The Genealogy of a Positivist Haunting: Comparing Prewar and Postwar U.S. Sociology

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This essay asks two related questions about the discipline of sociology in the United States during the middle decades of the twentieth century. The first of these questions is historical and comparative, and relates to the dominant epistemological[1] orientation in U.S. sociology in the two decades after World War II. What accounts for the postwar narrowing of sociology's epistemological and methodological diversity, or more precisely, for the shift from a relative balance between nonpositivist and positivist orientations in the interwar period to a clear positivist dominance of the discipline? A second and related problem is a counterfactual one and concerns U.S. sociology's substantive geographic focus, its emphasis on the United States to the relative exclusion of the rest of the world. Why did U.S. soci-

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1. I will use the adjective *epistemological* in this essay as shorthand to encompass ontological and methodological issues as well, except when specified; see my "American Sociology's Epistemological Unconscious and the Transition to Post-Fordism: The Case of Historical Sociology," in *Remaking Modernity*, ed. Julia Adams, Elisabeth Clemens, and Ann Orloff (Durham, N.C.: Duke University Press, 2004), 109–57.

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ologists remain focused on their own country, or treat the rest of the world in ways that disavowed or obliterated its difference from the United States? Why did they persist in this provincialism during a period in which the geographic reach of U.S. global empire was expanding exponentially?

In approaching the history of a discipline such as sociology, it is best to examine both the "internal or autonomous forces that shape the development of scientific inquiry on the one hand and those that arise externally in the cultural or social milieus of that scientific enterprise on the other."[2] More specifically, I will theorize sociology here "internally" in terms of its *fieldlike* qualities (or lack thereof) in Pierre Bourdieu's sense, asking about the emergence of recognized or agreed-upon definitions of unequally distributed social-scientific capital. But such a field-level analysis cannot explain why one particular definition of scientific distinction—in this case, the methodological-positivist definition—was able to ascend to paramountcy only after World War II. By the same token, Bruno Latour's approach can tell us what sorts of strategies scientists are likely to use in seeking scientific capital, but it cannot explain why the same techniques fail in one historical period and succeed in another. The *external* determinant that is emphasized in most of the literature on the sociology of the social sciences is money. The story of the rise of scientism and "big social science" has often emphasized the influx of federal funding after 1945.[3]

While this was certainly part of the conjunctural mix that helped the methodological positivists achieve preeminence, it was not enough. The explosion of funding for social science framed as a natural science was *itself* part of a broader societal-level formation that arose after the war: Fordism. Analyzing the relations between social science and Fordism is a corrective to the rigid distinction between "internalist" and "externalist" approaches in science studies, insofar as the internal workings of social science were directly linked to some of the more encompassing patterns of social life.

Yet even this is too one-sidedly economistic. An additional process that prepared the ground for positivism's sweeping victory inside sociology after 1945 has to do with the spontaneous social epistemologies that

2. Neil Smelser, "External Influences on Sociology," in *Sociology and Its Publics*, ed. Terence C. Halliday and Morris Janowitz (Chicago: University of Chicago Press, 1992), 43.
3. See especially Stephen Park and Jonathan H. Turner, *The Impossible Science: An Institutional Analysis of American Sociology* (Newbury Park, Calif.: Sage, 1990). For a parallel analysis of anthropology in this period, see David H. Price, "Subtle Means and Enticing Carrots: The Impact of Funding on American Cold War Anthropology," *Critique of Anthropology* 23, no. 4 (2003): 373–401.

were encouraged by Fordism. Sociologists were themselves participants in everyday forms of Fordist *societalization* (*Vergesellschaftung*) and individual structures of *socialization*. Sociologists' intuitive images of the social became more closely aligned with positivism than had been the case before the war.[4]

1. Methodological Positivism in U.S. Sociology Before and After World War II: From Epistemological Diversity to an Organized Field

U.S. sociology before 1945 already had a powerful positivist wing. By "positivism" I am referring neither to the philosophical doctrines of logical positivism nor to Auguste Comte's version, but instead to a historically specific set of practices, conventions, and assumptions about knowledge of the social. Elsewhere I have argued that this cluster can best be characterized as "methodological positivism."[5] This formation emerged between the late nineteenth and the mid-twentieth centuries, and it continues to evolve and flourish in the social sciences today.[6] Its most important features are (1) an epistemological commitment to covering laws, that is, to the identification of Humean "constant conjunctions" of empirical events; (2) an empiricist *ontology* (although this aspect has become somewhat less central in recent decades with the ascendance of rational choice theory, which is often anti-empiricist); and (3) a set of *scientistic* assumptions stemming from the belief that the methods of the human and the natural sciences should be identical. This third premise has meant that the objects and practices studied by sociologists were treated as brute material facts whose identity was independent of what people thought about them. It also meant that "social facts," like natural ones, were subject to "invariable natu-

4. This resonance between positivism and environing patterns of social regulation has come partly out of joint under present-day post-Fordism, a problem I address in "Scientific Authority and the Transition to Post-Fordism: The Plausibility of Positivism in U.S. Sociology Since 1945," in *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others*, ed. George Steinmetz (Durham, N.C.: Duke University Press, 2005).

5. Alternative terms that attempt to capture a similar cluster of disciplinary characteristics include *instrumental positivism* and *objectivism*.

6. For a comparative account of the versions of positivism that have prevailed in the various U.S. social science disciplines, see my introduction to *The Politics of Method in the Human Sciences*. See also the recent work of Immanuel Wallerstein, which has stimulated some social scientists to rethink their "nineteenth-century" assumptions.

ral Laws" (Comte) independent of time and place. Methodologically, scientism also suggested that the human sciences should strive to become quantitative and experimental. Finally, it was held that normative evaluations should not (and do not) impinge on social facts. This complex of elements constituted sociology's prevailing version of methodological positivism. Certain features were stressed more than others by different sociologists and in different times, places, and texts. But the core of this formation, the element without which it could not be called positivist in an epistemological sense, was the commitment to the discovery of general laws of social action. With respect to this, the positions of the ostensibly antipositivist sociologist Talcott Parsons and his explicitly positivist opponents between the 1930s and the 1950s were indistinguishable.[7] Just as Rudolf Carnap and other logical positivists began to distance themselves from strict empiricism by the end of the 1930s, acknowledging that scientific concepts could *not* be reduced to "sense-data," social scientists have been happy to reconstruct the idea of the general law as linking a depth-realist "mechanism," such as human rationality or the "mode of production," to surface-level events in a regular, lawlike way. And epistemically dissident movements in sociology have been repeatedly channeled back in the direction of the "natural sciences" approach.

Some have argued that a positivist syndrome already dominated U.S. sociology during the first half of the twentieth century.[8] According to Stephen Turner, positivist premises have been passed on tacitly from generation to generation, starting with the founder of Columbia University's sociology department, Franklin Giddings, through to the leading figures of present-day sociology.[9] During the interwar period, an increasing number of sociologists

7. Contrast, for example, Talcott Parsons, *The Structure of Social Action*, vol. 2 (1937; repr., New York: Free Press, 1949), 774, and the writings of George Lundberg, including "The Natural Science Trend in Sociology," *American Journal of Sociology* 61, no. 3 (November 1955): 191–202.

8. See Robert C. Bannister, *Sociology and Scientism* (Chapel Hill: University of North Carolina Press, 1987); Christopher Bryant, *Positivism in Social Theory and Research* (New York: Macmillan, 1985); and Dorothy Ross, *The Origins of American Social Science* (Cambridge: Cambridge University Press, 1991).

9. Turner stresses that Giddings trained numerous students who then went on to occupy leading roles in sociology departments around the country; see "The Origins of 'Mainstream Sociology' and Other Issues in the History of American Sociology," *Social Epistemology* 8, no. 1 (January–March 1994): 41–67. The other leading early sociology department, at the University of Chicago, was founded by Albion Small, who had been exposed to the "humanistic" side of the nineteenth-century "methods struggle" in Germany but later

endorsed the idea that sociology should pattern itself on the natural sciences. In his presidential address to the American Sociological Society in 1926, for example, John L. Gillin declared that "the application of the scientific method and the increased emphasis upon objective data have been acting as *selective agents* in consigning these enemies of sociology"—social theorists and social activists—"to a *deserved innocuous desuetude*."[10] Viennese logical positivism began making inroads into the sociological scene, even if the philosophers' more explicit appeals to social scientists came after World War II (culminating in Ernest Nagel's widely read *The Structure of Science: Problems in the Logic of Scientific Explanation* [1961]).[11] Sociology textbooks from this era also typically recommended emulating the natural sciences.[12]

A closer examination suggests, however, that positivism was far from hegemonic in U.S. sociology before 1945. The discipline was particularly riven during the ideologically turbulent 1930s, which saw discussions of the putative links between positivism and fascism and of the need to reconnect sociology with the humanities and social activism.[13] In addition to the wrenching social disruptions and vibrant political movements outside the academy and the new ideas brought by exiles from Europe, the discipline was also diversified somewhat in terms of its personnel, which became less rural and Protestant (even though it remained overwhelmingly white and

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embraced a scientific naturalism; see E. R. Fuhrman, "Images of the Discipline in Early American Sociology," *Journal of the History of Sociology* 1, no. 1 (Fall 1978): 91–116.

10. At http://www.asanet.org/governance/GillinPresidentialAddress.pdf.

11. See Jennifer Platt and Paul Hoch, "The Vienna Circle in the USA and Empirical Research Methods in Sociology," in *Forced Migration and Scientific Change: Émigré German-Speaking Scientists and Scholars After 1933*, ed. Mitchell G. Ash and Alfons Söllner (Washington, D.C.: German Historical Institute, 1996), 224–45.

12. E. Doyle McCarthey and Robin Das, "American Sociology's Idea of Itself: A Review of the Textbook Literature from the Turn of the Century to the Present," *History of Sociology* 5, no. 2 (Spring 1985): 27–30.

13. On arguments for a connection between fascism and positivism in this period, see Robert C. Bannister, "Principle, Politics, Profession: American Sociologists and Fascism, 1930–1950," in *Sociology Responds to Fascism*, ed. Stephen P. Turner and Dirk Käsler (London and New York: Routledge, 1992), 172–213. Many writers acknowledge sociology's epistemic rivenness in the 1930s but treat it as a sign of the field's immaturity or as an "identity crisis"; see Henrika Kucklick, "A 'Scientific Revolution': Sociological Theory in the United States, 1930–1945," *Sociological Inquiry* 43, no. 1 (1973): 3–22. I discuss the 1930s in more detail in my article "American Sociology Before and After World War Two: The (Temporary) Settling of a Disciplinary Field," in *The History of Sociology in America*, ed. Craig Calhoun (Washington, D.C.: American Sociological Association, forthcoming).

male). The methodological domination of U.S. sociology by the Chicago-style case study declined in this decade, ushering in a theoretical interregnum.[14] The positivists clamorously attacked "grand theory" (which usually, after 1937, referred to Parsons) and interpretive case studies, and promoted quantitative analyses based on surveys or experiments. Physics began to replace biology as the obscure (or not-so-obscure) object of desire. But the field remained in an unsettled, pluralistic state until after 1945. In retrospect, it is also clear that this was one of the more fruitful periods for sociology in the United States, yielding such classics as Robert S. Lynd's *Knowledge for What?* (1939), Robert K. Merton's essays on Puritanism and modern science, Herbert Marcuse's *Reason and Revolution*, and C. Wright Mills's early essays on theory.

U.S. sociology before 1945 was not a well-ordered or hegemonized field. But the difference from the postwar period does not have to do with the *availability* of the ideas and procedures that would become dominant. These were all present in the interwar era. What differed was the *effective* deployment of methodological positivism as a general measure for scientific capital. During the two postwar decades, positivist definitions of field-specific honor came to be universally recognized by all members of the disciplinary field, even if they did not adopt or appreciate it. The dominated sectors developed a "feel for the game" and a "sense of their place" (Bourdieu). Edward Shils stated explicitly what usually remained tacit knowledge, that, with respect to the discipline, "inferiors, however much they scoff, know their betters."[15] Decades later, Bourdieu described U.S. sociology in similar terms (although he was critical rather than celebratory), concluding that "a survey on power in the scientific field could perfectly well consist of . . . epistemological questions alone."[16]

It is important to emphasize that a settled field is not internally homogeneous, contrary to strong arguments about paradigms or hegemony.

14. Norbert Wiley, "The Rise and Fall of Dominating Theories in American Sociology," in *Contemporary Issues in Theory and Research*, ed. William E. Snizek, Ellsworth R. Fuhrman, and Michael K. Miller (Westport, Conn.: Greenwood, 1979), 47–83; and Patricia Madoo Lengermann, "The Founding of the *American Sociological Review*: The Anatomy of a Rebellion," *American Sociological Review* 44, no. 2 (April 1979): 185–98.
15. Edward Shils, "The Calling of Sociology," in *Theories of Society: Foundations of Modern Sociological Theory*, ed. Talcott Parsons et al. (New York: Free Press, 1961), 1405, 1410.
16. Pierre Bourdieu, "The Specificity of the Scientific Field," in *French Sociology: Rupture and Renewal Since 1968*, ed. Charles C. Lemert (New York: Columbia University Press, 1981), 257–92.

Instead, a certain multivocality of discourse, perception, and practice is one of the very preconditions for the organization of power differentials within any field. All social fields operate via universally acknowledged definitions of distinction and scientific capital. Without some intellectual diversity, such as disagreements on epistemology, methodology, and theory, there would be no raw materials that strategies of domination could sink their claws into and wield as weapons of differentiation or distinction. Indeed, it is likely that new differences will be invented even in fields in which old inequalities have been eradicated. The opposite of a well-structured field is not a more heterogeneous one but rather a congeries of practices that are not governed by any one definition of symbolic capital. This means that a historical sociology of sociology cannot demonstrate the existence of a genuinely open state of affairs simply by pointing to divisions, conflicts, or dissidents such as Mills after 1945. The fact that there were as many opponents as supporters of the "natural science" approach among presidents of the American Sociological Society/American Sociological Association does not tell us much about the field's internal power structure.

The situation in sociology before 1945 was not just internally "diverse." This was also a period in which vigorous criticism by respected intellectuals outside the field, such as John Dewey, was directed against methodological positivism. Friedrich von Hayek warned against the dangers of "scientism," which he summarized as a "slavish imitation of the method and language of Science."[17] Even the Sociological Research Association—an invitation-only professional group formed in 1936 in response to battles within the American Sociological Society—was divided between "value-neutral" positivists and more interpretivist types.[18] The leading departments in this period—those that produced and exchanged the most PhD's and were ranked highest—were epistemologically divided.[19] Sociology's lack of "fieldness" before 1945 is also revealed by the contents of the *American Sociological Review*, where the representation of theoretical articles

17. Friedrich A. von Hayek, "Scientism and the Study of Society (Part 1)," *Economica* 9, no. 35 (August 1942): 268.

18. Richard Evans, "Sociological Journals and the 'Decline' of Chicago Sociology: 1929–1945," *History of Sociology* 6–7 (1986–87): 123; Bannister, *Sociology and Scientism*, 189, 218.

19. These departments were at Columbia University, the University of Chicago, the University of Wisconsin, the University of Michigan, Harvard University, the University of North Carolina–Chapel Hill, and (before 1945) the University of Minnesota. I discuss these departments and Berkeley's new sociology department, which was created after World War II, in "American Sociology."

declined only gradually and articles appeared on topics such as psychoanalysis, "imagination in social science," Lenin's theory of revolution, and anthropological theories of culture.[20]

Widely read texts were epistemically ambivalent or multivocal. Parsons rejected "positivism," but at the same time he insisted that sociology's goal should be the discovery of "analytical laws" that state "a *uniform* mode of relationship between the values of two or more analytical elements."[21] Parsons's ego ideal for sociology was economics (and after the war, physics).[22] Lynd's *Knowledge for What?* criticized positivism and scientism but at the same time rejected "theory." Pitirim Sorokin's *Social and Cultural Dynamics* (1937–1941) rejected empiricism but viewed societies as progressing through a predictable inner logic of cultural development. But Sorokin went against the grain in more ways than one; his 1956 book *Fads and Foibles in Modern Sociology and Related Sciences* was one of the first full-scale attacks on the postwar positivist settlement, written several years before Mills's more famous assault on "abstracted empiricism" in *The Sociological Imagination*.

Another sign of sociology's epistemically labile state during the 1930s was its relationship to Freud. Although psychoanalytic theory was always open to biologizing and repressive interpretations, it posed a challenge to sociology's empiricism and behaviorist aculturalism. Founding American sociologist Charles Horton Cooley had already argued at the first meeting of the American Sociological Society in 1906 that the "social mind" had to be seen as encompassing an *unconscious* dimension. The *American Journal of Sociology* published a special issue on psychoanalysis and sociology in the year of Freud's death, 1939, with essays by leading analysts and literary critics. One contributor to this issue remarked that "sociology is sufficiently mature to adopt the methods of contemporary psychological science," adding that the "phenomenon which Freud called the return of

20. Patricia Wilner, "The Main Drift of Sociology between 1936 and 1982," *History of Sociology* 5, no. 2 (Spring 1985): 16, table 10.

21. Parsons, *The Structure of Social Action*, 2:622, my emphasis. Despite his emphasis on values or norms, Parsons also fell into an objectivist analysis of the subjective, reducing it to the single category of the means-ends schema; see Charles Camic, "*Structure* After 50 Years: The Anatomy of a Charter," *American Journal of Sociology* 95, no. 1 (July 1989): esp. 64–69.

22. See Talcott Parsons, Robert F. Bales, and Edward A. Shils, *Working Papers in the Theory of Action* (Glencoe, Ill.: Free Press, 1953); see also Jennifer Platt, *A History of Sociological Research Methods in America: 1920–1960* (Cambridge: Cambridge University Press, 1996), 102.

the repressed" was "of particular importance to sociology."[23] Needless to say, the concept of the return of the repressed did not play a central role in postwar sociology. Parsons had opened his *Structure of Social Action* (1937) by asking why the "positivistic-utilitarian tradition" had *died*, but it was this imperfectly repressed tradition that returned with a vengeance after the war.

Where prewar sociology had been distinctively polyvocal, this splintered situation had disappeared by the 1950s. Methodological positivism was a form of scientific capital that was increasingly *recognized* by its opponents, however much they disliked it. The shift to a more ordered field was established in several ways. Explicit methodological positivism now prevailed in the sociology journals, in the most widely used textbooks and introductions to theory and method, in the personnel of the leading departments, and in the relevant funding agencies. In addition to the continuing efforts of the prewar camp, an entirely new cast of characters entered the discipline from wartime and government agencies, and from backgrounds in private industry, advertising agencies, and survey institutes.[24] U.S. sociology's own view of itself in this period followed a narrative of steady progress from social meliorist beginnings toward scientific maturity. The main historical treatment of the field from the 1950s described the discipline as becoming ever more focused on "scientific method," which was identified with the quest to discover laws of behavior and a "preference for concrete, empirical work."[25]

Paul Lazarsfeld's Bureau of Applied Social Research at Columbia University was an exemplary center of methodological expertise pursuing an explicitly empiricist and positivist style of sociology. According to James Coleman, C. Wright Mills "seemed to matter little" in the local "social system of sociology" at Columbia in the 1950s, or "mattered only to those who themselves seemed to matter little." This provides a pungent sense of the marginalization of one of the most creative U.S. sociologists of the twenti-

23. Gregory Zilboorg, "Sociology and the Psychoanalytic Method," *American Journal of Sociology* 45, no. 3 (November 1939): 341.

24. See Stephen Turner and Jonathan Turner, *The Impossible Science: An Institutional Analysis of American Sociology* (Beverly Hills, Calif., and London: Sage, 1990), chap. 3, on this influx. One autobiographical account of the transformation of a chemist at Eastman Kodak into a sociologist who carried his self-described "positivist orientation . . . from the physical sciences" into the new field is given by J. S. Coleman, "Columbia in the 1950s," in *Authors of Their Own Lives: Intellectual Autobiographies by Twenty American Sociologists*, ed. Bennett M. Berger (Berkeley: University of California Press, 1999), 75, 93.

25. Roscoe C. Hinkle and Gisela J. Hinkle, *The Development of Modern Sociology, Its Nature and Growth in the United States* (New York: Random House, 1954).

eth century.[26] Lazarsfeld and Morris Rosenberg's extremely influential *Language of Social Research* (1955), with its title redolent of the Vienna Circle, concluded with a long section on the "philosophy of the social sciences" that was based exclusively on the deductive-nomothetic approach, a child of logical positivism. Harry Alpert, who held a PhD in sociology from Columbia, went to the newly created National Science Foundation, where he organized the conditions under which sociologists could attain federal funds. These conditions included "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"; belief in the "convergence of the natural sciences and the social sciences"; and attention to the "national interest." Sociologists were expected to draw predictive and practical lessons from their research, while "value neutrality" was to remain blind in one eye.[27]

Sociology departments at the other leading universities also came to be dominated by methodological positivism, although the timing and modalities of this shift differed from case to case. At the University of Chicago, representatives of the new paradigm were not solidly in control of a highly factionalized department until the mid-1950s; by that time, it had become "scientific, modern, positivist," and "mainstream."[28] A 1958 report by chairman Phil Hauser spoke of "the complete disappearance of the earlier bipolar division of departmental interests."[29] William Sewell Sr. "was instrumental in building the University of Wisconsin's 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," according to his son, historian William Sewell Jr.[30] Ironically, Max Horkheimer and Theodor Adorno returned to Frankfurt with the Institute for Social Research (Institut für Sozialforschung) in the same year that the identically named positivist Institute for Social Research was founded at the University of Michigan, growing out of a previously established Survey Research Center. By the early 1950s, Michi-

26. Coleman, "Columbia in the 1950s," 77.

27. Harry Alpert, "The Social Sciences and the National Science Foundation," *American Sociological Review* 20, no. 5 (December 1955): 656, 660.

28. Gary Alan Fine, "Introduction," in *A Second Chicago School? The Development of a Postwar American Sociology* (Chicago: University of Chicago Press, 1995), 9; see also Andrew Abbott, *Department and Discipline: Chicago Sociology at One Hundred* (Chicago: University of Chicago Press, 1999).

29. Quoted in Abbott, *Department and Discipline*, 59.

30. William Sewell Jr., "The Political Unconscious of Social and Cultural History, or, Confessions of a Former Quantitative Historian," in *The Politics of Method*.

gan's sociology department was dominated by adherents of the ascendant approach. The 1956 Michigan textbook, *Principles of Sociology*, which defined sociology's objects as "subject to study by the same methods as other natural phenomena," replaced an earlier, much less "scientific" textbook by Robert Cooley Angell and Lowell Juilliard Carr based on the writings of Charles Horton Cooley.[31]

The leading department after 1945, at Harvard University, presents a slightly more complex picture. The common view is that "the occupational roles of theoretician and bureaucratized research worker became entirely distinct" after 1945, and the former refers to Parsons.[32] But this "distinction" was a conscious division of labor rooted in deeper agreements about basic principles. The struggle between statistical "operationalism" and theoretical "functionalism" in the postwar period did not trouble this more fundamental consensus about what counted as scientific capital. Parsons also moved toward more scientistic formulations in his own writing during this period. His supposed "lack of methodological commitments" did not mean that his influence had no consequences for method, if we define *method* broadly in terms of the classical Greek *méthodos*, originally meaning "following after, pursuit," and in philosophical contexts referring to the "pursuit of knowledge, investigation," and, by further extension, to "a plan or strategy for carrying out an investigation."[33] By 1964, Parsons had been replaced by a Harvard colleague, George Homans, as the most-cited sociologist in the United States. Homans invoked Ernst Mach in insisting that science consists of the "careful and complete description of the mere facts" while avoiding "why" questions and "grand theory."[34]

Three arguments have been mobilized against the thesis of postwar

31. According to the 1956 text, sociology's aim was to "discover systematic . . . observable relationships between . . . phenomena." The discipline was "nonethical," although its findings could be used in "instrumental" ways. Ronald Freedman et al., *Principles of Sociology* (NewYork: Holt, 1956), 5, 6, 12. Compare Charles Horton Cooley, Robert Cooley Angell, and Lowell Juilliard Carr, *Introductory Sociology* (New York: Charles Scribner's Sons, 1933).

32. Kucklick, "A 'Scientific Revolution,'" 16.

33. Quotes from Platt, *A History of Sociological Research Methods in America*, 202–3; and William H. Baxter, "Where Does the 'Comparative Method' Come from?" in *The Linguist's Linguist: A Collection of Papers in Honour of Alexis Manaster Ramer*, ed. Fabrice Cavoto, vol. 1 (Munich: LINCOM), 42–43.

34. James J. Chriss, "Testing Gouldner's Coming Crisis Thesis: On the Waxing and Waning of Intellectual Influence," *Current Perspectives in Social Theory* 15, no. 3 (1995): 3–61; George C. Homans, "A Conceptual Scheme for the Study of Social Organization," *American Sociological Review* 12, no. 1 (February 1947): 14.

dominance of positivism in U.S. sociology. Some point out, quite correctly, that sociologists interact with people and ideas from other disciplines and are hardly restricted to sociology departments. This no more challenges the fieldlike character of sociology than does the presence of dominated dissidents. Moreover, sociology was shaped by an entire array of "outside" influences, including all of those discussed by "social epochal" approaches to the sociology of knowledge, such as Marxism. But this does not mean that the external determinants were part of the sociology field proper, in the Bourdieuian sense. Fields are located within other fields, like Russian dolls. But sociology departments and the discipline as a whole were to some extent autonomous, with their own internal definitions of capital. Sociologists had their own professional associations, annual meetings, job markets, and journals, and some shared culture. All sociologists recognized, for example, what another sociologist meant when praising an argument or model as "elegant" or dismissing another as "journalism."

A second argument can also be quickly rejected. This concerns the lack of explicit reference to positivism by those associated with the tendencies I am calling positivist. American sociologists at the time recognized this object. In addition to explicit discussions in the main journals and presidential addresses to the professional conventions by people such as George Lundberg, the widely used 1944 *Dictionary of Sociology* provided a concise definition of the term.[35] Sociology students at Columbia took seminars that were jointly taught by Lazarsfeld and Nagel.[36] Although Raymond Williams remarked in 1983 that positivism had become "a swear-word, by which nobody is swearing," he also acknowledged that "the real argument is still there." Moreover, these pejorative connotations were only weakly developed in the 1940s and 1950s. Another rejoinder is that unnamed and unperceived structures exist and influence empirical events. Even though the term *positivism* was largely replaced by substitutes such as "nomothetic-deductive" or simply the "philosophy of science," or by doctrines such as Popperian "falsificationism," the basic assumptions remained intact.

A third argument is that there is a lack of fit between positivist philosophical doctrine and sociological research practices.[37] Some assert that "the simple fact is that almost no theories of this kind [positivist ones] were

35. Henry Pratt Fairchild, *Dictionary of Sociology* (New York: Philosophical Library, 1944), 226.
36. See Coleman, "Columbia in the 1950s," 88.
37. See Platt, *A History of Sociological Research Methods in America*, for this argument.

ever tried in sociology."[38] But this assumes that the only relevant definition of positivism is to be found in some philosophical urtext. One distinction that is sometimes made contrasts inductivism and deductivism: positivist positions in the twentieth century were deductivist, but in mainstream sociology, it was standard practice to throw data into a statistical procedure (or later into a computer program) and then to invent ad hoc theoretical explanations for any empirical relationships that could be discerned. But the patron saint of scientists' spontaneous positivism is Hume, for whom inductively discovered regularities were the *sole* source of knowledge.[39]

Direct critiques of methodological positivism within sociology were almost imperceptible in the 1950s, aside from Mills and Sorokin. Most sociologists whose work diverged epistemically from the dominant model simply carried on.[40] Alvin Gouldner inched up to his full-blown epistemological critique of U.S. sociology only gradually after the 1950s, and in 1954 he quoted Homans to the effect that "sociology may miss a great deal if it tries to be too quantitative *too soon*," implying that quantification was the ultimate telos.[41] The antipositivism of Hans Gerth at the University of Wisconsin had to be gleaned from his decisions about what to translate and from his teaching, since he published so little.[42] Adorno's writings from the early 1960s were the most sophisticated critiques of positivism in this entire period, but they were not translated into English until 1976. Marcuse worked as an intelligence analyst for the U.S. Army during the war and headed up the Central European Section of the Office of Intelligence Research afterwards, and when he finally returned to teaching in 1951, it was in philosophy rather than sociology. Some earlier critics toned down or subtly adjusted their former antipositivism in ways that made their work more compatible with the reigning framework. Sorokin turned sharply against what he called "sham-

38. Nicholas C. Mullins and Carolyn J. Mullins, *Theories and Theory Groups in Contemporary American Sociology* (New York: Harper and Row, 1973), 218.
39. On this, see the work of Roy Bhaskar, especially *The Possibility of Naturalism* (New York: Humanities Press, 1979).
40. I am thinking of sociologists such as Leo Lowenthal, Harold Blumer, Erving Goffman, Reinhard Bendix, and Barrington Moore Jr.
41. Alvin W. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954), 17, my emphasis.
42. But see Gerth, "The Relevance of History to the Sociological Ethos," *Studies on the Left: A Journal of Research, Social Theory, and Review* 1, no. 1 (Fall 1959): 7–14. See the bibliography of Gerth's work in *Politics, Character, and Culture: Perspectives from Hans Gerth*, ed. Joseph Bensman, Arthur J. Vidich, and Nobuko Gerth (Westport, Conn.: Greenwood Press, 1982), 275–83.

scientific slang," "the cult of numerology," "pseudo experimentation," social "atomism," and sociological "simulacra" of the natural sciences,[43] but he did not break with doctrines of predictability or uniform social laws. Even Mills, whose 1959 critique of U.S. sociology in *The Sociological Imagination* is often seen as a heroic cri de coeur from the scientistic wilderness, had drifted slowly toward the epistemological mainstream. Whereas in the 1940s Mills had criticized the "low level of abstraction" in sociology textbooks and bemoaned the fact that U.S. sociology had "lost touch with the grand tradition" of European sociology, in 1959 he cast aspersions not only on "abstracted empiricism" but on "grand theory" as well.[44]

2. Fordism and the Spontaneous Social Epistemology of Sociologists

What accounts for the postwar consolidation of the sociological field? We cannot explain this change in terms of the turnover of generations or the "maturation" of a science. Another explanation that has been offered is that new sciences tend to emulate the currently most prestigious discipline. The problem here is that other newcomer disciplines that emerged at the end of the nineteenth century, most notably anthropology, did not follow the same epistemic path as sociology. Moreover, if we place sociology in a higher-order metafield of the sciences, it is not obvious that sociologists would necessarily experience "physics envy" as opposed to, say, "philosophy envy."

Bourdieu seems to promise some level of generality with respect to these questions by arguing that the most distinguished positions in any given field will be those which exhibit the greatest "distance from necessity." In the arts, this points toward more abstract or minimalist forms, or to a style of viewing that privileges form over content, as more distinguished. In

43. 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." See *A Long Journey: The Autobiography of Pitirim A. Sorokin* (New Haven, Conn.: College and University Press, 1963), 324. This lent ammunition to those who dismissed him as increasingly "shrill," "eccentric," and motivated by personal bitterness. Lewis A. Coser, *Masters of Sociological Thought* (New York: Harcourt Brace Jovanovich, 1971), 508.

44. C. Wright Mills, "The Professional Ideology of Social Pathologists," *American Journal of Sociology* 49, no. 2 (September 1943): 166; and "International Relations and Sociology: Discussion," *American Sociological Review* 13, no. 3 (June 1948): 271.

sports, distance from necessity might accrue to bodily movements that are more artificial and more clearly distinct from natural or everyday gestures, or to games that are wasteful of time (e.g., sailing) and space (e.g., golf). But how could Bourdieu's theory account for the prestige of positivism in sociology? While empiricism is certainly more *concrete* than depth realism, and thus in certain respects closer to necessity, positivism can be construed as *less* concerned with the messy details of the specific individual case, which it tries to subsume under covering laws. Quantification might be described as a substitute for the cruder "classification of events which our senses provide," in von Hayek's words.[45] Although "distance from necessity" need not be defined in aesthetic terms, it is remarkable that positivists in the social sciences spontaneously adhere to an aestheticizing language of "elegance" when describing statistical models.

The problem with this approach is its ad hoc and portmanteau quality. One could just as easily tell a story according to which nonpositivist approaches are *more* disdainful of necessity than positivist ones and hence *more* distinguished. It could be emphasized that the parsimonious accounts preferred by positivism are actually closer to *commonsense* understandings of social causality. Bourdieu's framework is best equipped to account for the workings of consolidated, settled fields, but its analyses of the origins or the substantive contents of any given settlement are less compelling. Bourdieu indicates how practices and perceptions are likely to be framed, the general kinds of arguments that will be mobilized by anyone trying to control a field, but he cannot explain why certain projects of distinction will be more successful than others, without falling back into an economism that he wants to avoid.

So why was sociological positivism so successful in its bid for leadership and prestige after 1945? The most significant and all-encompassing development after the war was the consolidation of the social patterns that retrospectively have come to be known as Fordism. The Fordist "security state" relied to a greater extent than previous state forms on the skills of social scientists, and this entailed an enhanced level of public funding for sociological research. By the 1950s and early 1960s, the intellectual, financial, and political forces that had existed before 1945 were combining with the ideological effects of the newly consolidated Fordist mode of regulation to catapult the methodological positivists to victory.[46]

45. Von Hayek, "Scientism and the Study of Society (Part 1)," 275.

46. I cannot address other elements of the overdetermining conjuncture that led to the

3. The Resonance of Fordism with the Positivist Vision of the Social

Regulation theory has described Fordism as an integral form of societalization, that is, as a temporarily stabilized system for producing and reproducing (capitalist) society in spite of its "conflictual and contradictory character."[47] Fordism was geared specifically toward maintaining a certain ratio between capital accumulation and consumption. Because this particular combination of mass production and consumption was pioneered by Henry Ford, his name became associated with the more general social form, which was not, however, limited to the economic level. Fordism also entailed Keynesian-style policies aimed at smoothing economic cycles; welfare-statist programs that partially socialized the costs of reproducing labor power; mass culture, which offered a homogeneous fare and eroded distinctions between highbrow and lowbrow culture; a family form organized around the male single breadwinner and located within privatized single-family housing, at least as a sociocultural ideal; and a tendency for economic and social practices to be located within the "container" of the nation-state and evenly distributed across its surface. Each specific "national mode of growth" (Bob Jessop) emphasized some of the elements from this menu of Fordist practices and omitted others.

The social patterns grouped by regulation theory under the heading of Fordism helped to make positivist approaches to social explanation more plausible on a mass level, both to sociologists and to others exposed to the new logics governing activity in the advanced capitalist world. By contrast, positivism had been experienced as a *less* plausible description of the crisis-ridden conditions of the interwar period. After the war, social reality appeared increasingly to conform to the positivist expectation that social practices could be subsumed under covering laws, falling into patterns that were the same everywhere and always. Social actors now seemed atomized, rational, and interchangeable, lacking any distinctive cultural peculiarities; social practice was more predictable, repetitive, and controllable.

The effects of postwar Fordism on the enhanced plausibility of so-

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success of positivism, one of which was the heightened level of anxiety around totalitarian irrationalism.

47. Alain Lipietz, "Akkumulation, Krisen und Auswege aus der Krise: Einige methodische Überlegungen zum Begriff 'Regulation,'" *Probleme des Klassenkampfes* 15, no 1 (1985): 109.

ciological positivism can be summarized under the following five broad categories.

Science and the Fordist State

Sociological positivism was buttressed first by the greatly enhanced role of science (including social science) in the Fordist form of governmentality. While significant private foundation funds were offered previously to sociologists willing to configure their work in the image of the natural sciences, the postwar period saw an unprecedented influx of new government resources to universities, think tanks, and individual researchers. In addition to military support for research on psychological warfare, which channeled money to social scientists involved in programs such as Project Troy, Project Camelot, and the Special Operations Research Office,[48] the domestic Keynesian branches of the security state drew on social science to track the economy, regulate business cycles, survey the population, and bring social practices into line with the serialized rhythms and desires of mass production and mass consumption. The National Science Foundation (NSF), created in 1950, was the single largest nonmilitary source of funding for sociological research (although the Defense Department offered twice the amount of the NSF to social scientists in 1965), and, because of its orientation toward "pure science," it was more immediately relevant than "think tank" research for defining scientific capital. Parsons insisted that "the same philosophical principles that guided the natural sciences were at the heart of the social sciences" in arguing for an expansion of the NSF mandate to encompass the social sciences, agreeing with operationalists such as Phil Hauser.[49] Along with the availability of funds from the Department of Health, Education, and Welfare, the National Institutes of Health, the National Institutes of Mental Health, the State Department, and an indeterminable thicket of initiatives funded by the Department of Defense, this NSF funding greatly enhanced the resources for positivist forms of sociology. In 1958, government at all levels contributed about 27 percent of all

48. On Projects Troy and Camelot, the Special Operations Research Office, the RAND Corporation, and other militarily funded social science during the Cold War, see the essays in Irving Louis Horowitz, ed., *The Rise and Fall of Project Camelot* (Cambridge, Mass.: MIT Press, 1967); and Christopher Simpson, ed., *Universities and Empire: Money and Politics in the Social Sciences during the Cold War* (New York: New Press, 1998).
49. Samuel Z. Klausner and Victor D. Lidz, "Introduction," in *The Nationalization of the Social Sciences* (Philadelphia: University of Pennsylvania Press, 1986), vii.

social science funds; by 1976, the rate was around 56 percent.[50] In 1966, the federal government spent $26,621,000 on "basic and applied sociological research" alone, and the Department of Defense spent $34 million on behavioral and social science research; by 1970, the Department of Defense was spending almost $50 million.[51] Before 1930, only 1.3 percent of articles in the *American Journal of Sociology* acknowledged financial support from any outside source at all, but by the 1960–64 period, the percentage of articles acknowledging financial support in the *American Journal of Sociology*, the *American Sociology Review*, and *Social Forces* had risen to 52.5 percent.[52] The proportion of total revenues at Lazarsfeld's Bureau of Applied Social Research coming from government sources rose from 0 to 91 percent between 1945 and 1974.[53] Methodological positivists were being offered unprecedented resources and allies in their intradisciplinary struggle, from the 1940s through the mid-1970s.

The integration of sociology into the Fordist scientific infrastructure seemed paradoxically to validate the claim that science was "value-neutral": social scientists could now conceive of themselves as a separate and autarkic "scientific community," after having freed themselves from dependence on the corporate funding that dominated interwar science.[54] As autonomous professionals, social scientists felt released from responsibility for the ways policy makers would use their research.[55] State Fordism thus supported the fact/value dichotomy, a mainstay of methodological positivism. By the 1950s or 1960s, social policies whose original historical roots lay in socialist, corporatist, or religious traditions were being implemented by "postideological"

50. Michael Useem, "State Production of Social Knowledge: Patterns of Government Financing of Academic Social Research," *American Sociological Review* 41, no. 4 (August 1976): 613–29; Harry Alpert, "The Funding of Social Science Research," in *Trends in Social Science*, ed. Donald P. Ray (New York: Philosophical Library, 1961), 152–66.
51. James L. McCartney, "The Financing of Sociological Research: Trends and Consequences," in *The Phenomenon of Sociology*, ed. Edward A. Tiryakian (New York: Appleton-Century-Crofts, 1971), 385; and Ellen Herman, "Project Camelot and the Career of Cold War Psychology," in *Universities and Empire*, 126n22.
52. McCartney, "The Financing of Sociological Research," 387–88.
53. Allen Barton, "Paul Lazarsfeld and the Invention of the University Institute for Applied Social Research," in *Organizing for Social Research*, ed. Burkart Holzner and Jiri Nehnevajsa (Cambridge, Mass.: Schenkman Publishing Co., 1982), 28–29.
54. Philip Mirowski, "How Positivism Made a Pact with the Postwar Social Sciences in America," in *The Politics of Method*.
55. Freedman et al., *Principles of Sociology*, 12; also Herman, "Project Camelot."

governments, and sociologists were becoming the organic intellectuals of this burgeoning sector.

Fordist Economy

Another way in which Fordism contributed to positivism's plausibility was by dampening economic turbulence and crisis, through fiscal policy, and by lessening some of the economic upheavals in the individual life course through wage and welfare state policies. These developments, which particularly affected the middle wage-earning classes (including many sociologists), made it seem more conceivable that social practices did, in fact, repeat themselves in ways that could be represented by covering laws, statistical models, and replicable experiments. Steady improvements in the standard of living and the thickening of the welfare-statist safety net lent credence to the idea of social regularities. The relatively generous protections against the risks of unemployment, sickness, and poverty in old age allowed the better-paid sectors of the working and white-collar classes, for the first time in capitalist history, to develop a horizon of stable expectations for the future. Increases in real wages were pegged to increasing productivity. The social ontology of the Fordist subject was aligned with *security*. Historical analysis became less significant for sociology now that the world seemed to have become "synchronic." Long before Francis Fukuyama, social scientists were urged to "go beyond history."[56]

Culture and the Fordist Ideoscape

A *culture* and an *ideoscape* that was increasingly replicated across regions and social groups began to make models of economic and developmental *modernization* seem more convincing, predicated as they were on a picture of universal, interchangeable subjectivity. Arjun Appadurai's notion of an ideoscape suggests that ideologies will be understood differently depending on one's position in social space, just as a landscape looks entirely different depending on the viewer's spatial location. Thus, some social scientists resisted the turn to methodological positivism even during the 1950s, while others were positioned to profit from the new perspective. But even those who initially resisted the new course were more likely to gravitate toward it because it resonated increasingly with their own experience of the sociotechnical world. The *depthless* culture of Fordism seemed to substan-

56. Bernard Brodie, "Strategy as a Science," *World Politics* 1, no. 4 (July 1949): 474.

tiate behaviorist and empiricist models of subjectivity. Social-psychological experiments that drew conclusions about human behavior from research on American college students began to seem less far-fetched. The positivist impetus to abandon all analysis of subjective meaning resonated with a wider culture that really did seem to have transcended ideology. The post-ideological character of the American citizen was demonstrated in numerous sociological studies of voting. Interpretations of culture in the sense of a deep structure (Claude Lévi-Strauss) or the unconscious were rare.[57]

Fordist Geospace

The refusal of hermeneutic or cultural interpretation was paired with a denial of the importance of *spatial* or geographic difference. Two aspects of the Fordist manner of configuring social space contributed to this change in social epistemology. First, the concentration of economic development and transactions on the scalar level of the nation-state and the relative evening out of regional developmental differences encouraged social scientists to take this level for granted as their unit of analysis.[58] The containment of most practices within the boundaries of the nation-state made it seem more self-evident that they could be described by general laws. By contrast, where practices are objectively more dispersed among multiple and shifting sites and scalar levels, as in present-day post-Fordist "globalization," they confound simple dichotomies of local and national. Countries no longer appear to be the natural units of comparative analysis.

Fordism's relative homogenization of domestic space also underwrote the positivist disavowal of the cross-cultural variability of concepts and causal mechanisms. The tendency to seek universal laws of human behavior, to disavow cultural difference, was opposed to the humanist and historical-hermeneutic emphasis on the unique and idiosyncratic. For behaviorists in the 1950s, including those social scientists who adopted a sci-

57. According to the new division of academic labor, anthropologists were to focus on (post)colonial peoples, while sociologists would attend to the metropoles, and, after the 1940s, only the former were seen as having "culture" in the anthropological sense. See my "Culture and the State," in *State/Culture: Historical Studies of the State in the Social Sciences*, ed. George Steinmetz (Ithaca, N.Y.: Cornell University Press, 1999), 1–49.

58. On Fordism and space, see David Harvey, *The Condition of Postmodernity: An Enquiry into the Origins of Cultural Change* (New York: Basil Blackwell, 1989); and Neil Brenner, "Between Fixity and Motion: Accumulation, Territorial Organization, and the Historical Geography of Spatial Scales," *Environment and Planning D: Society and Space* 16 (1998): 459–81.

entistic version of psychoanalysis, there were no salient differences between the United States and the rest of the world. Modernization theory treated the poorer zones simply as less-developed versions of the United States, and "foreign nationals" were regarded as "underdeveloped Americans."[59] Sometimes it was impossible to overlook the fact that "culture and ideology" were impeding "modernization," as in Vietnam, China, the socialist bloc, and parts of postcolonial Africa. But the eventual telos for all nations was identical: a condition exemplified by the United States, located beyond ideology and culture, in which social action could be distilled down to the interactions among rational individuals pursuing wealth, status, and power. Alex Inkeles, a leading sociological modernization theorist, insisted that there "cannot be one social science for the study of one's own country and a different one for the study of other nations."[60]

Fordist Imperialism

Of course, the collapse of Fordism was not the only reason for the erosion of faith in the universal applicability of U.S. social science categories. A final structural characteristic of the postwar period that was closely linked to the fate of methodological positivism in sociology was the emerging role of the United States as imperialist global hegemon. U.S. imperialism cannot be equated with Fordism, even if it was entwined with it in this epoch; indeed, we are currently in the midst of a rearticulation of U.S. imperialism with *post-Fordist* neoliberalism.[61] Two aspects of the cold war global configuration tended to work *in favor* of sociological positivism: the U.S. orientation toward an "imperialism of free trade" rather than colonialism, and its emphasis on counterinsurgency research, spawned by the rivalry with the Soviet Union.

59. Edward Twitchell Hall, *The Silent Language* (Garden City, N.Y.: Doubleday, 1959), 13.
60. Alex Inkeles, "Understanding a Foreign Society: A Sociologist's View," *World Politics* 3, no. 3 (April 1951): 269.
61. See Naomi Klein, "Baghdad Year Zero: Pillaging Iraq in Pursuit of a Neocon Utopia," in *Harper's* 309, no. 1852 (September 2004): 43–53; see also my "The State of Emergency and the Revival of American Imperialism: Toward an Authoritarian Post-Fordism," *Public Culture* 15, no. 2 (Spring 2003): 323–45. The founder of regulation theory, Michel Aglietta, insisted that "it should never be forgotten . . . that the rise of the United States to world hegemony forms an integral part of the social transformations" of Fordism, but he then proceeded to bracket this global aspect entirely; see *A Theory of Capitalist Regulation: The U.S. Experience*, trans. David Fernbach (London: New Left Books, 1979), 32. Alain Lipietz discussed only the economic aspects of "global" and "peripheral" Fordism in *Mirages and Miracles: The Crisis in Global Fordism* (London: Verso, 1987).

U.S. imperial hegemony after World War II, like today, was oriented mainly toward making the world into an "open" capitalist marketplace. The United States increasingly struck an imperialist but *anticolonial* stance. This reflected the shift in priorities toward anticommunism and "containment," and a preference for free trade as against the more protectionist type of relationship that prevailed between metropoles and their colonies. Because the United States generally eschewed direct colonization, it was not compelled to enforce a formal "rule of difference" (Partha Chatterjee) in its peripheral dependencies, which were treated as self-governing. Unlike colonial powers, for whom, in Jean-Paul Sartre's words, assimilation "taken to its extreme meant, quite simply, the ending of colonialism," U.S. imperialism sought convergence between peripheral polities, cultures, and economic approaches and the American model.[62] Without gainsaying the exploitative, manipulative, and violent character of U.S. interventions, we cannot ignore the fact that the postwar United States had fewer opportunities than European colonial powers to intrude racist views of "native" life directly into the global peripheries. Postcolonials were increasingly treated as a mass of interchangeable potential customers, as junior Americans in the making; social scientists were enlisted in promoting this transformation. The organization of core-periphery relations in the period from the 1880s to 1945 had encouraged the development of separate theories and even separate sciences for the colonized (anthropology) and the colonizer (sociology). The U.S.-dominated postwar global regime reinforced the positivist program of seeking a single model for "other nations" and "one's own country."

The pressure to develop a generalizing approach to the non-West also stemmed from national security interests. Faced suddenly with the problem of managing a world of former European colonies and the "presumed imperial ambitions of the Soviet Union," a core substantive focus of social science research was counterinsurgency.[63] Walt Whitman Rostow's *The Stages of Economic Growth* explicitly drew lessons from U.S. economic development for "the men in Djakarta, Rangoon, New Delhi, and Karachi; the men in Tehran, Baghdad, and Cairo; the men south of the desert too, in Accra, Lagos, and Salisbury," as well as Mexico, China, and India.[64] As in

62. Jean-Paul Sartre, "Colonialism Is a System (1956)," in *Colonialism and Neocolonialism* (London: Routledge, 2001), 46.
63. Carl Pletsch, "The Three Worlds, or the Division of Social Scientific Labor, circa 1950–1975," *Comparative Studies in Society and History* 23, no. 4 (October 1981): 568.
64. Walt Whitman Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960), 166, xii.

U.S.-occupied Iraq in 2003 and 2004, universal models, rather than models tailored to an "Arab mentality," were generally preferred.

Other aspects of the U.S.-dominated postwar world order were *corrosive* of the positivist unity of science program. Once the periphery is regarded from the standpoint of *production* rather than trade, for example, it immediately takes on a very different appearance. First, Fordism was just one of the many forms of societalization, and usually a minor one, in the global South. The social complexity that resulted from the articulation of differing "modes of regulation" and "modes of production" in the peripheries seemed to defy generalizing social science laws. The somewhat disparaged essay entitled "The Basic Concepts of Historical Materialism" at the end of Louis Althusser and Étienne Balibar's *Reading Capital* (1968) may have proposed a universal classification of "elements of the structure," but here, and in *For Marx*, Althusser explicitly rejected uniform models of social development. The real differences between the (post)colonies and their former metropoles made the application of universal models to peripheral societies much less compelling for those social scientists who actually lived in (or studied in detail) the so-called Third World. The complex forms of cultural hybridity in postcolonial sites made generalization even more difficult, whether we are talking about resistance to Western-style "development" or the problems of converting labor power into labor. But among the shrinking numbers of sociologists who attended to the world outside the United States, many did so from the vantage point of Berkeley or Ann Arbor, where they were unlikely to be confronted with these shocks to their homegrown epistemologies.

Generalizing approaches were also undercut by the pervasive "three worlds" model, which was predicated on the division between the communist, advanced capitalist, and postcolonial sectors of the world system. This mental map called attention to the instability of the third category, which was associated in this scheme with "tradition, culture, religion, irrationality, underdevelopment, overpopulation, political chaos, and so on," and was permanently susceptible to the lures of communism, which was usually seen as an "ideological" system rather than a "rational-natural" one.[65] Another barrier to the universalization of general models was racism, despite the countervailing impetus of U.S. imperialism to assimilate the foreign Other.[66]

65. Pletsch, "The Three Worlds," 574.

66. Seen most recently in Bush's ridiculous tarring of opponents of the Iraq War as racists for supposedly seeing Arabs as unsuited for democracy. While this was obviously motivated by domestic politics, Bush's "antiracist" stance also reflects the universalism of

A common response to the scientific difficulties of folding the periphery into general developmental schemas has been to fall back on discourses of cultural essentialism. Thus, for example, a team of social scientists sponsored by the army-funded Human Relations Research Office to study Korean and Chinese POWs during the Korean War pointed to the "unchanging behavioral traits of 'orientals'" (e.g., authoritarianism, adaptability) as reasons for communist organizational success. Another group of social scientists insisted during the Vietnam War that the Vietnamese were "peoples of the past," unsuited for modernization, with "no example, tradition, training, or even psychological aptitude for such an achievement."[67]

Thus, even while the American variety of liberal imperialism underwrote a universalizing variant of positivism, other forces located partly in the "Third World" and partly at the junctures and seams between core and periphery undercut this subsumption of the postcolony under general laws. It is no coincidence that the discipline dedicated to studying the periphery—anthropology—was not captured by positivists after 1945, despite equally lavish offers of funding, and that it continued to emphasize relatively ungeneralizable research on specific places and peoples.[68] Social scientists who set out with a universalizing approach to the (post)colonial world sometimes experienced an epistemological change of heart (as did some of the participants in the Camelot Project). Sociological discourse on the non-West was thus polyvocal even during the Fordist period, fluctuating between "nomothetic" approaches and "idiographic" essentialisms that attributed unique and timeless traits to the traditional other.

4. Conclusion

Fordism resonated powerfully with U.S. sociology's positivist unconscious after World War II and promoted positivist social research directly. But to claim that sociologists' spontaneous images and theories of the social were influenced by the wider social structures they inhabited does not mean

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American empire, analyzed trenchantly by Carl Schmitt in *The Nomos of the Earth* (1950), trans. G. L. Ulmen (New York: Telos Press, 2003).

67. Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton, N.J.: Princeton University Press, 2001), 149; Herman Kahn, "Toward a Program for Victory," in *Can We Win in Vietnam?* ed. Frank E. Armbruster et al. (New York: Praeger, 1968), 340–41.

68. Webb Keane, "Estrangement, Intimacy, and the Objects of Anthropology," in *The Politics of Method*.

that these representations accurately *reflected* social reality, any more than the field-specific status of methodological positivism meant that positivists were better sociologists. The Fordist mode of regulation was accompanied by a theater of relatively uniform self-interpretations that guided its subjects' understanding of their own society. The core components of U.S. Fordism were broadcast to the denizens of the Fordist cities and suburbs as the American Way of Life. When positivists pointed out the connections between existing social patterns and their preferred manner of studying society, reality seemed to directly ratify their approach.[69]

One result of this conjuncture after 1945 was the firm implantation of methodological positivism as *doxa* within the field of sociology. Despite differences of taste or viewpoint, most of the players in this field recognized common stakes and agreed on common definitions of distinction. Symbolic or reputational capital, like research funds, tended to accrue to the more positivist positions. "Fluency" in these codes functioned as a field-specific form of scientific prestige. Even those who disagreed with positivist positions tended to collude in their dominance; those who did not adjust to the new regime were often channeled into less rigidly positivist fields or into poorly ranked departments. Sociology had at last become a well-structured field.

By returning to this period, we may begin to understand the ways in which contemporary sociology continues to be haunted at every turn by specters of a seemingly obsolete epistemology. Settlements like the one I have discussed here are more fragile and internally heterogeneous than they appeared at the time. Indeed, the same can be said of all modes of societalization, regulation, or governmentality, including Fordism: they are inherently tenuous and prone to dissolution. But while poststructural theories have been salutary in calling attention to the inherent instability of social life, we should also recognize the existence of temporarily and partially stabilized patterns of social practice. The intensity of agreement on the basic features of social ontology varies both historically and spatially, among "lay" actors as well as professional analysts of those same social actors. Temporarily stabilized patterns are not the positivists' "constant conjunctions of events," however. They are both more elaborately woven and more fragile than is suggested by languages of general theory or probabilistic laws.

69. A further element of U.S. culture in this period that tended to strengthen the positivist position in the social sciences was the discussion of the role of antiscientific sentiment in the rise of Nazism and Stalinism.

Looking to the future, the question is whether the collapse of Fordism and the emergence of post-Fordist conditions that are less stable and less centered on the nation-state will undermine the natural plausibility of methodological positivism. Although this problem cannot be addressed here in any detail, let me briefly sketch some of the arguments favoring change and some of the countervailing tendencies. On the one hand, faith in the idea of "social laws" would seem to be undermined by the more ephemeral character of social relations today and the fragmentation of markets and lifestyles. The increased pressure on individuals to become self-promoting and the collapse of standardized models of the self might be expected to erode sociologists' faith in universal models of subjectivity and to underscore the need to develop greater *interpretive* skills. Similarly, the enhanced importance of *cultural* commodities in the overall mix of capitalist production in the United States, discussed by Fredric Jameson and others, seems to account in part for the burgeoning interest in cultural studies and for the cultural turn in some of the extant social sciences. At the same time, neoliberal capitalism erodes differences to a greater extent than ever before, imposing a more uniform model of development and suggesting implicitly that culture is everywhere the same. The effects of current tendencies of macrosocial regulation for spontaneous social epistemology are, thus, far from uniform.

The other question I asked in this article was why U.S. sociologists have tended to focus so strongly on the United States in a century of ever-increasing international involvement in overseas empire. The substantive reorientation of U.S. sociology from the colonized peripheries to the "metropole" had already started in the early twentieth century,[70] and this contraction likely had as much to do with the social origins of sociologists in that period, who were mainly rural, Protestant, midwestern men, as with the various enticements that were offered to the discipline to become a domestic *Polizeiwissenschaft*. But even when U.S. sociologists turned to the topics of "development" and "modernization" after 1945, they tended to view the rest of the world as a mirror of their own society. This was undoubtedly a reflection of the increasingly imperial character of the United States. Like classical empires, the United States was both expansive and encompassing, but its tolerance for diversity was hindered by the fact that another empire, the USSR, was competing on a global level for the allegiances of the diverse peoples. Social scientists often adopted this imperial standpoint

70. See R. W. Connell, "Why Is Classical Theory Classical?" *American Journal of Sociology* 102, no. 6 (1997): 1511–57.

spontaneously, coding all deviations from the American way of life as variants of tradition that should, and would, eventually crumble.

How might U.S. sociologists' disinterest in and disavowal of the rest of the world be changing? Contemporary U.S. empire is no more inclined toward an *official* racism than was the liberal empire of the postwar period, so that form of differentiating essentialism is unlikely to make inroads into sociology. The neoliberal institutions that have been introduced in postwar, U.S.-occupied Iraq and in other countries subjected to "structural adjustment" policies have universal models of human subjectivity as their premises. At the same time, neoliberal post-Fordism introduces a whole array of social destabilizations and fragmentations in these zones of imperial pressure, as in the imperial "homeland." Peripheral Fordism was certainly less prosperous and less homogenizing than Fordism in the global core, but the change in the peripheries is still marked. Fundamentalism and neotraditionalism are flourishing in part due to the resulting social dissolution. This makes it much more difficult for area specialists simply to overlook political "irrationalism" and cultural incommensurability. Like the social scientists focused on the global North, those who work on the global South are also facing contradictory epistemic pressures. Whether the positivist domination of U.S. sociology will persist in the discipline's third century is still, it appears, an open question.