students to statistical and descriptive work of narrow scope" was as problematic as the devotion of a second group to "philosophical dissertations on method, general laws, etc" (Cooley journals, vol. 10, 24). Sociologists' belief that "only quantitative methods should be used" was "an idea springing . . . from an obsolescent philosophy," one that "physicists themselves are beginning to discard" (1928b, 248, 249n1). Cooley especially recommended Whitehead's *Science and the Modern World,* since the author was "an eminent physicist" who advocated not a "mechanistic and atomistic perspective" but an organic one that also "answers to the evident facts of society" (1928b, 249n1). Cooley recommended "life-like description covering a period of time," which he called the "life-study method," a method of "grasping life in its organic reality" (1928b, 248). Such studies could certainly be empirical, as long as they attended to mental states and were conducted in a dialogue with social theory. But statistics could never approach the level of "descriptive precision that may be attained by the skilful use of language, supplemented, perhaps, by photography, phonography and other mechanical devices" (1928b, 249). Other good models for sociology included psychoanalysis, anthropology, photography, and literature (1928b, 250–53; Cooley journals, vol. 22, 51).

Prediction was for Cooley a "false ideal inconsiderately borrowed from the provinces of natural science" (Cooley 1918, 398). This belief stemmed from his view of social reality as a web of conscious (and unconscious) meaning and intention. In social life, "nothing is fixed or independent, everything is plastic" (44). A decade later Cooley responded in more detail to the sociological advocates of prediction:

> Generally speaking the less *life* there is in a phenomenon, the less it is involved in that complex and cumulative interaction that in its culminating human form tends to bring *everything* into play at once,—the more possible is exact understanding and prediction. But, you say, some phenomena of life (of heredity, for example), can be shown to be precise and predictable. This is true but only shows that the life-stream contains, as it were, undissolved mechanical elements which do not change their form. (Cooley journals, vol. 22, 103)

At best, he believed, "one who claims to be a sociologist" might "attempt predictions at least as to the proximate future of the main social currents" (vol. 22, 104).

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the Board of Regents, and *Annual Register,* various years.

Cooley was open-minded about the various forms that social knowledge might take. He insisted on a sociology that was both interpretive and causal, and he was therefore skeptical about merely descriptive approaches. Nonetheless, within the dominant split in U.S. sociology in the interwar years, he defended the increasingly embattled case study as against statistical surveys, maintaining that "the phenomena of life are often better distinguished by pattern than by quantity." "What," he asked, "could be more precise, as a record of visible human behavior, than a motion picture? Yet it is not quantitative. Its precision is total, not incremental, a matter of patterns rather than of minute differences in space" (Cooley 1930a, 314). The sociologist's interpretative work could not take a standardized form but needed to be "imaginative" and rooted in a "dramatic vision" (Cooley 1918, 395–97). Indeed, the "'scientific' and the 'literary'" were not "antithetical terms." Sociology was at once a science, a philosophy, and "an art also" (Cooley 1927, 160). As a result, "the method appropriate to sociology must be learned in part from the great men of letters, who alone have dealt strongly with the facts of human life" (Cooley journals, vol. 13, 48). In a prescient warning against what Adorno called the "higher forms of reification" occasioned by mass-produced "teamwork" in social science (Adorno 1972, 498), Cooley noted that all sociological work was "in a certain sense, autobiographic" (Cooley 1930a, 317; 1918, 402, 404). His own writing was personal, conversational, and essayistic.

Cooley was thus the only founder of a leading U.S. sociology department who firmly opposed scientism. But Anthony Oberschall is grievously mistaken in his claim that Cooley prevented *empirical* sociologists from being hired at Michigan—or else he is confusing the adjectives "empirical" and "empiricist" (1972, 223–24). Cooley's hires were sympathetic to his holistic and interpretive approach, but all of them engaged in empirical research.[31] Most consequentially for the future of the department, Cooley hired his nephew, Robert Cooley Angell, as assistant professor in 1924. Angell's first book, *The Campus,* was based on Cooley's method of "sympathetic insight" and defined its object as "a mental unity" (Angell 1928, viii, 1).

The year after Cooley's death in 1929, Michigan hired Roderick McKen-

31. For example, Cooley hired Arthur Evans Wood, a progressive criminologist and penologist, in 1917 as an instructor. Wood stayed at Michigan until his retirement in 1951. Cooley also hired rural sociologist Roy Hinman Holmes as assistant professor in 1922 and Lowell Julliard Carr in 1925. Carr was a former *Detroit Free Press* writer who had studied in London with Hobhouse, Malinowski, and others and who worked in the fields of industrial sociology and delinquency (Cooley 1930b). Information from the University of Michigan, *Proceedings of the Board of Regents,* and *Annual Register,* various years.

zie, who chaired the department until his death a decade later. McKenzie was the creator, along with Robert Park and Ernest Burgess, of "human ecology," and he is credited with writing the first monograph rooted in that perspective (Gaziano 1996; M. Gross 2002, 31). McKenzie aligned Michigan firmly with the Chicago side of the opposition between the case study and statistical surveys—that is, with a position that was perhaps empiricist but not overly scientistic.[32] McKenzie brought in Chicago sociologists Robert Park, Louis Wirth, Herbert Blumer, and Ellsworth Faris as visiting professors. In 1938 two young instructors were added to the teaching staff as instructors, Werner Landecker and Amos Hawley. Landecker taught European social theory in the department for many years and contributed to theories of social class crystallization. He had written a dissertation in Berlin in 1936 on legal sociology that attacked legal positivism (Lüschen 2002).[33]

McKenzie's successor after his death in 1940 was Robert Cooley Angell, who had been promoted to full professor in 1935 and remained chair of the department until 1952. Angell's long tenure thus began during the *Sattelzeit* (saddle period) between the interwar pluralism in U.S. sociology and the postwar hegemony of methodological positivism. As at Chicago, the consolidation of positivist control did not occur overnight or immediately after 1945 but emerged during the 1950s. Angell's initial hiring efforts were marked by his leaning toward Cooley's tradition. In 1940 he wrote to Ernest Burgess at Chicago,

> we are looking for a social psychologist to add to our staff. . . . I am very anxious that we obtain a man who would be sympathetic to the Cooley tradition and at the same time one who would carry forward fruitful research. It seems to be a difficult combination since most able researchers are being developed in statistics and nothing else. I should want our men to be competent in statistics but would also wish him to have conceptual originality. (Angell to Ernest Burgess, November 5, 1940, 1–2, Angell papers)

Angell's first hire, in 1941, was social psychologist Theodor Newcomb, who was about to publish the results of his landmark four-year study of attitudinal change among students at Bennington College. At the time this

32. As Gaziano (1996) notes, little of the work by McKenzie, or by Park and Burgess for that matter, actually refers to evolutionary ideas.

33. Landecker and his family had belonged to the German-Jewish *Kulturbund* in Berlin. Along with five other refugees he was brought to the University of Michigan in 1937 with funds raised by the University of Michigan Hillel Foundation. He became assistant professor in 1942–43, obtained a PhD in sociology in 1947, and retired in 1981. His dissertation was finally published in 1999.

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represented an innovative hire, since, as his later colleague Daniel Katz writes, "it was unusual for sociology departments to offer tenure appointments to psychologists, but . . . Angell saw in [Newcomb] a true social psychologist in the tradition of Charles Horton Cooley" (Katz 1986, 295). At the same time, as David Riesman remarked on a "follow-up study in the 1960s of the Bennington alumnae whom Newcomb had studied in the early years," what seems missing in such attitudinal research was "any ethnographic material" that might provide the reader with some idea "what the scholars are really scholarly about" or "how creative and idiosyncratic the Creative Individualists really are" (Riesman 1968, 628). Although Parsons imagined psychoanalysis as part of his postwar interdisciplinary social relations mixture, the version of social psychology represented by Newcomb (quantitative and based on surveys or experiments) pointed in a different direction.

Newcomb was called away almost immediately after arriving in Michigan to work for four years "for the government in the Bureau of Overseas Intelligence in order to decipher foreign broadcasts and gain an understanding of enemy morale" (Johnson and Nichols 1998, 57). Angell later noted that by late 1940 "the likelihood that the country would soon be at war was obvious" and "it was not a time to attempt innovations in academic departments." Angell himself left for service in the army air force in 1942 and remained "absent on leave" through 1945 (Angell 1980, 76).

Angell's own research in the interwar period remained loyal to the basic principles of Cooley's approach. His third book, *The Integration of American Society* (1941), was discursive and theoretical and contained no tables or figures at all. Angell continued in the interwar period to defend Cooley's view that only a "pseudo-science" could deny that "the essential facts of social life are mental" and believed "that the sociologist must deal in large measure with interactive behavior." This entailed "the use of the case method in one of its numerous forms" (1930, 340–41; 1931, 204). According to Angell, "the quarrel which many of us have with the usual use of statistical analysis in sociology is that it deals with small segments abstracted from tremendously complex wholes and does not preserve what seems most important—the pattern or configuration of the parts of the whole" (1933, 85). Like Cooley he held that statistics were often "out of place" and a "source of . . . laborious futility" (1930, 342). Against "the school" of the positivist-oriented sociologist F. Stuart Chapin and Ogburn, which "advocates the quantification of our data," Angell defended the position of "the late Professor Cooley and Professor MacIver," who feel "that measurement is only applicable to external things and that such externals constitute only the shell of social relations, not their essence" (Angell 1932, 208). Statistical research that failed to enquire into what we would nowadays call the causal mechanisms producing the relationship would remain inadequate. Angell recommended instead a research design that would select "fairly homogenous" entities for study and focus on the effects of one "condition" or mechanism (1931, 205).

The study that grew out of this methodological orientation was *The Family Encounters the Depression* (Angell 1936). The book was closer in style to Shaw's *The Jack-Roller* (1930) or to Thomas and Znaniecki's *The Polish Peasant* (1918–20) in that each case was presented in the form of a short (six- to twelve-page) narrative rather than being disaggregated into "variables." Angell made clear that he was interested in causal relations, however, namely, in the impact of a "severe decrease in income from accustomed sources" resulting from the economic depression on family life (1930, 258). He was groping toward means of doing "statistical analysis" in a way that "would preserve the wholeness of the cases instead of mutilating them to the extent most statistical analysis does" (Angell n.d., 25). But the family narratives are the most (indeed the only) interesting aspect of this study from our own contemporary point of view. As with Shaw, Thomas and Znaniecki, McKenzie, and other sociologists teaching or trained at Chicago in the 1920s and 1930s, the key to the continuing readability of works like this is the authors' commitment to a holistic case-study method as against the replacement of the names of people and places by the names of "variables."

Another way of classifying work like Angell's *Family* was as "documentary" research. British filmmaker John Grierson had appropriated the term to describe Robert Flaherty's ethnographic film *Moana* in 1926, and it later was turned into a noun. In the social sciences, *documentary* was used as an adjective to describe qualitative source materials, typically usually sources produced by others (Glaser and Strauss 1967, 161). Because of his well-known belief that sociology had to be based on "sympathetic insight" gained through and perhaps recorded and presented in the form of qualitative documents, Angell was invited in 1940 by Ernest Burgess to contribute the chapter on sociology to a planned SSRC volume titled *The Use of Personal Documents in History,* whose publication was delayed by the war until 1945. In his chapter Angell singled out authors such as Franklin Frazier, Clifford Shaw, Edwin H. Sutherland, Frederic M. Thrasher, and Harvey Zorbaugh as leading examples of the documentary approach in sociology. For Angell, personal documents were one way for sociologists to grasp "the objectives toward which men are striving and how . . . situations are interpreted" (Angell 1945, 178). Angell saw no reason why the term *nomo-*

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thetic could not be used also to "cover laws that have been worked out for, and are applicable to, individual cases only" (229). Needless to say, this definition of the words *nomothetic* and *law* departed sharply from positivist understandings. Angell again insisted that sociologists had "no option" other than to engage in "sympathetic understanding," that is, in the "painstaking plodding along the trail which Cooley and Mead long since blazed," even though "many have gone to the extreme" of believing "that facts could speak for themselves" (230–31).

If we follow Cooley's lead and attempt to summarize the interwar field of U.S. sociology in terms of participants' own understandings, it seems that the basic conflict pitted advocates of statistical surveys and experiments against champions of more holistic case studies. The latter were pursued in a more psychological and cultural vein (like Angell) or in a more materialist vein, as in McKenzie (1923). Angell's work after 1930 embodied a middle-ground interpretivist position associated with his illustrious relative and the increasingly prestigious "scientific" approach. Between 1930 and 1945 his research seems to express the balance of epistemological forces in the discipline at large. As we shall see, he tried to continue to reach a kind of compromise in departmental appointments after the war, but forces larger than his own limited power as departmental chair pulled the department in the positivist direction.

WISCONSIN. Edward A. Ross, longtime chairman at Wisconsin and another founder of American sociology, opposed making sociology scientific at the cost of political relevance (M. Gross 2002). Ross was internationally oriented, writing on the Russian Revolution firsthand (Ross 1918, 1921b, 1923) and visiting and reporting on numerous other countries, including China, India, Mexico, Portuguese Africa, and South Africa. This global perspective stood in marked contrast to the U.S. centrism of most interwar and postwar American sociology (Connell 1997; Oberschall 1972, 224–25). Ross criticized the "natural science" crowd for describing society as a "theater of mechanical forces." Unlike positivists nowadays, Ross did not see causal and interpretive knowledge as alternatives but insisted instead on "a causative interpretation of social facts" that "must consider the thoughts and feelings of the units whose behavior is to be explained" (1903, 114). Something of an interpretivist, Ross criticized sociology's fondness for "the objective statement of the behavior of associated men in preference to the subjective interpretation" (1903, 106). Of course his well-known *Social Control,* first published in 1896, was framed broadly in the terms of the social evolutionary views that were widespread in the nineteenth century. Just a few years later, however, he rejected the notion that "culture epochs answer to the gradations in the intellectual life of mankind" and insisted that "it is vain . . . to correlate closely the actual course of evolution of a society with intellectual development, seeing that so many other factors influence it" (1903, 111). Here his words sound quite contemporary—or like a throwback to Herder: "Far from traveling a common highway the peoples have followed routes as various as have been their conditions of life. . . . Vain, likewise, is it to frame a universal law for the succession of political forms" or social ones (1903, 115–16; cf. Gaonkar 2001 and Noyes 2006). Half a century later he argued that the sociologist cannot conduct true experiments but is "really a field observer with a notebook" and insisted that transhistorical generalizations were impossible in sociology since "the behavior of man varies so much from age to age" (1945, 491–92).

Howard P. Becker, who was hired to replace Ross, was a social theorist who had translated Leopold von Wiese's *Systematic Sociology* (1932). Becker attacked scientism in his writings and in clashes at Madison with Ogburn's student, statistician T. C. McCormick (Martindale 1982, 31, 35, 38). In 1934 Becker coauthored *The Fields and Methods of Sociology* with Luther Lee Bernard, a student of Albion Small and president of the American Sociological Society in 1932. Although Bernard had initially defended behaviorism, he was one of several sociologists in the 1930s and 1940s who decried the putative links between positivism and fascism (Bannister 1992).[34] However exaggerated these arguments, the discussion is suggestive of the splintered and nonhegemonized character of U.S. sociology from the 1930s to 1945. Hans Gerth came to Madison in 1940 and, according to C. Wright Mills, was "the only man worth listening to in this department" (quoted in Martindale 1982, 2; see also 27). Gerth had studied with Horkheimer, Adorno, and Fromm in Frankfurt during the early 1930s (Greffrath 1982, 18). He harshly criticized the ahistoricism and antitheoretical bent of U.S. sociology (Gerth 1959). Mills himself was a strong local presence during his two years as a student at Wisconsin (1939–40) and was already publishing prolifically.

HARVARD. Harvard is a special case, and discussed mainly in the next section, because the sociology department was founded only in 1931 when Pitirim Sorokin was hired and then was dissolved into the new interdisciplinary Department of Social Relations in 1946. During the 1930s Sorokin

34. This anticipated the arguments of other sociologists like Frank Hartung (1944, 337), Marcuse (1941) and Horkheimer and Adorno (1944), which linked positivism to Nazism (Bannister 1992, 185). Bernard provocatively characterized sociologists who "aped the physical scientists" as "Fascists at heart" and occasionally in point of fact (Bernard 1940, 344, 342, 340).

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was seen as "a leading figure in American sociological positivism." He had received his training at the Psycho-Neurological Institute in St. Petersburg (Nichols 1992, 215; Sorokin 1963, 67–73). Sorokin's defense of a scientific approach to sociology, in contrast to the reformist and religious precedents that had dominated sociological teaching at Harvard before his appointment, gave him the reputation of a positivist, but his writing was more complicated. One of Sorokin's major works, *Social and Cultural Dynamics* (1937–41), rejected empiricist claims to exclusivity, arguing that "Sensate culture" was just one of three different forms of truth. But as Hans Speier pointed out in a review of the book, Sorokin's study was itself "imbued with the spirit of the doctrine that he desires to refute" and was "expressive" or "derivative" of its own civilization rather than being a critique of it. Sorokin "discusses the philosophical problems which he raises . . . with the help of quantitative methods" without ever asking about the adequacy of these methods for this sort of question, that is, "without ever disentangling the ethical problem of what is good from the essentially meaningless one" of trying to *quantify* the good (Speier 1948, 891). Although Sorokin now proposed a thoroughly culturalist interpretation of society that flew in the face of his own earlier behaviorism, he saw societies as progressing through cultural stages in a predictable logic of development, in a sort of idealist mirror image of the orthodox Marxism that he rejected. Sorokin thus exemplifies the widespread phenomenon in this period of epistemically hybrid sociology, while the work of Adorno, on the one hand, and Lundberg, on the other, represent the purified epistemic extremes.

OTHER INDICATORS OF THE EPISTEMOLOGICAL STANDOFF IN THE DISCIPLINE BEFORE 1945

The *ASR* reveals an epistemological and methodological diversity in the decade before 1945 that is striking in comparison with subsequent decades. Theoretical articles only declined gradually in the journal's pages (Wilner 1985, 16, table 10). The first two volumes of the *ASR* ran essays on psychoanalysis by Karen Horney (1936), "Language, Logic, and Culture" by C. Wright Mills (written while he was still an undergraduate at Texas), and on topics like "imagination in social science" (Bowman 1936) and Lenin's theory of revolution (F. Becker 1937). The journal carried a critique of Comtean positivism by the founder of the original version of critical realism, philosopher Roy Wood Sellars (1939). Anthropologists were free to publish discussions of the culture concept in the *ASR* during this period, a topic that would be exiled from sociology after the war when Kroeber and

Parsons (1958) divided up the social-ontological field like the European powers splitting up the colonized world at the Berlin West Africa Conference.

Another sign of the philosophically labile condition of U.S. sociology in the 1930s concerns its relationship to Freud. Psychoanalysis is often difficult to reconcile with empiricism and aculturalist behaviorism, even though it is open to biologizing interpretations (Elliott 2005; Jacoby 1983) and was used by some early sociologists in politically conservative ways (Schwendinger and Schwendinger 1974, 345–80). Cooley (1907, 675) had already argued at the first annual meeting of the American Sociological Society in 1906 that the "social mind" had to be seen as encompassing an *unconscious* dimension. Even the would-be positivist Read Bain was driven into a more epistemically ambiguous position in the mid-1930s, writing in the *ASR* that "sociologists have always known that social and societal phenomena" are "indeterminate, relativistic, [and] non-mechanistic." Sounding more like his former Michigan PhD adviser, Charles Cooley, Bain concluded that "F. S. Chapin's statement about latent culture patterns" should be reinterpreted as a form of "societal unconscious" (Bain 1936, 204). The *AJS* published a special issue on psychoanalysis and sociology in 1939, the year of Freud's death, with essays by A. L. Kroeber, Harold Laswell, Karen Horney, and Kenneth Burke (writing on "Freud and the Analysis of Poetry"), along with articles by medical doctors, psychiatrists, and sociologists. This collection suggests a lower level of anxiety about disciplinary boundaries than in later periods and an openness to the depth-realist categories and concepts of psychoanalysis.[35] One of the contributors to the 1939 *AJS* issue even remarked that "sociology is sufficiently mature to adopt the methods" and categories of psychoanalysis, including "the phenomenon which Freud called the return of the repressed," which was "of particular importance to sociology" (Zilboorg 1939, 341). It goes almost without saying that the concept of the "return of the repressed" has not figured centrally in most postwar American sociological writing.

There are other indicators of the epistemically unsettled nature of U.S. sociology in this period. Individual texts were internally heterogeneous.[36] A volume on the "family in the Depression" by Stouffer and Lazarsfeld (1937), who are "often remembered as two of the staunchest proponents of quantification, repeated throughout their volume the need for both types

35. Each text has to be examined closely to determine *which* Freud is being endorsed—the "radical" version or the repressive, biologistic one.

36. Such epistemic slippage and ambivalence also characterize some would-be sociological positivists today; see Steinmetz (2005a) for a case study of one such text.

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of research" (Camic, chap. 7, this volume). It may not tell us much about the field's epistemic power structure to register that the American Sociological Society was divided between "value-neutral" positivists and "humanistic" social activists. It is revealing, however, that the Sociological Research Association was itself divided between the scientistic operationalists like Lundberg, Bain, and Stuart Rice, and more interpretivist "Chicago men" like Herbert Blumer.[37] The SSA was an invitation-only professional group that was formed in 1936 in response to battles within the ASS.

Edward Shils worried in 1948 that the still feeble efforts toward theoretical development in sociology were "in danger of being suffocated in the stampede for concrete results with immediate descriptive or manipulative value," adding that "the post-war financial prosperity of American sociology with the vast sums of money made available by governmental bodies, foundations, and private associations and firms makes this danger a very real one" (Shils 1948, 55). This prophetic phrase (from someone who played both sides of the street, epistemologically speaking) leads us directly to the (re)consolidation of the discipline after 1945.

The Postwar Settlement

By 1950 this balanced or splintered epistemic condition had disappeared, and sociology was becoming a well-structured field. Sociologists had so little in common in substantive terms that their disagreements and settlements necessarily revolved around the politics of method and epistemology. Methodological positivism was becoming orthodox or even *doxic,* that is, its practices and proclamations were increasingly recognized even by its opponents as a form of scientific capital, however much they disliked it.[38] As noted earlier, epistemic unanimity within a field is not a prerequisite for the hegemony of one particular position. A journal like *Qualitative Sociology,* for example, publishes work that is sometimes distinct from the hegemonic model; at the same time, however, its very title seems to acknowledge its own dominated status.[39] But during the 1950s and well into the 1960s

37. Farris (1967); Evans (1986–87, 123); Bannister (1987, 189, 218; 1992).

38. The (Hegelian) category of recognition is at the heart of Bourdieu's analysis of the working of fields (Steinmetz 2005b).

39. *Qualitative Sociology* was founded in the late 1970s, a period that saw a resurgence of positivist hegemony. One reader of this chapter commented that similar claims were made with respect to the Society for the Study of Social Problems (founded at a meeting in Chicago in 1951) and its journal *Social Problems.* The founding statement of the SSSP differed from the scientistic model in ways that were so coded as to be almost unnoticeable. Emphasizing social

(and perhaps beyond), methodological positivism prevailed in the leading sociology journals, in the most widely used textbooks and methods books, in the top departments, and in the tastes of the relevant funding agencies. In addition to the continuing efforts of the prewar camp (Bain, Lundberg, Lazarsfeld, Ogburn, Dodd, and others), there was an influx of entirely new characters. Many of them, like the statistician and survey methodologist Leslie Kish at Michigan, rotated into the discipline from wartime jobs with government agencies.[40] James S. Coleman entered sociology from a job as a chemist at Eastman Kodak with a self-described "positivist orientation . . . carried over from the physical sciences," an orientation he was able to polish in courses he took with Ernest Nagel on the philosophy of science (Coleman 1990, 75, 96, 98). Nagel was probably the most widely read positivist philosopher in sociology during the postwar decades. Although Coleman came from the same evangelical Protestant background as Lynd, he rejected the latter's antiscientistic and meliorist orientation in favor of a more "modern" mathematical and utilitarian brand of sociology.

Let us look briefly at the same four departments examined earlier and cast a brief glance at Harvard's postwar Department of Social Relations, which ascended to top ranking in the postwar decades.[41]

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problems was a mild critique of value-free social science and was linked to a sense of the SSSP as providing a defense of sociologists who were under "attack by representatives of vested interests and of reactionary groups" (Burgess 1953, 3). The emphasis on interdisciplinary collaboration with anthropologists and psychologists rather than economists or natural scientists (Burgess 1953) was also a polite rejection of the scientistic approach.

40. Kish was a political radical who fought in the Spanish Civil War in 1937–39. He helped found Michigan's Institute for Social Research in 1947, before joining the sociology department four years later. Most of the logical positivist philosophers were also on the political left, of course. Epistemology and politics were orthogonal.

41. Most specialists (e.g., Oromaner 1973) locate these departments through the late 1960s among the top five, along with Berkeley, which I leave aside here due to the paucity of secondary literature, the fact that it was not in the top rankings before the war, and the contributions of Burawoy and VanAntwerpen (n.d.) and VanAntwerpen (2005). As VanAntwerpen notes, Berkeley's Department of Social Institutions was founded in 1923 and changed its name to the Department of Sociology and Social Institutions in 1946. Glenn and Villemez's (1970) comparison of departments in the 1965–68 period finds the same six departments in the lead, although UNC sometimes ranks ahead of Harvard on two of their productivity indexes. This suggests to me that we need to know more about departments than productivity rates in order to assess their relative status. For example, Oromaner's data (1969, 333) on the top American sociologists according to graduate student reading lists and *ASR* citations during the late 1950s and early 1960s show three of those sociologists at Harvard (Parsons, Homans, and Sorokin), two at Columbia (Merton and Lazarsfeld), one at Chicago (Shils), and one formerly associated with Michigan (Cooley), but none at UNC or Wisconsin.

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EPISTEMOLOGY AND THE LEADING U.S. SOCIOLOGY DEPARTMENTS, 1945–1965

COLUMBIA. Lazarsfeld's Bureau of Applied Social Research constituted a center of methodological expertise and research in the positivist spirit. Whereas relations between Lazarsfeld and the Institute for Social Research had been on a more equal footing before the war, Lazarsfeld now suggested that the Institute be integrated into his Bureau (Jay 1973, 220). Lazarsfeld's work became "more academic" as he moved "toward academic respectability." At the same time Merton "was becoming more quantitatively empirical" (Coleman 1990, 81). He had written a series of influential historical and theoretical essays on the sociology of science and knowledge during the 1930s and 1940s that had little in common with the quasi-positivist "program for concentration on 'theories of the middle range'" for which he became famous in the postwar period (Parsons 1937/1968, 1:ix; Merton 1968a). Lazarsfeld and Rosenberg's influential *Language of Social Research* (1955), its title redolent of the Vienna Circle with which Lazarsfeld had once maintained contact, contained a section on the "philosophy of the social sciences" that was based entirely on the nomothetic-deductive approach, which was itself a child of logical positivism. This section included a chapter by Nagel, who ran a series of seminars together with Lazarsfeld on mathematical sociology (Coleman 1990, 88). *Language of Social Research* also included an excerpt from Hans Zetterberg's *On Theory and Verification in Sociology* (1954), which argued that axiomatic or deductive theory was "the most satisfactory" type. Zetterberg gave examples of the "if A then B" variety and defended additional positivist postulates such as an a priori preference for "parsimony" in explanation (1954, 534).[42] According to Mullins and Mullins (1973, 218), Zetterberg's book was "the accepted philosophical basis for theory in standard American sociology." Hans Zeisel, Lazarsfeld's lifelong friend and his coauthor (with Marie Jahoda) of the famous Marienthal study (1933), published a popular book on the use of mathematical figures and tables in social science (1947/1957). Zeisel insisted that there was "no logical difference between the study of voting or of buying" and argued for the superiority of social surveys over experiments (xviii, 131–33).[43] A former Columbia student who wrote a dissertation on Durkheim, Harry Alpert,

42. Mullins and Mullins (1973) discuss Zetterberg alongside others in what they call the "positivist style."

43. As Dan Breslau (1998) shows, experimentalists lost struggles with econometricians over control of research on U.S. labor market policy, even though both were broadly positivistic. Zeisel began teaching at the law school at Chicago in 1953 (Sills 1992, 536).

went to the Programs Analysis Office of the newly created National Science Foundation, where he determined the conditions under which sociologists and other social scientists could attain NSF funds. Alpert privileged what he called "the hard-science core of the social sciences" (Cozzens 1996, 3), and in a series of articles he laid out the conditions under which sociologists would be eligible for NSF funding.[44] The first of these was "the *criterion of science,* that is, the identification, within the social disciplines, of those areas characterized by the application of *the* methods and logic of science" (Alpert 1955, 656; my emphasis). This criterion necessitated the "convergence of the natural sciences and the social sciences." Attention to the "national interest" constituted a third prerequisite for funding, suggesting that sociologists would be expected to draw predictive and practical lessons from their research and that value neutrality would have to remain blind in one eye. Alpert concluded one of his articles with a warning: "the social sciences . . . are here to stay, but their future growth and development"—that is, their access to government funding—would "depend largely on their capacity to prove themselves by their deeds" (1955, 660). Alpert's NSF division funded research carried out at Lazarsfeld's Bureau (see Menzel 1959, 199n) and at other centers.

C. Wright Mills was not a bulwark against this positivist tide. James C. Coleman (1990, 77) recollected that when he was a student, Mills "seemed to matter little" in the Columbia "social system of sociology"—or that he "mattered only to those who themselves seemed to matter little." This statement provides a poignant sense of the local and the disciplinary marginalization of one of the most important U.S. sociologists of the twentieth century during this era of cold war and hard science.

CHICAGO, WISCONSIN, AND MICHIGAN. Sociology at the other three universities discussed earlier—the second "Capitoline triad" in postwar U.S. sociology, alongside Parsons, Merton, and Lazarsfeld[45]—was also increasingly dominated by methodological positivism, although the timing

44. As Keat and Urry (1975, 91) point out, Alpert's PhD thesis (1939) was influential in making Durkheim palatable to positivist sociology in the United States by arguing that Durkheim "did not adhere to such a strong interpretation of the social as had often been claimed" and because he inductively built up "general laws of social life through the accumulation of statistical findings." After serving in various other foundation functions, Alpert moved to the University of Oregon in 1957 as dean of the graduate school and served as president of the Pacific Sociological Association in 1963.

45. The Capitoline triad was the union of three Roman deities who shared a temple on Rome's Capitoline Hill. Bourdieu (2001b, 198) discussed the "triade capitoline" of Parsons, Merton, and Lazarsfeld.

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46. Faculty in this area at Wisconsin before 1
Armer, Archibald Haller, and Edgar F. Borgatta.

and modalities of this shift varied. At Chicago, representatives of the new paradigm did not gain firm control of a highly factionalized department until 1957. Their takeover then was so complete, however, that the editor of a recent volume felt compelled to seek a "valorization, a vindication" of the years 1946–52 as the era of a "second Chicago school" centered around Herbert Blumer, Everett Hughes, and Anselm Strauss (Fine 1995, 1–9). As Fine notes, by 1960 the Chicago department had become more "scientific, modern, [and] positivist" (9). About half of the PhD theses written in Chicago's sociology department between 1946 and 1962 "used entirely quantitative methods"; two-thirds of the journal articles published by Chicago sociology faculty in this period were quantitative (Platt 1996, 266). New quantitative faculty hired in this period included demographers Don Bogue (from 1954), Otis Dudley Duncan (from 1951), and Evelyn Kitagawa (from 1951) and mathematician Leo Goodman (from 1950); Ogburn remained at Chicago even after his retirement in 1952 (Fine 1995, 404–5). And although the qualitative party still had a "coherent focus" (Abbott 1999, 56–58) in 1955 during the chairmanship of Everett Hughes, it had completely dissipated by 1957. David Riesman later recalled that "when the demographers . . . took over, the climate of the Department changed. . . . Students began to worry that unless they had tables in their theses, they wouldn't get their Ph.D.'s" (Bainbridge 2002, 4). A report by chairman Philip Hauser from 1958 spoke tellingly of "the complete disappearance of the earlier bipolar division of departmental interests" (quoted in Abbott 1999, 59).

At Wisconsin, William Sewell Sr. was "instrumental in building [the] powerful and notoriously positivist sociology department and in obtaining a place for sociology at the federal feeding trough, especially at the National Institutes of Mental Health and the National Science Foundation" (Sewell Jr. 2005; see also Sewell Sr. 1988). Together with Michigan and Chicago, Wisconsin pioneered the so-called new causal theory in sociology, using path modeling and related techniques.[46] There were a few exceptions at Wisconsin: Warren Hagstrom, a sociologist of science, who arrived from Berkeley in 1962; Joseph Elder, a comparativist South Asianist and student of Parsons, who arrived in 1963; and Robert Alford and Jay Demerath, who came from Berkeley several years later. But they were located within a large and rapidly expanding department that was dominated by stratification research, demography, and experimental social psychology. Not until the arrival of Maurice Zeitlin in the second half of the 1960s, the visiting profes-

46. Faculty in this area at Wisconsin before 1965 included Sewell Sr., Vimal Shah, J. M. Armer, Archibald Haller, and Edgar F. Borgatta.

sorships of Manuel Castells in the mid-1970s, and the recruitment of Erik Olin Wright at the end of the 1970s did the department's monolithic character begin to diversify.

What about Michigan? After the war Robert Angell returned home to chair the sociology department until 1952. He also returned home to an academic field that was expanding rapidly and undergoing turbulent growth. Several developments were especially important as immediate or proximate causes of change at the local level. One was the cycling into sociology departments of researchers from government and wartime agencies (Turner and Turner 1990). Related to this was the expansion of federal funding for social science research from "military, intelligence, and propaganda agencies" such as the U.S. Army and Air Force. These agencies remained the most important sources of funding for social research "until well into the 1960s" and often initiated "social science concepts and projects" (Simpson 1999, xiv). The massive involvement of military funders entailed "a marked preference for quantitative analysis as opposed to historical, qualitative, or other forms of social research that seemed 'soft' by comparison" (Solovey 2001, 177). Another important development that was more internal to the academic and sociological field was the decision at Harvard to allow Talcott Parsons to create an interdisciplinary social relations department. While the shifts related to the war and military policy tipped the hand of those who wanted to channel departments like those at Michigan and Chicago toward the "natural science" approach, Parsons's social relations model resisted the temptations of scientism to some extent by bringing in cultural anthropologists and psychologists who were sometimes willing to engage with psychoanalytic concepts and theories. The postwar Harvard model replaced the interwar Chicago case-study approach as the leading alternative to full-bore scientism in sociology.

Angell's activities after 1945 were closely attuned to these countervailing tendencies. On the one hand, he and Theodor Newcomb founded the Survey Research Center in 1946. This was the precursor of the Institute for Social Research (ISR), which was created in 1949, the same year in which Horkheimer returned to Frankfurt with his identically named Institut für Sozialforschung (Jay 1973, 282; Frantilla 1998, figs. 1–4). Of course, the difference between the two institutes could not have been more profound, and Adorno almost seemed to have the Michigan ISR in mind when he wrote his critique of "teamwork in social research" (1972). Michigan sociology cannot be equated with the ISR, given the institutional separation of the two entities and the fact that many members of the ISR have been nonteaching research scientists or members of psychology and other de-

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partments. At the same time, one cannot separate postwar Michigan sociology from the ISR milieu, as Hollinger (1996) has demonstrated and as anyone who has spent time in that department during the past five decades can corroborate. Angell served on the executive committee of the ISR for many years.

On the other hand, Angell described himself as being strongly "attracted by the broad coverage of behavioral science that had been worked out in the Social Relations Department at Harvard," which entailed an integration of cultural anthropology, psychology, and area studies. His first move as department chair in 1946, therefore, was to recruit anthropologist Horace Miner on "the strong recommendation of Robert Redfield to give us a capability in social anthropology" (Angell n.d., 30). Miner had already published an ethnography of St. Denis, a French Canadian parish. He was promoted to associate professor in sociology and anthropology in 1947, but his salary continued to come from the sociology department. Miner served on sociology's executive committee continuously from 1951 to 1979 (Griffin 1995, 291). He wrote books on Timbuktu, Algeria, and Fez, Morocco, carried out research in other parts of Africa, and taught courses on African studies in the sociology department from the 1940s onward. In 1952, the last year of his tenure as department chair, Angell hired a fresh anthropology PhD from Columbia, David F. Aberle, who had published a book the previous year on Hopi culture. Aberle conducted research during the coming years on Navaho and Ute peyotism and taught courses in the sociology department on culture contact.

Angell's commitment to anthropology and international area studies thus expressed itself at the local departmental level, and not just in his better-known activities with UNESCO's social sciences department and as president of the International Sociological Association, or in his often-expressed belief that the national state is an anachronism. Angell was also one of the first to recognize the significance of history for sociology. In his notes for a presentation titled "What Does History Offer Sociology" in 1962 with historian Sylvia Thrupp, Angell wrote, "from about 1918 strong emphasis on improved research techniques, statistical methods, close study of contemporary scene. History and social evolution largely ignored, at least in this country. . . . Sorokin a lonely figure in American Sociology." He then alluded to a "third period" that

> begins with great interest in underdeveloped world with throwing off of colonialism in the 1950s. Sociologists . . . don't deal with the underdeveloped world much. Well trained sociologists found out how to use historical sources—

> Marion Levy, Robert Bellah, Ed Swanson. Two at least have focused on particular historical processes—Eisenstadt on the growth of empires, and Barrington Moore on alternative processes of change from agrarian to industrial societies. Excellent studies.[47]

Angell's interdisciplinary, international, anthropological, and historical orientation was almost completely marginalized in the department, however, until the hiring in 1969 of Charles Tilly, a student of the "lonely" Sorokin, as a professor of sociology and history. The only part of the interdisciplinary social relations model that survived after 1952 was the social psychology axis, which was powerfully aligned with positivism. The psychological faculty and graduate students in this program tended to use experiments, while the more sociological members used surveys and organizational analysis. All were imbued with the same strong "scientific" spirit, according to one graduate of the program.[48]

Angell's other appointments between 1945 and 1952 struck a sort of balance between the less positivist approach and the new scientism that was rapidly gaining momentum in the discipline and locally at Michigan. Some of these appointments were more strongly associated with the first side of this division, such as Guy E. Swanson and Gerhard Lenski (both hired in 1949), and Morris Janowitz (hired in 1951). Others were associated with the latter tendency, especially Ronald Freedman (hired 1946) and Rensis Likert, who taught sporadically in the sociology department in this period.[49] But all of the new hires except Miner apparently felt the pressure to become more statistical and "scientific," applying "unsympathetic" methods to phenomena that Cooley would have insisted required "sympathetic understanding." Gerhard Lenski went the farthest in the direction of scientism. During his time at Michigan (1950–63) he was a sociologist of religion, which had traditionally been one of the least scientistic of sociological subfields, concerned as it was with meaning. But Lenski based his 1961 opus *The Religious Factor* on survey research from the 1957–58 Detroit Area Study.

47. Cooley papers, Bentley Historical Library, box 2, Outlines of Talks folder.

48. Personal communications from Mayer Zald (University of Michigan PhD, 1961).

49. Likert had been director of research for the Life Insurance Agency Management Association in Hartford, Connecticut, from 1935 to 1939, when he was appointed director of the Division of Program Surveys in the Bureau of Agricultural Economics of the U.S. Department of Agriculture in Washington, DC. His various activities in World War II are discussed in Johnson and Nichols (1998). Likert was initially hired as director of the Survey Research Center in 1946 and was professor of sociology in 1946–47 without salary (University of Michigan, *Proceedings of the Board of Regents*, October 1946, p. 562), and professor of sociology and psychology from 1956 to 1963.

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The 1956 Michigan textbook *Principl*

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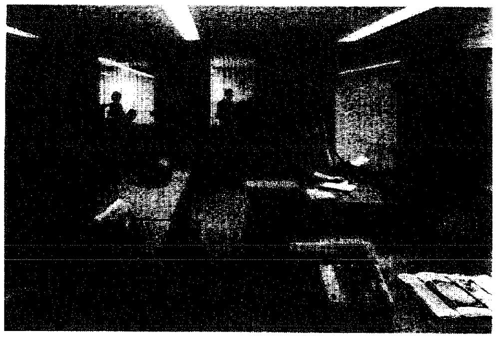
In subsequent work he expanded on the technologically reductionist evolutionary theory of Michigan anthropologist Leslie White (Lenski 1966) and articulated an explicitly "neopositivistic" epistemology and radically empiricist ontology (Lenski 1988).

Angell's own work became two-pronged. On the one hand he moved closer to what he felt was a more legitimate and scientific perspective, writing that "sociology was proud that it was becoming truly scientific, and I wanted to be an up and coming sociologist" (Angell n.d., 21). His first postwar project on "the moral integration of cities" (1951) departed from the style of his earlier work. Here he examined the effects of ethnic heterogeneity and mobility on low crime rates and high levels of welfare effort ("moral integration") in a sample of cities, using methods that have been recognized as an early example of the use of regression analysis in sociology (Angell 1951). But there was no attention to the actual mentalities or discourse of people living in cities except for a survey using a Likert-style scale of how much people liked living in their hometowns. In his work on transnational movements and international conflict resolution Angell remained primarily theoretical and qualitative. Indeed, he seemed to swing back toward his youthful skepticism toward the natural science perspective as the years went on. He praised a book by a philosophical sociologist who made the relativist argument that "the standards of scientific validity are themselves social products so that no scientist can ever really prove that his theory is correct" (Angell 1956, 235). Three years after his retirement Angell praised the "suggestiveness" of Erving Goffman's (1959) work for "exploring image control by governments" (Angell 1972, 115). This was around the same time that Goffman was being trumpeted by Alvin Gouldner (1970) as the standard-bearer for a long overdue postpositivist revolution in U.S. sociology.

Thus with very few exceptions, the Michigan sociology department was dominated in the postwar period by adherents of methodological positivism. The hegemony of this perspective was expressed in the expansion of the Institute for Social Research (figs. 9.1–9.4), which threatened to extinguish Cooley's legacy of respect for autonomous theory, humanism, interpretivism, and the holistic analysis of specific places and historical processes.[50]

The 1956 Michigan textbook *Principles of Sociology,* edited by Ron Freed-

50. The publications from the Michigan department in the 1950s and 1960s are suggestive of this thoroughgoing positivism. An edited volume on Cooley opened with an essay on Cooley as "demographer" (Schnore 1968), indicating the degree to which that particularly positivistic social-science subspecialty had made inroads into sociology (the author was a 1955 Michigan PhD).



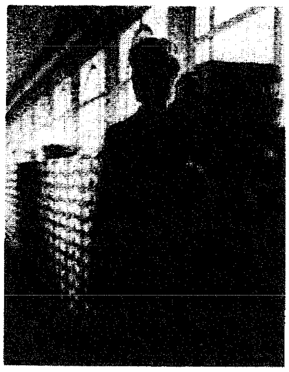


Figure 9.1 TOP LEFT: Postwar technoscience at the (Michigan) Institute for Social Research; the ISR building, completed in 1965. Courtesy of ISR.
Figure 9.2 TOP RIGHT: Inside the Institute for Social Research. Courtesy of ISR.
Figure 9.3 BOTTOM LEFT: Rensis Likert, director of the Survey Research Center, 1948–49 and of the Institute for Social Research, 1949–70, with data files. Courtesy of Bentley Historical Library, University of Michigan, Ann Arbor.
Figure 9.4 BOTTOM RIGHT: Charles Cannell, director of field operations at the Survey Research Center, with interviewers. Courtesy of ISR.

man, Amos Hawley, Karl Landecker, Gerhard Lenski, and Horace Miner, replaced the earlier, less positivistic Michigan textbook by Cooley, Angel, and Carr (1933). Although the individual chapters were diverse, the book's introduction defined sociology as a science of "human groups . . . subject to study by the same methods as other natural phenomena"; the discipline's aim was to "discover systematic . . . observable relationships between . . . phenomena" (Freedman et al. 1956, 5). Sociology was both inductive and deductive, and it was "nonethical" since "the scientist has not techniques by which he can determine what the ultimate ends of a society should be," although his knowledge may well be "instrumental" (6, 12). Another key Michigan methods text that was widely read in the joint PhD program in

social psychology was Festinger and Kat
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51. Abraham Kaplan (1918–1993) taught at Mic
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magazine in 1966.

52. According to the OCLC catalogue, 1,417 co
U.S. libraries, as opposed to 1,096 copies of the Fes

social psychology was Festinger and Katz's *Research Methods in the Behavioral Sciences* (1953), which enshrined the experimental social psychology laboratory framework with its notorious tendency to let college sophomores stand in for "man" in general. Festinger and Katz did not employ a formal philosophical framework (Hollinger 1996). Another relevant Michigan faculty member was philosopher Abraham Kaplan, a student of Bertrand Russell and "by training a positivist," who wrote *The Conduct of Inquiry: Methodology for Behavioral Science* (1964). Unlike the works of Nagel and Lazarsfeld, this text explicitly combined logical positivism with methods for the behavioral sciences.[51] Although it is difficult to know how widely any of these books were read, Robert Friedrichs (1970, 36) mentioned Kaplan's volume as part of a small explosion of new philosophical writing that sociologists were reading during the first half of the 1960s.[52]

The postwar epistemo-methodological realignment in sociology fundamentally transformed the Michigan department. After Angell's term as chair, the department added Hubert Blalock (1954–64), a statistician-cum-sociologist whose positivism inspired refutations by professional philosophers (e.g., Miller 1987, 240–41); Lillian Cohen (1950–57), who wrote an introduction to statistical methods for social scientists (Cohen 1954); demographer Harry P. Sharp (1955–61), a 1955 Michigan PhD; and demographer-statistician Otis Dudley Duncan (1962). Among the rare exceptions in this period were Allan Silver, who had written a Michigan dissertation with Janowitz and Swanson in 1963 and taught in the department during the next decade (1954–64), and East Asian specialist Robert Mortimer Marsh, hired in 1958. The ideological hegemony of methodological positivism was so powerful that when Angell died he was eulogized by a member of the department as a statistician and survey researcher who created a department devoted to quantitative research (Ness 1985, 10). As late as the 1980s the required methods course for all graduate students in sociology at Michigan was the Detroit Area Study, in which they studied survey methods. Only in very recent years has this methodological hegemony started to crumble.

HARVARD. Harvard presents the most complicated departmental story and seems at first glance to be another exception (with Berkeley) to the postwar rule of positivism. Sociology was located between 1946 and 1970

51. Abraham Kaplan (1918–1993) taught at Michigan from 1962 to 1972 before moving to the University of Haifa and was named one of the top ten teachers in the United States by *Time* magazine in 1966.

52. According to the OCLC catalogue, 1,417 copies of Kaplan's text are owned in total by U.S. libraries, as opposed to 1,096 copies of the Festiger and Katz volume.

in the Department of Social Relations, which was founded by academics who were fascinated with theory and "what were then considered the 'softer' sides of the social sciences, especially the relations between personality, culture, and society" (Homans 1984, 294). The social relations program nonetheless quickly began attracting hundreds of applicants annually (Nichols 1998, 90). It produced 80 PhD's between 1946 and 1956 (Johnston 1998, 34). Moreover, Harvard was at the very top of the departmental rankings by sociology department chairs in 1957 (Keniston 1959, 146). In 1964 it was second only to Berkeley, which had shot up from seventh place during the intervening years (Burris 2004). These events would seem to indicate that sociological theory was central in U.S. sociology during the 1950s. In his 1950 presidential address to the ASA, Parsons (1950, 5) argued that the "wave of anti-theoretical empiricism has, I think fortunately, greatly subsided." One common view of this situation is that Harvard played the role of theory maker to the more empirical remainder of the discipline. As Kuklick writes (1973, 16, citing Rossi 1956), "after the Second World War the occupational roles of theoretician and bureaucratized research worker became entirely distinct."

If we examine this picture a bit more closely, however, things quickly become more complex. First, the division of labor between theory and *empirie* in sociology, to the extent that it actually existed, was superimposed on deeper agreements about basic principles and goals. The alleged struggle between "operationalism" and "functionalism" did not go to the heart of the consensus on what counted as scientific capital. The 1953 collection *Working Papers in the Theory of Action,* for instance, included an entirely empirical paper by Bales (Parsons, Bales, and Shils 1953). Second, aside from Sorokin (who had been marginalized in the meantime), Parsons was Harvard's only theorist, if we consider theory as more than a restatement of causal relations among variables. Parsons's main coauthor in his theoretical texts written during the 1950s, at the height of his (and Harvard's) power and influence, was not one of his Harvard colleagues but Edward Shils, who taught at Harvard as a visitor. Most of the Harvard sociology PhD's from this period who went on to illustrious careers pursued Mertonian "middle-range" topics and theories.[53] Kingsley Davis (1959, 767), a student of Par-

53. Craig Calhoun made this point in earlier comments to the author. One might include in this list of early Department of Social Relations PhD's whose dissertations were in this "middle-range" vein Marion Levy (1948), James A. Davis (1955), Robert N. Bellah (1955), Neil Smelser (1958), and Ezra Vogel (1958). Harold Garfinkel (PhD, 1952) had more microscopic research objects, but his theoretical ambitions were sweeping, while Edward Tiryakian's work after his 1956 dissertation moved in the direction of sociological theory.

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Third, Parsons muted and even recanted some of his prewar antipositivism during the 1950s. Parsons was criticized by Gouldner in 1970 for his alleged positivism, but he had explicitly rejected "the positivistic-utilitarian tradition" in his *Structure of Social Action* (1937). Yet Parsons also argued, both before and after the war, that "the same philosophical principles that guided the natural sciences were at the heart of the social sciences" (Klausner and Lidz 1986, vii). *Working Papers in the Theory of Action* opened with two illuminating chapters by Parsons on the superego and symbolism, but the subsequent chapters, coauthored with Robert F. Bales and Edward Shils, employed a scientistic language derived more from physics and cybernetics and terms from the older lexicons of biology and evolution. The social system was described here as a space-age orrery populated by "particles," "inertia," "flows," "phase movements," "feedback loops," "orbits," and "input-output processes" (quotes from Parsons, Bales, and Shils 1953, 164–68, 210, 212, 214, 217–22). Thus the archpositivist Lundberg (1956, 21) could write convincingly in the mid-1950s that there was now "considerable agreement among the systems" of Parsons-Bales and Stuart Dodd.[54] Finally, the interdisciplinarity of the social relations department bore little resemblance to some current versions of interdisciplinarity as a playground of epistemic diversity and experimentation. Parsons's vision of a convergence of theory in the various social sciences rather recalled the logical positivist dream of the unity of science (Carnap 1934). As Parsons wrote in his preface to the programmatic book *Toward a General Theory of Action*, "these many streams of thought are in the process of flowing together" (1951, viii).

Nor was Parsons the only Harvard sociologist whose own antipositivism was muted or nonexistent. Statistician Samuel Stouffer, who ran the Harvard department's Laboratory of Social Relations, was seen as having "destroyed one of the principal contentions of the case-study side" of the

54. I am skeptical of Platt's (1996, 202–3) claim that Parsons's "lack of methodological commitments" meant that his influence had "no consequences for method." Platt also maintains that Lundberg was *not* taken too seriously (although she herself devotes considerable space to him). But Theodor Abel (2001, 323) criticized "the influences of Parsons and of Lundberg" in one breath in 1950 and discussed Lundberg repeatedly in his diaries between 1931 and 1957; see also Angell (1930, 345; 1945, 229–30). Four years later Hinkle and Hinkle discussed Lundberg as one of five contemporary U.S. sociologists in detail, describing him as "the leading exponent of neo-positivism in contemporary American sociology" and concluding that his "continuing endeavor to develop a sociology modeled upon the physical sciences, especially physics, has had considerable influence among younger sociologists" (1954, 54). Sorokin considered Lundberg important enough to include him as a target of his 1956 antiscientistic tract.

debate over statistics versus the case-study method in his Chicago dissertation (Faris 1970, 114). Alex Inkeles rendered Parsons's *implicit* modernization theory more explicit (Gilman 2003, chap. 3). Modernization theory was essentially positivist in denying that causal mechanisms and paths of development vary across time and space (Gaonkar 2001; Harootunian 2004; Steinmetz 1999). Inkeles (1974) combined these ontological assumptions with survey methods, measuring individuals' levels of modernity in various countries. The most influential counterweight to Parsons in the sociology wing of the social relations department in this period was George Homans, who had replaced Parsons as the most-cited sociologist in the United States by 1964 (Chriss 1995). Homans disliked "grand theory" and claimed to have told Parsons in a faculty meeting that "no member shall be put under any pressure to read" Parsons and Shils's (1951) new treatise (Homans 1984, 303). Homans was explicitly positivistic (1947, 14) and invoked Ernst Mach, the godfather of post-Comtean social science positivism, insisting that "science consists of the 'careful and complete description of the mere facts'" and avoids "why" questions to focus on "how" questions. According to Dennis Wrong (1971, 251), Homans dismissed "all intellectual traditions in sociology stemming from nonempiricist philosophies as 'guff.'" Homans also staked out a methodological individualist ontology, arguing that Durkheim was simply wrong ("that is, untrue") in claiming society to be "an entity *sui generis,* something more than the resultant of individual human beings" (Homans 1984, 297–98).

ALTERNATIVE DIAGNOSES OF POSTWAR AMERICAN SOCIOLOGY

Several arguments have been mobilized against the thesis of postwar dominance of positivism in U.S. sociology.[55] Some point out that sociologists interact with people from other disciplines, that they read widely, and that they are not restricted to sociology departments. Sociologists would therefore be exposed to a more varied epistemological menu. But this no more challenges the fieldlike character of sociology than pointing to individual dissenters or even entire departments of dissenters: such diversity is one of the very conditions of existence of a field. A second counterargument can also be quickly disposed of. This concerns the lack of explicit references to positivism by those associated with the tendencies I am calling method-

55. The first two counterarguments have been made explicitly to me by anonymous reviewers and by discussants of an earlier draft of this chapter.

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ological positivism. American sociologists more or less agreed about what positivism was. The widely used 1944 *Dictionary of Sociology* provided a concise definition of the term (Fairchild 1944, 226). Lundberg pounded it into them in article after article and book after book. Students at Columbia and elsewhere read Nagel. By 1983, Raymond Williams could remark that positivism was "a swear-word, by which nobody is swearing" (although he also acknowledged that "the real argument is still there"; 1983, 239). But these overwhelmingly pejorative connotations of positivism date to the 1960s. Moreover, any philosophical realist will admit that unnamed social structures exist and can influence empirical events. Even though the logical positivists and their successors in sociology started to drop the term *positivism* in favor of alternatives like *nomothetic-deductive,* they continued to argue for (1) general and empirical laws (or a "postulate of regularity in the sequence of events"; Shils 1961a, 1419); (2) a view of the social as a closed system (such closure is a precondition for "regularity determinism"; Bhaskar 1975/1978); (3) doctrines of prediction and forecasting (Comte's "savoir pour prévoir et prévoir pour pouvoir"—"from knowledge comes prediction, and from prediction comes power") and (4) falsification (the Popperian reformulation); (5) a spontaneous preference for "parsimonious" explanations (forgetting that the first definition of *parsimonious* is "stingy"); (6) a belief that mathematical and statistical modes of analysis and representation are superior to linguistic forms; and (7) adherence to an idealized view of the natural sciences as a model for the human sciences, that is, to scientism.

According to a third argument, there is a lack of fit between positivist philosophical doctrines and actual sociological research practices. Mullins and Mullins (1973, 218) asserted, for example, that "the simple fact is that almost no theories" of the positivist kind "were ever tried in sociology" (see also Platt 1996 for a similar objection). But this assumes that the only relevant definition of positivism is in terms of some philosophical urtext. Mullins and Mullins (1973, 221) also point to a cluster of "new causal theorists"—a.k.a. positivists—"scattered" across a number of universities. Some writers (Kuklick 1973) argue that twentieth-century positivism was usually deductivist, while mainstream sociological practice has been largely inductivist (throwing data into a computer program and coming up with ad hoc explanations for correlations) and hence not positivist. But as Roy Bhaskar (1975/1978, 1979) argued, the patron saint of the spontaneous positivism of scientists is David Hume, theorist of the inductively discovered "constant conjunctions of events." Moreover, some logical positivists were willing to adjust their theory in the direction of the actual practices of positivist social researchers. In 1951 Hans Reichenbach (1951) proposed a loosening of the Humean "necessity" model of the scientific law to allow for probabilistic "laws" in the social sciences, and this was developed further by Nagel (1961/1979, 503–20).

A final argument insists on the diversity of sociological practices even during the 1950s and 1960s.[56] Berkeley's sociology department, which was at the top of the comparative rankings by 1964, had a number of people like Erving Goffman, Neil Smelser, William Kornhauser, Reinhard Bendix, and Herbert Blumer who pursed nonpositivist styles of research. Like Bendix, a student of Wirth who wrote a Chicago thesis (1943) on German sociology and began teaching at Berkeley in 1947, most of them seem not to have worried publicly about the divergence between their own styles of sociology and the dominant model at the time (Bendix 1990).[57] It is remarkable, however, that muted criticism of positivism came from Herbert Blumer in the pages of the *AJS* against the doctrine of value neutrality in 1940 and again in 1956. Smelser later (1986) published a penetrating critique (in German) of American sociological scientism called "The Persistence of Positivism in American Sociology."[58] Lipset commented frequently on social science positivism. But my argument about the postwar settlement really extends only to the mid-1960s, and public disciplinary recognition of Berkeley seems to come at the very end of this period, despite the fact that many members of this crew had already been there for some time. Did Berkeley change or were the conditions undergirding the postwar settlement beginning to crumble, allowing less positivist forms of sociology to gain recognition?

Even with respect to the 1945–65 period, one might also point out that there was a handful of epistemological dissidents even in the early 1960s, many of them contributors to Irving Louis Horowitz's (1964b) collection in honor of C. Wright Mills, optimistically titled *The New Sociology.* A handful of recognized sociologists continued to criticize methodological positivism directly and publicly during the 1950s. Gerth's epistemological leanings had to be gleaned mainly from his decisions about what to translate (and his teaching), although one published essay attacked sociology for its ahistoricism and antitheoretical bias (1959; see also Gerth and Mills 1964). There was a "humanist" rebuttal at the 1962 meetings to ASA president Paul Lazarsfeld's call for an empiricist sociology, but this came not from a sociologist at all but from historian Arthur Schlesinger Jr. (1962). Alvin Gouldner inched up to his full-blown epistemological critique of U.S. sociological

56. This argument was proposed by the editor of this volume to the author.

57. The same goes for Leo Lowenthal, the only member of the Institute of Social Research who had a career in U.S. sociology.

58. This article has, to my knowledge, never appeared in English.

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positivism only gradually. His *Patterns of Industrial Bureaucracy* quoted Homans to the effect that "sociology may miss a great deal if it tries to be too quantitative *too soon*" (Gouldner 1954, 17; my emphasis)—conceding that quantification was the field's ultimate goal. During the early 1960s, as Friedrichs noted (1970), there was a small but noticeable uptick in works arguing that sociology should be seen as part of the humanities, discussing its links to history and philosophy, questioning value neutrality, and proposing a more self-reflexive approach to the discipline, a "sociology of sociology" (see Wolff 1959; Nisbet 1962; Berger 1963; Horowitz 1963; Bierstedt 1960).[59]

A sociology of sociological epistemology would have to determine whether these dissident positions could match the positivist mainstream in terms of their symbolic capital. In fact, many of the critics were not centrally located according to a sociological map of disciplinary ranking. Adorno's writings on sociology from the early 1960s were the most sophisticated critiques of positivism in this period, but they were not translated into English until 1976. Shils (1961a, 1407) could thus assert that "the seed of German sociology ripened only when it was transplanted to America," ignoring (or suppressing) the fact that the editors of the leading pre-Nazi era German sociology journal (*Zeitschrift*) were at the time of his writing already back in Germany and restarting critical sociology in Frankfurt. Marcuse worked as an intelligence analyst for the U.S. army during the war and then headed up the Central European Section of the Office of Intelligence Research. When he finally returned to teaching in 1951 it was in philosophy rather than sociology departments, so it is difficult to count him as an American sociologist.[60] Berkeley may have been a top-ranked department by 1964, but American intellectual life and academic sociology had never been significantly located on the West Coast, and it would take some time to change that.

Finally, some of the earlier critics toned down or subtly adjusted their antipositivism (perhaps not deliberately) in ways that made their work more compatible with the new framework. It is difficult to say whether this was instrumental and intentional or the result of the hegemonic sway of the newly dominant ideas in the discipline—or of the positivist worldview's en-

59. Bierstedt, in his 1959 presidential address to the Eastern Sociological Society, insisted that "great sociologists . . . were humanists first" and reminded his listeners that "Veblen used no questionnaires, Sumner no coefficients of correlation" (1960, 5). I. L. Horowitz's antiscientism in this period is discussed in Steinmetz (2005c).

60. The same can be said of a handful of theorists and "qualitative" sociologists like Daniel Bell (Waters 1996) and Barrington Moore Jr. (D. Smith 1983), who were able to stay aloof from the mainstream, partly because of their idiosyncratic career patterns and self-confidence.

hanced plausibility. After Sorokin was marginalized at Harvard by Parsons, he began to turn sharply against methodological positivism, fulminating against "sham-scientific slang," "sham quantification," "the cult of numerology," "pseudo experimentation" posing as "real experiments," social "atomism," sociological "simulacra" of natural sciences, and empiricist philosophy.[61] But Sorokin still did not break with doctrines of predictability and uniform social laws (1956, chap. 11, 312). Even C. Wright Mills, whose 1959 critique of U.S. sociology is often seen as a heroic cri de coeur from the scientistic wilderness, had drifted toward the mainstream, in contrast to his essays from the 1940s. Mills now cast aspersions not only on "abstracted empiricism" but on "grand theory" as well.

THE FIT BETWEEN POSTWAR SOCIOLOGY AND METHODOLOGICAL POSITIVISM

It may be helpful to back up for a moment and specify the linkages between the main dimensions of methodological positivism and the sociology of the postwar period. The most important component of the former was the belief in general social laws of the "if *a*, then *b*" variety, or the probabilistic variants. The case study was therefore unacceptable since one could not generalize from an "*N* of 1" (Stouffer 1930; Steinmetz 2004a). There was a general movement away from the study of the town, neighborhood, or the single individual and toward the survey, whose unit of analysis was the individual, multiplied (Coleman 1990, 92).[62] Nonsurvey researchers followed suit. Guy Swanson's *Birth of the Gods* (1960) used statistical methods on a sample of fifty "simpler societies" to test a Durkheimian model of religion, gathering information from Swanson's "idiographic" colleagues like Horace Miner, who had written a book on Timbuktu (1960). The existence of Berelson and Steiner's (1964) inventory of 1,045 sociological "findings of proper generality"—some of them couched in multiple ceteris paribus clauses[63]—suggests that assumptions about causal regularity were wide-

61. Several years later Sorokin ended his autobiography with the suggestion that the empiricist "sensate order" was leading to the destruction of mankind and an "abomination of desolation" (1963, 324).

62. As one reviewer of this essay pointed out, many early postwar surveys were actually conducted in a limited geographic area but presented as if they had sampled the entire United States

63. An example, taken almost at random: "Recourse to prayer under combat conditions is common among (American) soldiers, especially for those under great stress and with few personal resources for coping with the situation" (Berelson and Steiner 1964, 447). Needless to say, this was based on Stouffer et al. (1949–50), the template for much of the postwar work in this vein.

spread at the time. Historical sociology was considered out of bounds, except as a means of increasing the number of data points (Sorokin 1956, 50).

Despite this holy grail of the universal law, there is no ontological reason to expect the social world to exhibit such regularities. This failure of the real world to conform to positivist philosophical assumptions meant that empirical social scientists had to loosen the constraint of universalism, emphasizing *probabilistic* laws instead. Hans Reichenbach (1951, 122) argued soothingly in a symposium with the nation's leading social scientists in 1951 that "whoever wants to carry on Humean empiricism in modern times must be willing to accept the frequency interpretation of probability." Social statisticians came up with the idea of "scope conditions," which gestured toward social reality while sticking to the ontological assumption that social causes are *normally* universal, that is, independent of time and space. By the same token, the concept of "path dependency" is epistemically incoherent unless one starts from the positivist assumption that social or historical processes are ideally, essentially, or usually *not* determined by a contingent array of prior conditions and processes. In attempting to force their data into the framework of if-then statements, statistical sociologists also began resorting to interaction terms. But they could not agree to represent history as the result of ever-changing congeries of causal processes or mechanisms, even though this was in fact more adequate to the real qualities of the social world (Bhaskar 1975/1978; Sewell 1996; Steinmetz 1998). Instead, the social was at least implicitly treated as a *closed system*. Parsonian functionalism was famously unable to account for change. Even when Parsons began more explicitly laying the groundwork for a modernization-theoretical account of historical change, history was constrained to follow a path that flattened it out in ways that allowed for an "essential section" of historical time, "a break in the present such that all the elements of the whole revealed by this section are in an immediate relation with one another, a relation that immediately expresses their inner essence" (Althusser 1970, 94). Such closure is not just politically conservative, as was often argued in the 1960s and 1970s; it is also an ontological precondition for the existence of general laws, even if these were laws involving depth-realist concepts as their *explanans*. This is one sense in which structural functionalism, which Mullins and Mullins (1973, 39) called "the faith of our fathers" and Kuklick (1973) analyzed as a triumphant Kuhnian paradigm, corresponded to methodological positivism at the level of its deepest assumptions, even if its depth realism and "hyper-theoreticism" made it a target of dismissal by the more empirically oriented sociologists (who were located on both the left and the right politically).

The second sign of this realignment was the increased emphasis on strictly *empirical* concepts and "theories" (see Willer and Willer 1973). The supposed theoretical dominance of Parsons does not sit well with this claim that empiricism was pervasive. However, Parsons was more often admired than emulated (at least until Niklas Luhmann came along). The officially approved Mertonian formula of "middle range theory" combined with empirical research guided most of Parsons's ostensible followers. Platt (1996, 192) finds a continuous *decrease* in theoretical articles and an *increase* in empirical ones in the three leading sociology journals during the 1920–90 period, with the sharpest decline occurring between the 1950s and 1960s. Depth-realist concepts also dropped out of sociological writing. Operationalist methods were often described as empirical measures that tapped underlying entities (Kaplan 1964, 297), and social statisticians sometimes talked about the murky issue of the *substantive* adequacy of their measures to those deeper objects. But in sociology, at least, "latent" variables were usually construed as also being located on the empirical level, even if they were not directly measured. Lazarsfeld (1951, 156) recommended that the researcher involved in creating concepts should start "with fairly concrete categories." Even after Carnap and other logical positivists began to acknowledge that scientific concepts could *not* be reduced "to the given, i.e. sense-data, or to observable properties of physical things" and to admit the possibility of "theoretical concepts" (Carnap 1956; also 1966, chap. 23), the latter were "regarded as mere devices for deriving the sentences that *really* state the empirical facts, namely the observation sentences" (Putnam 2002, 24).

Empiricism was linked to the doctrine of value neutrality. As Herbert Marcuse pointed out at the debates about Weberian social science at the 1965 German Sociological Congress, "Weber's notion of value freedom simply meant that he refused to subject his own values to any kind of rational criticism" (Zimmerman 2006, 74; Marcuse 1971). And as critics often pointed out during the 1960s, sociology had implicitly allied itself with the reproduction of the current social order by declaring the unobservable or the speculative off limits for scientific discussion.

U.S. sociology also moved away from the interpretivist approaches inspired by Mead, Cooley, Freud, and the various tendencies grouped under phenomenology and symbolic interactionism (despite the just-mentioned short burst of activity in the latter movement during the 1950s). The existence of a "culture concept" in functionalist sociology or an interest in psychology or even psychoanalysis does not really contradict this point. Culture, personality, and subjectivity could all be treated positivistically, as in rational choice theory (Somers 1998). In their analysis of "theory groups" in U.S. sociology, Mullins and Mullins (1973) argued that the "cluster stage"

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1945–51 "was notable intellectually for the introduction of Freudian psychology." But the richer and more interpretive Freudian categories such as the unconscious, irrationality, and "the return of the repressed" were largely absent now, and most sociologists seem to have ignored Parsons's own intense interest in at least the superego elements of psychoanalysis. As Kuklick (1973) and Camic (1986) show, postwar sociology's "revolt against behaviorism" also led it to reject what it saw as biological approaches, including the concept of habit. Psychoanalysis was thus rejected both for its antibiologistic and depth-psychoanalytic concepts like the unconscious and for its biologistic tendencies.

The tendency to understand the mechanisms and laws of the social sciences as universal was related to postwar American sociology's geographical and temporal focus, which typically took the United States in the present as its object or, in modernization theory, as the standard for comparison. Categories like "social class" or "social development" were assumed to exist transhistorically and trans-spatially, meaning that it was unproblematic to apply them to earlier historical periods and distant, disparate places. There was a view of historical sociology as "merely the sociology of the past" and as "valuable above all because it increases the number of data points" (Sewell 1996, 246). By the same token, positivist comparativism recommended "replacing proper names of social systems by the relevant variables" (Przeworski and Teune 1970, 30).

The question of sociology's treatment of historical time is complicated by the fact that social evolutionary theory in the eighteenth and nineteenth centuries had already construed colonized parts of the world as earlier stages of the modern West (Fabian 1983). This was reinvigorated in the postwar period under the guise of modernization theory.[64] Connell (1997) sees American sociology's inward turning and its geospatial provincialism as beginning earlier in the twentieth century and continuing after World War II. But this is too general. There were few leading disciplinary figures during the postwar period who were as globally oriented as Edward Ross (1936), but Connell ignores Russian/Soviet specialists like the early Barrington Moore Jr. or Alex Inkeles, Africanists like Immanuel Wallerstein and Horace Miner, as well as comparativists working on "simper societies" like Guy Swanson. The point is not that modernization theory ignored the rest of the world but that it applied the "parochial" concepts of the European-American "ethnosocial sciences" to foreign realities. Such theories do not ig-

64. In addition to the references given previously on modernization theory, see also Tripp (1973), Mazrui (1968), and Cooper and Packard (1997).

nore difference so much as they disavow it by subjecting the other to a standardizing logic (Lambek 1991; Clifford and Marcus 1986; Bhabha 1994).

An even simpler approach to global difference was expressed by adherents of the "natural science" approach in postwar sociology. Here the difference between self and other was collapsed altogether, and the life of "non-Western" cultures was equated with the American way of life. The popular postwar sociology textbook by Lundberg, Schrag, and Larsen (1954) pursued such cross-cultural equations relentlessly (see fig. 9.5). S. Dodd's *Project Revere* research for the Korean War effort of the air force examined the effectiveness of air-dropped leaflets in rural towns in Washington State. The explicit assumption here was that rural Washingtonians were identical to Korean peasants rather than being located at a higher stage of a uniform developmental trajectory (DeFleur and Larsen 1958; Robin 2001, 100). Horace Miner mocked such beliefs in his notorious analysis of "Body Ritual among the Nacirema" (1956).

Lest one think that these gestures were found only among the now disparaged acolytes of the "natural science" approach, it is important to recall that the leading voices of postwar sociological theory often claimed to be developing an all-encompassing approach. Edward Shils coauthored with Parsons the chapter in *Toward a General Theory of Action* (1951, chap. 1) that introduced the infamous "pattern variables" that provided modernization

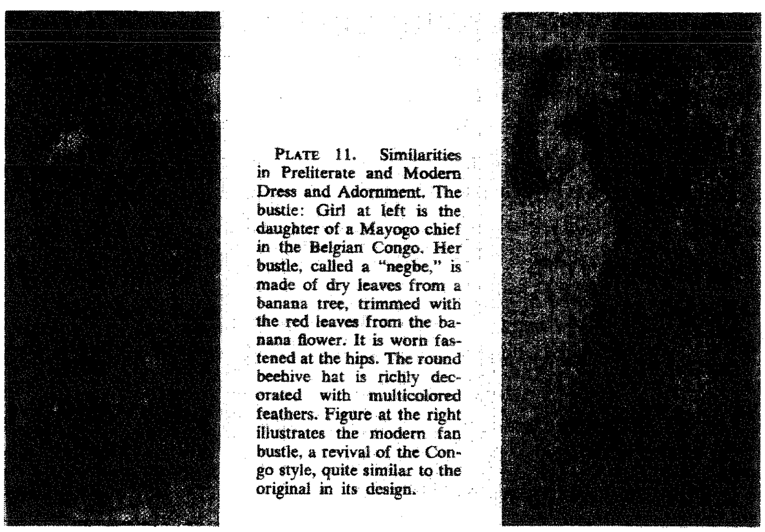

Figure 9.5 A figure from Lundberg, Schrag, and Larsen (1954), illustrating "similarities in preliterate and modern dress and adornment." Courtesy of Harper & Bros.

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theory with some of the categories used to characterize traditional societies (ascription, particularism, and so forth; see Gilman 2003, 86–88). But in the concluding essay of *Theories of Society,* Shils wrote: "Sociological theory as it stands today is . . . [a] sort of short-hand description of the chief features of 'modern society,' with occasional extensions to non-western and nonmodern societies. It is the aim of general theory to become genuinely universal and transhistorical . . . attain[ing] a generality of scope . . . that render[s] it equally applicable to all societies of the past and present" (1961a, 1424). In some respects this approach was even better suited to American imperialism than modernization theory. Since the Suez crisis, the United States had started to support decolonization (Louis and Robinson 1993). The preferred American modus operandi was informal domination that left local sovereignty intact, even insisting on self-rule (Steinmetz 2005f; Liu 1999). Modernization theory infantilized non-Western peoples but at least predicted their eventual convergence with modernity, whereas the colonial rule of difference (Chatterjee 1993) deferred such convergence indefinitely and therefore generated ideologies of the non-Westerner's inherent inferiority.

Here and elsewhere, Shils criticized American sociology's technocratic and manipulative scientism, its empiricism and positivism, its "deficient sense of the past," and even the "disposition to universalize what is, in fact, particular to one society and one epoch." He commented appreciatively, if superficially, on psychoanalysis and on the humanities and the "geisteswissenschaftliche analysis of the realm of symbolic forms," and called for self-reflexivity, the study of history, and attention to the classics. But none of this prevented Shils from insisting that sociology was fundamentally about the search to discover the "variables" underlying "general laws" of action.[65]

Finally, the scientistic program encompassed a new level of enthusiasm for statistical research and for the accoutrements of scientific high modernism (see figs. 3 and 4). As Charles Lindblom (1997, 227) wrote, "in the 1940s and 1950s . . . thanks to new techniques and the computer, the social sciences were perceived as becoming harder, that is, more like the hard sciences" (see also Inkeles 1964, 43). Although we cannot equate quantitative

65. Shils (1961a, 1427, 1433, 1444, 1411, 1417); also Shils (1948). Nor, it might be added, did this prevent Shils from asserting with spectacular arrogance and without referring to a single shred of empirical evidence that contemporary "Western societies" (by which he clearly meant the United States) were "more decently integrated" in the sense that "there is more mutual awareness, more perception of others, more imaginative empathy with the states of mind and motivations of others, more fellow feeling" than in "any societies that have preceded them in world history or are contemporaneous with them in other parts of the world" (1961a, 1429).

or statistical methods with methodological positivism, they clearly certainly had elective affinities. In an analysis of the methods used in the main sociology journals, Platt (1996, 191–93) finds the greatest decadal increase in the use of quantitative methods between the 1940s and 1950s and between the 1950s and the 1960s. By the 1960s a substantial majority of total articles were quantitative.[66]

Conclusion: Fordism and the Awakening of Spontaneous Positivism among the Sociologists

The consolidation of the sociological-scientific field after World War II cannot be explained simply in terms of disciplinary maturation or generational succession. Nor is it satisfying to argue that new sciences tend to emulate the currently most prestigious disciplines.[67] Other newcomer disciplines that emerged at the end of the nineteenth century, most notably anthropology, did not follow the same path as sociology (Keane 2005), and sociology itself did not converge around emulation of any particular "harder" science, whether physics, biology, or economics, during the interwar period. Bourdieu's theoretical framework cannot account for the specific substantive contents of a settled scientific field, that is, it cannot explain why certain epistemologies or theories are felt to be more distinguished than others. There is no universal reason, intrinsic to the study of the social, why methodological positivism came to have more field-specific capital than nonpositivism.

The Holocaust and the cold war made the postwar period inherently different from the prewar period. The impact of the cold war has been rightly emphasized in historical writing about area studies and modernization theory in this period, and the role of emigration from Europe and the Holocaust on American social science has also been explored (e.g., Suedfeld 2001; Srubar 1988). But the impact on sociology of the epochal shifts in the organization of social life in the advanced capitalist world has not been ad-

66. It is also crucial, as R. Miller (1987, 240–41n11) suggests in a discussion of Blalock, to examine statistical as well as broader methodology texts. This is especially true since statistics courses have long been the prime venue for the communication to sociology graduate students of overarching metaphysical and epistemic premises. As Inkeles (1964, 42) pointed out, many sociologists who use statistical methods "would be somewhat surprised if you were to point out that in the mere adoption of a particular statistical technique they are accepting a certain" social ontology, or a model as an "appropriate description of the social world." For one discussion of the ontological implications of different statistical methods, see Abbott (2005).

67. This argument is made by Camic (1995; Camic and Xie 1994), although it is certainly not his entire account.

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68. An instructive case in point is Irving Louis
dent critic of sociological "scientism" and the "supp
(in 1964a) to attacking the "bobble heads" who "spe
"empiricism" (Steinmetz 2005c, 487–89).

69. Other social science disciplines responded
conditioned by their location in the university field
thropology, specialized in the cultures of the coloni

dressed in any detail. The most significant and all-encompassing new condition after the war was the consolidation of the social forms that retrospectively have come to be known as Atlantic Fordism. By the 1950s and early 1960s, the intellectual, financial, and political forces that had existed before 1945 within sociology combined with the ideological effects of the newly consolidated Fordist mode of regulation to help lift methodological positivism to triumph. On the one hand, the Fordist security state relied to a greater extent than previous regulatory forms on the skills of sociologists, and this entailed a greatly enhanced level of state funding for sociological research. Sociologists' opportunistic efforts to cash in and curry favor with political, military, and business elites played a powerful role in the postwar convergence of the field around positivism. Cold war anticommunism also played a role in convincing some sociologists to embrace the "value-free" ethos and in marginalizing people like C. Wright Mills who retained a political commitment to the "wrong" side. The antiscientific policies and the overall irrationalism of the Nazi and Stalinist regimes played an additional role in convincing some people that politics and science did not belong together, although for the most part the "antitotalitarian" impetus for positivism set in after the 1960s and 1970s with the turning against "political correctness."[68]

The social patterns that regulation theory groups under the heading of Fordism played an equally important role in making methodological positivism seem more plausible to a larger group of sociologists. The Fordist patterning of many aspects of social life in the advanced capitalist world resonated with positivist approaches to social explanation for people who were exposed to those new logics. This is not to say that sociologists actually described the new Fordist conditions. As Camic notes with respect to the Depression, most sociologists display a "head-in-the-sand response to contemporary events," and the postwar period was no different. Sociologists' métier consisted in observing and making sense of the social, but the new social conditions had a powerful impact in ways that did not figure centrally in their writing and that they may not even have perceived consciously.[69] The deeply unstable conditions of the interwar period partly ex-

68. An instructive case in point is Irving Louis Horowitz, who shifted from being an ardent critic of sociological "scientism" and the "suppress[ion] of values at the expense of facts" (in 1964a) to attacking the "bobble heads" who "speak contemptuously of . . . positivism" and "empiricism" (Steinmetz 2005c, 487–89).

69. Other social science disciplines responded to the emerging Fordist formation in ways conditioned by their location in the university field and their particular objects of inquiry. Anthropology, specialized in the cultures of the colonial and postcolonial peripheries, was more

plain why positivism was less compelling at that time as a description of society. After the war, social reality became more orderly and was presented using tropes of stability, repetition, and "the end of history." All of this corresponded more closely to the positivist expectation that social practices can be subsumed under universal covering laws. Social actors now seemed increasingly atomized and interchangeable, losing any distinctive cultural peculiarities, and thus lending themselves to general models of subjectivity (behaviorism, rational choice, and positivist versions of psychoanalysis). Because social practices were more regular and repetitive, it was plausible to forecast and even to control them.[70] For historically contingent reasons, in other words social reality now resonated powerfully with methodological positivism.

Six aspects of postwar Fordism were especially central to this process.[71] (1) The role of science (including social science) in *state policy* was greatly enhanced within the Fordist form of governmentality in comparison to the previous period. The Fordist state relied on a whole panoply of social and fiscal policies to smooth out the bumps in the crisis-ridden process of capitalist accumulation, and it relied on armies of social scientists to design, evaluate, and administer these programs. This integration of sociology into the domestic and foreign policy scientific infrastructure somewhat paradoxically validated the claim that science was "value free," since social scientists could conceive of themselves as a separate and autarkic scientific community only after they had freed themselves from the private corporate organizations that had dominated social research funding during the prewar period (Mirowski 2005). (2) Fordism contributed indirectly to positivism's plausibility by dampening economic crisis through fiscal policies and by lessening some of the economic turbulence in the individual life course via wage and welfare state policies. These developments, which affected unionized industrial workers and the middle wage-earning classes (including many sociologists), made it more plausible that social practices actually repeated themselves in ways that could be represented with gen-

immune to these positivism-enhancing effects, since the global periphery was only marginally brought under the sway of Fordism and was highly unstable in the postwar period. The "behaviorialist revolution" in political science responded to the form and demands of the Fordist state and politics. Keynesian economics, which emerged before Atlantic Fordism, was still highly regarded. Fordism was also connected to various movements in aesthetic modernism (see T. Smith 1993).

70. See the essays in Crawford and Biderman (1969) for confident statements of this forecasting and control perspective.

71. For more detail see Steinmetz (2005a, 2005d, and forthcoming b).

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eral covering laws, statistical models, and replicable experiments, thus allowing predictions. Historical analysis became irrelevant for sociology because history had effectively "ended." (3) The increasingly *depthless* and *postideological* culture of Fordism that was replicated serially across regions and social groups and classes enhanced the credibility of models predicated on a picture of universal, interchangeable subjectivity, including behaviorist and rational-choice approaches. Interpretive and semiotic approaches to culture could therefore be abandoned. (4) Fordism's specific *spatial* regime made positivism's denial of the importance of geographic difference seem reasonable. Most social practices, including economic transactions, were not located at multiple and shifting scalar levels and sites but were instead concentrated within the "container" of the nation-state. This encouraged social scientists to take the level of the nation-state for granted as a unit of analysis. Fordism also tended to homogenize this national space internally, even if fewer policies were implemented in the United States than in West Germany or northern Europe to undercut uneven regional development (Brenner 1998). (5) A final aspect of this formation that heightened the credibility of positivism was the emerging foreign policy role of the United States. American imperialism did not begin with Fordism, but the United States now became a global hegemon. As the leading economic power, U.S. hegemony after World War II was oriented toward shaping the world into an open capitalist market via an anticolonial stance (Louis and Robinson 1993; Bergesen and Schoenberg 1980). The United States eschewed direct colonization and was therefore not compelled to enforce a racial "rule of difference" (Chatterjee 1993) in its peripheral dependencies but could promote the convergence of its peripheries with the American way of life. Competition with the USSR for the hearts and minds of the third world also meant that the state encouraged counterinsurgency research like Project Camelot, as long as it promised to yield applicable "lessons for empire" (Steinmetz 2004b). "Ideographic" researchers who learned the languages and histories of specific places tended to be relegated to dominated corners of the discipline, from whence they have only recently been emerging.

In this chapter I have traced the emergence of a hegemonic disciplinary formation and identified some of the ways Fordism resonated with (and directly promoted) sociological positivism after World War II. To claim that sociologists' spontaneous images and theories of the social were influenced by the wider social structures they inhabit does not mean that such images directly *reflect* social reality. Rather, sociologists (like other people) tried to make sense of their own social world, and one of the available models was the positivist one. They were aided in this by the fact that the Fordist mode 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 scientistic 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

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[TEN] Orthodoxy, Heterod "Mainstream" Sociology a

Craig Calhoun & Jonatha

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selves as they do about taxicab drivers or do
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ALVIN GOULDNER, *The Coming C*

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PIERRE BOURDIEU, *Language*

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West Coast sociology departments—like
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